# H-Diplo | ISSF

**Roundtable**, Volume VI, No. 1 (2013)

A production of H-Diplo with the journals *Security Studies*, *International Security*, *Journal of Strategic Studies*, and the International Studies Association's Security Studies Section (ISSS).

**http://www.h-net.org/~diplo/ISSF | http://www.issforum.org**

H-Diplo/ISSF Editors: **Thomas Maddux and Diane Labrosse**
H-Diplo/ISSF Web and Production Editor: **George Fujii**
Commissioned for H-Diplo by **Thomas Maddux**


Introduction by **Bruce Russett**

---

**Jack S. Levy and William R. Thompson.** ***The Arc of War: Origins, Escalation, and Transformation.*** Chicago: University of Chicago Press, 2011. ISBN: 9780226476285 (cloth, $97.00); 9780226476292 (paper, $27.00).


Published by H-Diplo/ISSF on **16 September 2013**

Stable URL: http://www.h-net.org/~diplo/ISSF/PDF/ISSF-Roundtable-6-1.pdf


## Contents




**Copyright © 2013 H-Net: Humanities and Social Sciences Online**

H-Net permits the redistribution and reprinting of this work for nonprofit, educational purposes, with full and accurate attribution to the author, web location, date of publication, H-Diplo, and H-Net: Humanities & Social Sciences Online. For any other proposed use, contact the H-Diplo Editors at h-diplo@h-net.msu.edu

## Introduction by Bruce Russett, Yale University

The book under discussion here is *The Arc of War: Origins, Escalation, and Transformation* by two political scientists of international relations, each with impressive track records of work drawing on both historical detail and political science theory. It is a very ambitious book that deserves close attention by an interdisciplinary audience such as the readers of H-Diplo. The authors' ambition may seem vaulting, and the book is susceptible to tough criticism. Yet ambition can be laudable, and they have their chance to make a spirited rebuttable defense at the end.

Their book attempts both to describe the usually expanding scope of warfare over the millennia and to analyze what they see as the causal forces underlying it. One controversial issue common to all the contributors concerns the quality of data on wars and casualties which varies greatly over time and space. This matter has occupied the attention of many historians and political scientists in recent decades. Certainly the record is best for Europe after about 1500 BCE, and drops off as one goes back into earlier historical periods and into prehistory. Both Stephen Morillo and Clifford Rogers are very critical of Levy and Thompson's evidence and doubt that it can support reliable generalizations. R. Brian Ferguson is a distinguished anthropologist who knows the archaeological literature well and has made important contributions to it. He has some disagreements with Levy and Thompson's use of this material, but on the whole commends it as about as good as one can hope for, and good enough for proposing some reliable generalizations about facts and causes.

Various social scientists, notably Azar Gat, Joshua Goldstein, and Steven Pinker, have made long-term generalizations from the evidence.[1] They generally support the view that, when controlling for differences in the number of people involved, the evidence for a relative decline in war deaths in recent centuries is solid enough, especially for the past 65 years. Causal consensus is harder to find, but changes in values, political institutions (democratization), the accumulation of wealth and global commercial ties, and perhaps the emergence of international organizations have claims to credit.

The debate on data here derives in large part from other differences between the particularist study of single events as practiced by many historians, and political scientists' common preference for explicit theory and generalizations. I am typically in the latter camp, but find that the disciplinary boundaries can be quite permeable. In co-teaching a

[1] Azar Gat, *War in Human Civilization* (Oxford: Oxford University Press, 2006), Joshua Goldstein, *Winning the War on War: The Decline of Armed Conflict Worldwide* (New York: Dutton, 2011), Steven Pinker, *The Better Angels of Our Nature: Why Violence has Declined* (London: Penguin, 2011). When focusing on the post-World War II era, Goldstein gives greatest credit to peacekeeping efforts by the UN and other international organizations, which is the most contestable claim. Even for the years since World War II some dispute over the data continues. See Anita Gohdes and Megan Price, "First Things First: Assessing Data Quality before Model Quality," *Journal of Conflict Resolution* and the effective defense by Bethany Lacina and Nils Petter Gleditsch, "The Waning of War is Real: A Response to Gohdes and Price," both published online 15 November, 2012 in *Journal of Conflict Resolution,* and forthcoming in print volume 57 (2013).

Yale graduate seminar, "History and Political Science Approaches to International Security" in various years with Paul Kennedy, Diane Kunz, Gaddis Smith, and Marc Trachtenberg I found a mutual acceptance and respect for disciplinary differences to be paramount. A frequent guest was Paul Schroeder, whose ability to straddle those lines is exemplary. I also benefitted much from collaboration with Carol and Melvin Ember, proprietors of the Human Relations Area files at Yale. Dispute between them and the largely dominant views of their fellow anthropologists seemed even sharper than that between historians and political scientists. On this matter too, Ferguson is more sympathetic to Levy and Thompson than are the other two commentators.

Another issue, deriving from those above, is Levy and Thompson's central metaphor, the Arc of War. It describes what they see as a rise in warfare incurring multiple fatalities produced by urbanization and greater wealth which enabled the development of a class of warriors with more lethal weapons and the ability to project power against distant settlements. The arc, they contend, rose over the centuries with periodic spurts in a process which they call co-evolution between war itself and its socio-political causes. Finally the downward leg of the arc emerged in the twentieth century as the costs of very big wars overwhelmed their seeming benefits. Every element of this metaphor—the rise, its spurts, the ultimate downturn, the causes, and the theoretical idea of co-evolution--comes under fire from one or all of the commentators. Readers of the debate will find it invigorating and informative, and, I hope, will emerge with a desire to read the book to make their own informed and varied evaluations.

**Participants:**

**Jack S. Levy** is Board of Governors' Professor of Political Science at Rutgers University. He is past-president of the International Studies Association and of the Peace Science Society. Levy's primary research interests focus on the causes of interstate war and on foreign policy decision-making. He is author of *War in the Modern Great Power System, 1495-1975* (1983), co-author (with William R. Thompson) of *Causes of War* (2010), and co-editor (with Gary Goertz) of *Explaining War and Peace: Case Studies and Necessary Condition Counterfactuals* (2007) and (with Leonie Huddy and David O. Sears) of *The Oxford Handbook of Political Psychology*, 2$^{nd}$ ed. (2013).

**William R. Thompson** is Distinguished Professor and Donald Rogers Professor of Political Science at Indiana University. He is currently the Editor-in-chief and Managing Editor of the *International Studies Quarterly* and a past president of the International Studies Association. His most recent books are *How Rivalries End* (2013, Pennsylvania), with Karen Rasler and Sumit Ganguly, and *Transition Scenarios: China and the United States in the Twenty-first Century* (2013, Chicago), with David Rapkin. *Ascending India and Its State Capacity: Resource Extraction, Violence Monopoly and Legitimacy* (Columbia), with Sumit Ganguly, should be available in 2014.

**Bruce Russett**, Ph.D. Yale University, 1961, is Dean Acheson Research Professor of International Relations and Political Science at Yale. He edited the *Journal of Conflict Resolution* from 1973 through 2009, and with Paul Kennedy staffed the Ford Foundation's

1995 report, *The United Nations in Its Second Half-Century.* A past president of the International Studies Association and of the Peace Science Society (International), in 2009 he received the Society's third quadrennial Founder's Medal for "significant and distinguished life-long scientific contributions to peace science." Of his 27 books, recent ones include *Grasping the Democratic Peace* (1993); *The Once and Future Security Council* (1997); and Hegemony and Democracy (2011). *Triangulating Peace: Democracy, Interdependence, and International Organizations* (2001) with John Oneal won the International Studies Association's prize for Best Book of the Decade 2000-2009.

**R. Brian Ferguson** is Professor of Anthropology and Director of the MA Program in Peace and Conflict Studies at Rutgers University-Newark. His primary area of research is war and political violence. A generalist, he has investigated war in tribal societies and among ancient states, archaeological evidence regarding the origins of war, large-scale identity-linked violence in the contemporary word, the current U.S. Army quest for ethnographic intelligence and cultural competence, human nature and war, and anthropological theory on war. He wrote *Yanomami Warfare: A Political History* (School of American Research, 1995) and edited *Warfare, Culture, and Environment* (Academic, 1984), *War in the Tribal Zone: Expanding States and Indigenous Warfare* (with Neil Whitehead, School of American Research, 2000), and *The State, Identity and Violence: Political Disintegration in the Post-Cold War World*) (Routledge, 2003). He is currently completing a book manuscript, *Chimpanzees, War, and History: Are Men Born to Kill?* Other interests are culture and biology, policing, and the development of organized crime in New York City.

**Stephen Morillo,** D.Phil Oxford, Professor of History and Chair of Division III (Social Sciences) at Wabash College, specializes in pre-modern comparative world and military history. He is President of De Re Militari, the Society for Medieval Military History. He has written *Frameworks of World History: Structures, Systems, Cultures,* a world history textbook forthcoming from Oxford university Press, and is working on a cultural history of warrior elites in world history. His numerous other books, articles, and chapters include *What is Military History?* and *War in World History: Society, Technology and War from Ancient Times to the Present,* a military world history textbook.

**Clifford J. Rogers** is a Professor of History at the United States Military Academy. His seven books include *War Cruel and Sharp: English Strategy under Edward III, 1327-1360* and *Soldiers' Lives through History: The Middle Ages,* both winners of the Verbruggen Prize, and the three-volume *Oxford Encyclopedia of Medieval Warfare and Military Technology*, which received a Distinguished Book Award from the Society for Military History. He is also co-editor of *The Journal of Medieval Military History.*

## Review by R. Brian Ferguson, Rutgers University, Newark

Jack Levy and William Thompson's *The Arc of War* does not suffer from a lack of ambition. It proposes an over-arching analysis that applies to all war, at all times, in all types of societies, excepting recent warfare among tribal peoples. The authors cover the origins of war, its escalation and transformation over millennia, the rise and elaboration of major variations in war trajectories, and even at the end, look into the future. Beyond the history, they provide a magisterial tour and evaluation of the literature and debates that have animated students of war for decades (most of which I know nothing about). The book can be a task for the reader, who has to assimilate a complex model and then follow its application to myriad times, places, and issues, even though it is clearly written, even conversational at times. I am intimidated by this book.

Levy and Thompson make six basic arguments:

First, the origin of war was based on tools and skills utilized in hunting, which created the capacity for killing. Then, a critical step was the development of segmented groups, with collective local identities and responsibilities for revenge. With hunting and collective identities in place, a mix of demographic, subsistence, and environmental factors leading to increasing complexity and/or scarcity created material needs that led to wars. The origin of war will receive the most discussion in this review.

Second, war co-evolves as one societal sector in conjuncture with five others: political-economy, political organization, military organization, weaponry, and threat environment. Co-evolution means changes in one sphere will interact with and produce changes in others. So changes in war may impel changes in political organization which may enable changes in military organization, which leads to changes in war, and so on. This is what general system theory used to call "systemic interaction." It is "co-evolutionary" because these developments rise and spread through variation and selective retention–some win out over others, and their ways continue into the future.

Third, although there is no strict causal hierarchy among these sectors, the most important driver is political-economy, which in this book looms largest as gross changes from hunting and gathering, to agriculture, to industrialism. But those evolutionary changes also bring increasing costs of war, especially with industrialism.

Fourth, there have been three major accelerations in war. The first began in southern Mesopotamia with the rise of cities and states during the Bronze Age of the late fourth-early third millennium. The second occurs with war by Iron Age states and empires of the eastern Mediterranean and China, in the last half of the first millennium BCE. These accelerations are discussed and illustrated with cases of Ancient Egypt, Greece and Rome, China, and Mesoamerica. The third acceleration, with which we are most familiar, began around 1500 CE in the Europe. That spiraled outwards along with colonialism, changing the world in the process, and reached a zenith in 1945 with the A-bomb.

Fifth, within these broad accelerations, regional dynamics are pushed by efforts to centralize regional political and military power, producing a major change in threat environments. This is most pronounced in the third acceleration and in its western trajectory, and receives the most attention as a test of their theory. Their reconstruction shows more then ten millennia where one power with a "paradigmatic army" got out ahead of its competitors, transforming military competition and leading to a new coevolutionary sequence.

And sixth, great swaths of the world did not go through the third acceleration, remaining more agricultural than industrial. This characterizes much of the global south. These have less developed state organizations, and are consequently vulnerable to internal warfare, and less involved in lengthy inter-state wars.

I am an anthropologist who studies war, a stranger in the intellectual landscapes usually covered in H-Diplo reviews, and to most of what is critically considered in *The Arc of War*. My comments here will focus on areas where my research intersects with Levy and Thompson's theory: an alternative general model of war, the origins of war, and a closing comment about wars within weak states today.

I too developed a general cross-cultural model of war in an article titled "A Paradigm for the Study of War and Society."[1] The approach derives from Marvin Harris's *Cultural Materialism*,[2] which divides aspects of social life into three broad categories of infrastructure, structure, and superstructure. To borrow a paragraph from the article, this framework categorizes sociocultural phenomena into infrastructure, structure, and superstructure, which are

> conceptualized together as a complex hierarchy of progressively more limiting constraints. Somewhat simplified, infrastructure is a broad conjuncture of variables involving interaction with the physical environment, population characteristics and trends, technology, and the labor techniques of applying technology, which affect a people's physical existence and relation to nature. Structure consists of organized social life, patterns of interpersonal connections and divisions sorted into social organization, economics, and politics. Superstructure includes the mental constructs of culture, its belief systems, and patterned emotional dispositions. Each level is hugely complex, and each is equally important for human existence. But they relate to different aspects of the culture of war and provide answers to different sorts of questions.[3]

[1] R. Brian Ferguson, "A Paradigm for the Study of War and Society," in War and Society in the Ancient and Medieval Worlds: Asia, The Mediterranean, Europe, and Mesoamerica, ed. Kurt Raaflaub and Nathan Rosenstein, 389-437, Cambridge: Center for Hellenic Studies, 1999.

[2] Marvin Harris, *Cultural Materialism: The Struggle for a Science of Culture* (New York: Random House, 2001).

[3] Ferguson, "Paradigm," pp. 389-390.

A fundamental difference between my approach and that of Levy and Thompson is that - cultural materialism explains sociocultural patterns in general, and I then applied that theory to war, whereas they developed their model to understand the coevolution of war, specifically. Levy and Thompson's six co-evolving spheres involve mixes of infrastructural and structural variables, and exclude superstructure variables (beliefs, values, patterned dispositions, norms) as irrelevant to broad patterns and long-term trajectories. For their purposes, that is clearly the way to go. Imposing a framework like mine would be unnecessary and fatally cumbersome, even though the results of their analyses are broadly compatible with cultural materialism.

The strength of this focused approach also sets its limitations. The authors certainly do not intend their model to apply to such questions as to why societies have the religious or kinship systems they do, as cultural materialism aims to do. Neither do they claim the ability to explain how scarcities worth killing over come to exist, or the bases of war-structuring patterns of alliance, or the variation between periods of intense war and times of relative peace, or why particular wars happen when and where they do. The cultural materialist model does address such questions. To make an obvious point, theory is a tool for understanding what you want to know.

Turning to prehistory, Levy and Thompson refer to my earlier research on archaeological evidence on the origins of war. The following comments draw on my more recent research on prehistoric war mortality and the origins of war in Europe and the Near East.[4] This provides considerably more detail on developments in the formative periods of Western war, the subject of Levy and Thompson's Chapter Two and other discussions. It slots neatly into their grand theoretical scheme in that my coverage ends at the point their first acceleration begins, the Bronze Age.

Levy and Thompson red-line biological hypotheses on innate proclivities to war as both questionable and irrelevant for the questions they address. They avoid the spell cast by a few archaeologists and many psychological Darwinists, who claim that evidence shows war existed throughout human prehistory; and that on average, 15% of all mortality, and considerably more for males, was due to war. This figure has become axiomatic for many scholars. But the claim of the ubiquity of prehistoric warfare is entirely unsustainable, and the estimate of 15% average war mortality is absurdly high. Both assertions are based on a very small set of very unrepresentative cases of violent death.

Levy and Thompson's discussion of origins provides a much better grounded human context for situating early warfare. Their coverage of the anthropological literature is well

[4] R. Brian Ferguson, "Pinker's List: Exaggerating Prehistoric War Mortality," in Douglas P. Fry, ed. *War, Peace, and Human Nature: The Convergence of Evolutionary and Cultural Views* (New York: Oxford University Press, in press); "The Prehistory of War and Peace in Europe and the Near East," in Douglas P. Fry, ed. *War, Peace, and Human Nature: The Convergence of Evolutionary and Cultural Views* (New York: Oxford University Press, in press). Both articles are available from the author on request.

informed and even handed. Floating above all the theoretical fights that animate this literature, they show how much common ground exists. But as I will elaborate, the existing literature can use some refinement, and I do take issue with one of Levy and Thompson's main points--the importance of hunting technology and techniques as precursors of war.

In this emphasis they follow the position of Keith Otterbein,[5] (although Otterbein goes further to claim war as a concomitant of big game hunting that goes back deep in our evolutionary history). The archaeologists Jonathan Haas and Matthew Piscitelli[6] show the fragility of Otterbein's evidence for Upper Paleolithic warfare. They searched through reports of 2,930 human remains from over 400 sites prior to 10,000 BCE, and found very little evidence of violence, and only one very exceptional case of war.[7] They discuss the remarkable cultural similarity of archaeological remains–with no breaks and differentiation as typically occurs when war is present--from Europe to Siberia 40,000-25,000 years ago, and across all of North America for thousands of years after its initial settlement. All of those people were hunter-gatherers. On the other hand, the most dramatic examples of large scale killing in all of Europe come from Neolithic Talheim and Herxheim in Germany and the killings there were done with the adzes local people used to work wood.[8] Where there is a will to kill, the means will be found.

On another of Levy and Thompson's key precursors of war, they are right on the money. They follow Raymond Kelly[9] in emphasizing social segmentation: differentiation of populations into discrete, corporate groups, with recognized membership, and collective liability and right to avenge killings. With this development, the social substitutability inherent in what we call war is reached: it is no longer about killing a particular individual, but anyone of 'them.' It is remarkable how quickly and widely Kelly's point has been accepted and incorporated into otherwise disputatious archaeological and anthropological publications, including my own. Often group membership is not archaeologically ascertainable. But one of the great contrasts between Europe and areas of the Near East, especially the Southern Levant as discussed below as a peaceful region, is that across Neolithic Europe, collectively constructed partial enclosures surrounded many settlements. Some of these were suitable for defense, some not, but all suggest distinct local identifications. Nothing like that has been found in the Southern Levant.

[5] Keith F. Otterbein, *How War Began* (College Station: Texas A & M University Press, 2004).

[6] Jonathan Haas and Matthew Piscitelli, "The Prehistory of Warfare: Misled by Ethnography," in Douglas P. Fry, ed. *War, Peace, and Human Nature: The Convergence of Evolutionary and Cultural Views* (New York: Oxford University Press, in press).

[7] That is Nubian Site 117, or Jebel Sahaba, long reputed as the earliest clear evidence of war, and cited by Levy and Thompson. Questions about Jebel Sahaba are raised in my article, "Pinker's List."

[8] Jean Guilaine and Jean Zammit, *Origins of War: Violence in Prehistory* (Oxford: Blackwell, 2005), pp. 86-95.

[9] Raymond C. Kelly, *Peaceful Societies and the Origins of War* (Ann Arbor: University of Michigan Press, 2000).

My explanation of the origins of war has posited a number of causally intertwined preconditions, which in varying combinations make the onset of war more likely. These include concentration of key resources, major ecological reversals, more settled villages, higher population density, stored food or livestock, social segmentation, political hierarchy, and monopolizable long-distance trade in high value items–a factor which is customarily overlooked (as by Levy and Thompson) but often seems critical.[10] My recent articles take an expanded approach, also drawing upon anthropological research on peace. Peace is not just the absence of war, it is an active state with its own contributing factors that are distinct from those that explain war. As discussed by Douglas Fry, these include cross-cutting ties that connect different groups, mutual interdependence and cooperation, value systems that encourage peace and discourage violence, socially recognized authorities that can prevent attacks, and workable processes to resolve conflicts.[11] With varying mixtures of both, different areas may have markedly different trajectories of social development, militaristic or peaceable. This is shown by the different evolution of war in Europe and the Near East.

In Paleolithic Europethere is no good evidence of war. True, two extremely early instances of cannibalism might have arisen from intergroup killings, but these are prior to modern *Homo sapiens*, and even modern humans sometimes eat their own–so war cannot be diagnosed. Other than that, there is one child killed by an arrow in the spine, and a woman who recovered from an arrow in the hip. Violence, yes, but single cases can come from many contexts. No one doubts that homicide has been around a long, long time. The first multiple violent deaths suggesting intergroup violence appear scattered across the Mesolithic period, very roughly 8,000 BCE up to the inception of agriculture. The Mesolithic, generally, was a time of climatic fluctuation, population growth, geographic concentration of subsistence efforts, and sometimes social separation and increasing complexity–all of which are preconditions of war.

The earliest Neolithic communities across the continent, anywhere from 6,500 BCE to 4,000 BCE, lack evidence of war. Really bad violence arrived with a bang in Northern Europe with Talheim and Herxheim around 5,000 BCE. Elsewhere and after, signs of war vary greatly by and across regions. A broad generalization is that signs of war become more widespread though Middle and Later Neolithics, as sedentary populations grew and signs of social differentiation increased. By the final Neolithic period, there are signs of the cultural valorization of warriors. By the subsequent Copper and Bronze Ages, Europe was covered by societies with elaborate war weaponry and militaristic elites, the barbarians of our imagination, even though how much actual killing went on remains an open question.

[10] R. Brian Ferguson, "The Birth of War", *Natural History*, July 2003, 28-35.

[11] Douglas P. Fry, *The Human Potential for Peace: An Anthropological Challenge to Assumptions about War and Violence* (New York: Oxford University Press, 2006); Leslie E. Sponsel and Thomas Gregor, eds. *The Anthropology of Peace and Non-Violence* (Boulder: Lynne Rienner, 1994).

Europe provides good illustrations of the origins of war following upon the intensification of its preconditions.

My discussion of the Near East covers the areas north and west of Mesopotamia, which is the focus of Levy and Thompson. I divide it into three broad regions: the Southern Levant, from contemporary Israel through Jordan and Lebanon to southern Syria; Turkish Anatolia; and the northern Tigris area of northeastern Syria and northwestern Iraq. The northern Tigris could justifiably be called the birthplace of war. Two sites suggest the presence of war around 8-7,000 BCE, the earliest in the Near East. In the seventh millennium, another site has a clear fortification wall (after many years without one), also the first in the Near East, or anywhere that I know. In the sixth millennium, the Halaf culture, which is the first culture known from anywhere to expand by violent conquest, crystalized. Around 4,200 BCE, enormous settlements, perhaps states, emerged, challenging the idea that southern Mesopotamia was the heartland of cities. Between 3,800 and 3,500 BCE, one has mass graves, and the other was destroyed by an assault, then occupied by people of southern Mesopotamia.

Except for the Halaf, these bloodily superlative sites are less than 90 miles apart, in a locale where the trade routes for Anatolian obsidian came down to the plains, and connect to paths leading west to the Mediterranean coast, and south to Mesopotamia. The Halaf were deeply involved in obsidian trade, and their expansion followed those routes. The most likely cause for the development of war in the Near East was efforts to monopolize the trade in this precious stone. Anatolia followed suit. The earliest unambiguous indications of war appear in the sixth millennium, several of them associated with Halafian intrusions. In the Copper and Bronze Ages, signs of both extensive trade in precious goods and war signs persist and expand, connecting the fringe of Mesopotamia to Greece in a system of war. This system has continued unbroken down to modern times.

The Southern Levant offers a striking contrast. The record begins with pre-agricultural Natufian people around 13,100 BCE, who themselves developed cultivation by around 9,600 BCE. Beginning with the Natufian, and continuing until the Early Bronze Age IIb around 3,200 BCE, there is not a single case where the presence of war is demonstrated by evidence. There are a few possibilities–an ambiguous wall, burned structures, or odd burials--but they are weak cases, all considered. There are only about seven individuals who were the victims of lethal violence for the entire span. On the other hand, the consistent absence of typical war signs is eloquent. Settlements were spread out, undefended, and grade from large to tiny, extensive interaction spheres extended across regions, and cultural differentiation occured not by boundaries or gaps but only by distance. There were no pronounced chiefly hierarchies, no marking off of one village from others, no profusion of weapons, and no militaristic iconography or burials. One intriguing variable I learned of after my papers were in press is that there is very little to suggest a male-dominated gender hierarchy, as shown by equal mortuary treatments and representations of both sexes in art.[12] Yet there are times when war would seem to have

[12] Jane Peterson, "Domesticating Gender: Neolithic Patterns from the Southern Levant," *Journal of Anthropological Archaeology* 29 (2010): 249-264.

been expectable, such as periods where recently expanded populations ran into severe environmental downturns.

My hypothesis is that the people who domesticated nature also domesticated conflict. They developed a peace system, with major ritual centers and sometimes burial areas located between settled areas, redistribution centers linking different ecological zones, and extensive trade in both practical and ritual goods. What brought this peace to a violent end was the rise of the Pharaohs in Egypt. Centuries of balanced trade between 'Canaan' and the Nile turned into militarily enforced tribute, and in an archaeological instant, the whole land of Jordan was fortified. Contrary to dreary but ever-popular "beast within" theories on the antiquity of war,[13] the people of the Southern Levant went nearly ten millennia without the plague of war. That amounts to roughly 500 warless generations, quite possibly on top of thousands of generations before, and the record was broken only by a conquering empire. Humans are perfectly capable of living in peace.

This record can be accommodated with Levy and Thompson's model. As noted previously, the people were not segmented into distinctive local groups, unlike much of Europe. Seen in terms of preconditions for peace, they were actively integrated by exchange, cooperation, and ritual; and the planning evident in ritual center construction and some settlements suggests recognized authority. Those considerations were major factors in their threat environment, or lack of one. Under a developed system of peace, the real threat could be the exclusion of belligerent parties from all the life-sustaining benefits of cooperation. After the Scorpion King of Egypt's Dynasty Zero, peaceful cooperation was no longer an option. It was a Hobbesian time. This abrupt transformation is about as good an example of selection as one could imagine, and equally well illustrates Levy and Thompson'scoevolutionary watershed of the rise of centralizing political structures.

I do have one critical observation in closing. Levy and Thompson's third acceleration of war focuses on Europe from 1500 CE on. Much of the world did not pass through this acceleration itself, the authors argue, instead it experienced it only indirectly. These countries did not experience the state-building wars of European history, and borders were often just lines drawn on maps by foreign powers. They remained primarily agricultural, with few and short interstate wars, and recurrent problems of intrastate wars. But are these conditions best understood as the failure to pass through Acceleration Three?

When Europe first encountered them, the armies of much of Meso and South America, Africa and southern Asia were more formidable than later military organizations in those same regions. Europe's subjugation of local chiefdoms, kingdoms, and empires through a combination of disease, commerce, alliances, and over time, increasing advantage in

[13] E.g. Robert Ardrey, *African Genesis* (New York: Dell Publishing, 1961); Michael Ghiglieri, *The Dark Side of Man (*Cambridge: Perseus Books, 1999); William James, "The Moral Equivalent of War," in *War: Studies from Psychology, Sociology, Anthropology*, Leon Bramson and George Goethals, eds. (New York: Basic Books, 1965), 21-31; William McDougal, "The Instinct of Pugnacity," in *ibid.,* 33-43; Richard Wrangham and Dale Peterson, *Demonic Males: Apes and the Origins of Human Violence* (New York: Houghton Mifflin Co., 1996).

military technology and organization, destroyed their political economies, political structures, and militaries. Even simpler peoples were enmeshed in extensive, complex networks of social integration, which frequently included war. These locally appropriate systems developed in place through their own long history of social evolution, and were obliterated or radically distorted by the colonial experience. This is not to say that those peoples would have developed their own industrial and military revolutions if left alone, but rather that centuries of colonialism–surely a perfect example of industrialism's tendency toward consolidation and centralization–should be foregrounded to understand the weak states and frequent internal wars that often characterize the global south today.

## Review by Stephen Morillo, Wabash College

Jack Levy and William Thompson are political scientists making a brave foray into military and world history. Their stated aim is not to write a chronological, narrative history of warfare, but to present a theoretical, model-based, explanatory overview of the history of warfare from its origins to the present state of global armed conflict (p. 1). Their focus is therefore not on the details of military history but on big patterns, and they hope that their approach will provide greater insight into the development of warfare in ways that will assist the formation of current military policy and clarify the possible future challenges that policy makers face. They make six major arguments about war, as outlined clearly in the introductory chapter.1. They present a theory about the origins of war. 2. They argue that war coevolved with other factors. 3. They claim that political economy is the most important of these factors. 4. They argue that there have been three major "accelerations" in the history of war (p. 2). 5. They argue that attempts to centralize political-military power drove these accelerations. Finally, the claim that the third acceleration was largely confined to "the western military trajectory," making for a process of differentiation and transformation that explains the current divided state of global military power (p. 3).

Attempting to model the evolution of war is an admirable aim. Doing so highlights the difference between historians' particularist explanations of individual wars and political scientists' desire to explain wars in general, a comparison in which the advantage is, even to this historian (though one who admittedly likes model-based approaches to studying the past), by no means all with historians. The potential strength of Levy and Thompson's approach is that it could provide a basis for cross-cultural comparison and give their explanation some predictive power. But a whole series of problems make their project, ultimately, a disappointment. The strengths and weaknesses of this book are best approached through a serial analysis of each of these arguments.

The section of the book on the origins of war (Chapter 2) is the strongest of all and constitutes a valuable overview and synthesis of the large and contentious literature on this topic. Defining war (quite reasonably, in this reviewer's opinion) as "sustained, coordinated violence between political organizations" (p. 3), they conclude that while the exact origins of warfare must remain somewhat murky due to the paucity of evidence, it clearly emerged from the same set of factors that gave rise to agriculture and settled societies, and thus was non-existent before around 12,000 BCE. The problem here is that while the conclusion is reasonable, the evidence is indeed sparse, and any conclusion must remain so tentative as to provide extremely insecure grounds upon which to build further conclusions. But Levy and Thompson devote the effort to establishing the origins of warfare because they argue that the nature of those origins are important to the subsequent evolution of warfare (p. 19). It is therefore not clear how this chapter ultimately connects to the rest of the project or what it has to say that carries forward.

Their second argument about the *coevolution* of war underlies the entire project, as it generates the theoretical model the authors deploy throughout. They see war as evolving in tandem with five major factors that they isolate from the mass of possible influences on the practice of warfare. Their list of factors includes military weaponry, military organization, political organization, threat environment, and political economy. This list contains the first serious problem of historical analysis, in this reviewer's opinion. The list reveals the social science methodological preferences of the authors: it is a list of materialist, 'pragmatic' factors. It removes culture and cultural perceptions from the analysis. The authors acknowledge and attempt to justify this move, but their argument takes an almost neo-Marxist and certainly overly narrow view of culture as superstructure (not their word) or window dressing to the real materialist factors that matter in the long run. Yet in cases too numerous to lay out in detail, culture has clearly played a role in the evolution of war. The authors' own discussion reveals this. Examples range from earliest times to the present: the authors note the formation of group identities as crucial to the processes that led to the invention of war — what more cultural process can there be? — while their analysis of the decline of major-state warfare in the late twentieth century comes down to shifts in attitudes towards the costs and benefits of war. Their own emphasis is on actual shifts in the costs and benefits, but any cultural historian will know that perceptions are as crucial as (and often shape) material reality in such cases, which the authors' use of the word "attitudes" (e.g. p. 146) implicitly acknowledges.

Almost as an aside, the social science methodology the authors employ produces its worst results in the set of tables in Chapter 1 (Figures 1.2-1.5) that purport to establish the trajectory of war in various numerical-demographic ways. By charting, for example, the severity of great wars since 1490 in terms of the absolute number of deaths, rather than as a percentage of global population, the information they convey strikes this reviewer as being virtually meaningless. In general, the charts are built on data sets unreliable enough as to make any conclusions suspicious.

The lists of wars in these tables also reveal a second problem of historical analysis: state-centrism. The book's argument suffers from a problem that is common to much political science and historical analysis, in that it takes states as a central analytical unit to the exclusion of networks of exchange and interaction. Among the many problems this perspective creates is that it allows the authors to characterize many of the states in today's world as still "agrarian."(p. 3). Industrialization is not a process isolatable to individual countries: no state today whose economy is still primarily agricultural is agrarian in the way that any pre-industrial state was. Global economic and cultural connections make equating them, as the authors do, highly questionable.. The distorting effects of ignoring network connections extend back into the many charts the authors present of agrarian era military power that valorize particular states (e.g. the Carolingian Empire for the "feudal" era of "western" military development) as models of military organization. Finally, ignoring networks leads the authors to overemphasize the role of states as opposed to market-based private enterprise in many aspects of military organization (the authors should read David Parrott, *The Business of War* (Cambridge, 2012) on this point).

Finally, in critiquing the model that the authors' list of factors generates, it must be said that the model ends up generating, at least for historians, very little real insight. Of course historical events are not monocausal. Of course war coevolves with other factors. Serious historical analysis of war has always recognized this. Putting a new term on the complexities of historical reality does not make for new analysis. And yet, on the other hand, the model that the authors generate is complex enough and builds in enough in the way of contingent connections among its various factors that it cannot really generate predictions precise enough to actually test: almost any piece of evidence can be said to fit it.

The authors' third argument is that though all six factors (the five noted above plus war itself) influence each other in varying ways (this is the central concept of coevolution), primacy at the largest scales of history belongs to political economy. The conclusion this should lead to is that there are three major eras in world history, and by extension world military history: the hunter-gatherer era extending to around 10,000 years ago; the agrarian era; and the industrial era that began about 200 years ago. The book gets the first two of these basically right. Levy and Thompson claim, plausibly and as noted above, that the origins or war lay near the end of the hunter-gatherer era in the conditions that also gave rise to agriculture. They locate the first two "accelerations" of warfare, involving first the creation of urban-based military forces in the third millennium BCE and, second, the emergence of administratively and tactically more complex armies and empires in the second half of the first millennium BCE, within the agrarian era. This, too, is plausible, though their specific interpretations are at times contestable and the case studies they present are very Eurocentric. It is open to question, however, whether the specific eras of change they identify are in fact the most significant over the long agrarian era, and whether "acceleration" would always be the best word for the changes that were significant. The fragmentation of political and military power in many regions of Eurasia after the age of the great classical empires, for example, hardly fits the trajectory that Levy and Thompson's accelerations imply.

The real problem, however, comes with what they claim is the third great acceleration, dated to the second half of the second millennium CE, that is, since about 1500, and confined, so they argue, to the "western military trajectory". Their model should have pointed them to the changes in warfare (and economy, society, politics, and everything else) brought about by industrialization since 1800: this is clearly the third great era of world history and a period when warfare changed significantly both organizationally and technologically. The nineteenth century, however, has always been the red-headed step child of Military Revolution theories, as no single technological revolutionary moment stands out: military change is part and parcel of much larger changes. And the central problem is that Levy and Thompson's presentation of the third "acceleration" is a recycled, thinly theorized version of the standard literature on 'The Military Revolution of Early Modern Europe,' and shares with that literature all its faults.[1]

[1] See, e.g., Jeremy Black, *Beyond the Military Revolution: War in the Seventeenth Century World* (New York, 2011) for a fairly recent discussion of the period of the Military Revolution with full bibliographic references and an excellent critique of the standard Military Revolution literature. The central work in the standard view is Geoffrey Parker, *The Military Revolution* (Cambridge, 1988).

These faults include problems of deciding what the 'revolutionary moment' was in the Military Revolution (Cliff Rogers has extended the European evolution of warfare back to 1300, and his model of "punctuated equilibrium evolution" is a more useful adoption of biological metaphor, in many ways, than the "coevolutionary" model Levy and Thompson deploy[2]), what the causal relationship was between changes in military technology, military organization, and social and governmental development (a problem Levy and Thompson's multi-factor coevolutionary model neatly sidesteps without illuminating), and explaining exactly what the global impact of European developments were (a topic on which Jeremy Black has written extensively, and that is actually clearest in terms of naval warfare, a topic Levy and Thompson virtually ignore[3]).

The central philosophical problem with the Military Revolution literature, however, and the one that Levy and Thompson not only repeat but exacerbate, is one of definition. The authors treat this "acceleration" as confined to the "western military trajectory." No coherent definition of 'western" is possible that justifies this. Why do Sumer and Egypt count as "western" in early history (down through sometime around the early first millennium CE) but not later? (Not even Victor Davis Hanson, champion of the "western way of war", includes these societies — his "western" starts with the Greeks, a more intuitive beginning point that still does not stand up to scrutiny.[4]) Nowhere do the authors define what they mean by 'western".

The book offers no definition of 'western' based on geographic or cultural continuity. This reviewer is skeptical that a coherent definition based on geography or culture is possible. Nor could the book offer a culturally-based definition of 'western' continuity without undermining central assumptions of the model it presents, since the model ignores cultural factors.

By focusing on an undefined 'western' trajectory, the book ignores the recent and growing literature on Asian military developments in the early modern period that make European developments look far less exceptional.[5] Furthermore, the idea of a 'western' trajectory then leads to a conflation in the book's analysis of "western" and "modern" (the latter defined as "industrial") in explaining military developments over the last century. This seriously obviates the value of their final theme, the differentiation of military power since 1945. While they accurately observe the pattern of strong-state peace and weak-state

[2] Clifford Rogers, "The Military Revolutions of the Hundred Years War," in Clifford Rogers, ed. *The Military Revolution Debate* (Boulder, 1995).

[3] Black, *op. cit.,* and *War in the Eighteenth Century World* (New York, 2013). On naval developments and the rise of the fiscal-military state see also John Brewer, *The Sinews of Power* (Cambridge, MA, 1988) andJan Glete, *Warfare at Sea, 1500-1650* (London, 2000).

[4] V.D. Hanson, *The Western War of War. Infantry Battle in Classical Greece* (New York, 1989).

[5] See, e.g., Peter Lorge, *The Asian Military Revolution. From Gunpowder to the Bomb* (Cambridge, 2008); David A. Graff and Robin Higham, *A Military History of China* (Lexington, KY, 2012).

internal war that has emerged after World War II, the problems of analysis in the chapters leading to this conclusion should make readers question their explanatory scheme here.

Given the problems with the arguments the authors present, it is necessary to conclude that despite the book's admirable ambition, in the end this study does not live up to the promise the project might have had. Neither historians nor political scientists will find much of value here. We still await a broad, truly global analysis of warfare in world history.

## Review by Clifford J. Rogers, United States Military Academy (West Point)

Trying to analyze a large topic in a small book is a dangerous undertaking. Authors who undertake the task must try to find a middle course between generalizations so broad that they are true but obvious, and assertions that are not obvious, but that cannot, within the space available, be persuasively supported. Thus, in attempting to "explain the origins, escalation, and transformation of warfare" (1) on a global scale, from its prehistoric origins to it probable future trajectory, all in 217 pages of text and 23 pages of endnotes, the distinguished political scientists Jack S. Levy and William R. Thompson have certainly earned the descriptions of "brave," "bold," and "ambitious" used on the book-jacket blurbs. However, in the case of *The Arc of War* the disproportion between those stated objectives and the scale of the book is so great that I do not see how any authors, however intelligent and learned, could navigate between the twin dangers noted above without wrecking against one or both.

In the best traditions of political science, Levy and Thompson are very clear about what they are arguing, and very systematic in laying out their points and how their conclusions relate to the work of other scholars (including historians and anthropologists as well as other practitioners of their own discipline). On the first three pages, they identify six arguments, which can be summarized as follows: first, war, defined as "sustained, coordinated violence between political organizations," (3) began in different regions at different times, as human groups became more complex and began to compete for scarce resources; second, "War coevolved with other activities, including military and political organization, political economy, threat environment, and weaponry" (1); third, of the six co-evolving factors, the most important for transforming war have been "political economy," meaning the organization of society's means of production; political organization; and threat environments. The general pattern has been that more efficient production has led to population growth and larger and stronger political organizations fielding larger and better-armed military forces, creating a threat environment requiring other groups to follow suit, leading to larger and more severe wars. That pattern, however, was broken after the first half of the twentieth century, when the increasing severity of war pushed its costs (at least for Great Powers fighting other Great Powers) past the level of any possible benefits, making war less probable.

Fourth, coevolutionary change in warfare has not occurred at a steady rate, but rather in bursts of radical change separated by periods of relatively gradual development. At the macro level, there have been three periods when change occurred with especially great rapidity: the centuries on either side of 3000 BCE; the period 500-1 BCE; and, in some parts of the world, 1500-2000; fifth, the attempt to centralize regional political-military power is one of the major drivers of periods of acceleration and transformation, especially in the third acceleration, which was concentrated in the western trajectory" (2); and sixth, the states of the non-industrialized world, having bypassed the third acceleration, missed out on its state-building action, and are therefore weaker, making them both more prone to internal warfare and less able to sustain long interstate wars.

The first argument is the subject of Chapter Two. Levy and Thompson lead off by observing that there is no consensus among scholars about when and how war originated, but that "although data limitations may preclude us from ever knowing the precise origins of war with any certainty, we need to think about this question because it could have strong implications for our understanding of subsequent evolution of war and its contemporary manifestations." (19) The sentence just quoted illustrates a number of my concerns about the book. First, it seriously understates how problematic the information available to us is: actually, data limitations *will certainly* prevent us from knowing even the *approximate* origins of war with any *probability*. As to the current state of our knowledge, Levy and Thompson first note that "there is little evidence of war 50,000 years ago"— a statement about absence of evidence, not evidence of absence, still allowing the possibility that the phenomenon of war is that old or older—and then admit that it is "anybody's guess" as to whether the gradual extinction of *Homo neanderthalis* involved warfare with *Homo sapiens* starting around 35,000 years ago. (4-5) If we know so little about the earliest warfare involving *Homo sapiens* that we cannot say with confidence whether it was already practiced by the first members of the species over 100,000 years ago, and it is "anybody's guess" whether it existed in 35,000 BCE, or dates back only to some time after 8,000 BCE (53), or even only after 4,350 BCE (Table 2.9, p. 43)—if we cannot pin down its origins even within a 10,000-year margin of error—then we should admit that we also cannot really know much of anything about the origins of war that does not derive tautologically from our definition of war. Levy and Thompson, along with many other scholars, believe that the value (and they are far from alone in this), the value we *would* derive from knowing something about the earliest warfare (e.g. whether it emerged before or after the development of bows, or of agriculture) justifies the effort to try to answer the basic questions about it. They therefore devote one of eight chapters in the book to the topic, despite the high opportunity cost of doing so. To my mind, however, the *desire to know* does not justify the *effort to know* if the effort is doomed to failure, which in this case it seems to be. That basic problem cannot be disarmed by the passing, but again greatly understated, admission that "the paucity of early evidence…forces us to speculate *to some extent* about the question of origins" (19, emphasis added).[1]

After warfare began, Levy and Thompson argue, it "coevolved with other activities, including military and political organization, political economy, threat environment, and weaponry." (1) With this argument we shift from the problem of potentially useful

[1] Similarly, on p. 33 Levy and Thompson ask, with reference to Raymond Kelly's analysis of the linkage between use of marriage payments and warlike behavior in foraging groups in historic times (in *Warless Societies and the Origin of War* [Ann Arbor: University of Michigan Press, 2000]), "what does this very early co-evolution between social organization and warlike activity tell us about the history of warfare"? But the data from which we they proceed to draw conclusions is actually not, despite their use of the phrase, "very early." The justification for this elision is on p. 32: "since we have no information on group attributes in the distant past, the proxy behavior of known foraging group behavior may be as close as we can get to analyzing them in any systematic fashion." Indeed, it probably is *as close as we can get*, but that does not necessarily mean that we can assume it is *close enough* to make drawing any conclusions from it methodologically sound. Levy and Thompson on p. 19 note "the paucity of early evidence" as a problem "that must be addressed or at least highlighted before we can expect to make much headway." I would say that *highlighting* the problem, in this case, is certainly is not sufficient to clear the road for significant progress.

observations that cannot be properly supported, to assertions that can easily be supported, but are not very useful. To say that war coevolved with the other five factors means simply that war changed over time, and that "a substantial change in one of the six spheres is *likely* to lead to major changes in *some* or all of the other spheres" (13, emphasis added). The latter implies that "none of the six primary variables has been the exclusive driver [of change] throughout time" (208). The null hypothesis to the former proposition would be that each of these six variables is largely independent of all the others, so that a substantial change in any of them would be unlikely to result in a major change of any of the others. The null hypothesis to the second quotation would be that five of the six factors never drive change in any of the other spheres. Those null hypotheses are propositions which are so clearly false that they do not really require scientific refutation. If the null hypotheses do not merit scientific refutation, then the initial hypotheses do not merit elaborate supporting argument.[2]

The book's third argument does make claims that are not obvious, but they bring us back to the problem of insufficient support. It would be useful to learn that changes in weaponry and in military organization have overall been less significant as drivers of change than political economy, political organization, and threat environments (209). Yet, as Levy and Thompson recognize, there are reasonable arguments that developments in weaponry and military organization have sometimes driven major changes in political organization and others among the six factors. For example, the rise of the hoplite and the phalanx in ancient Greece is generally believed to have led to a political system which spread political power more widely among the citizens of the Greek *poleis*. Moreover, every major change in weaponry and military organization in one political community by definition amounts to a major change in the threat environment of its neighbors. So why should weaponry and military organization be deemed less important as drivers of change than alterations in threat environments? Thus Levy and Thompson have failed to make a persuasive case for the lesser importance of military organization and weaponry as drivers of change, relative to the other factors.[3]

Levy and Thompson not only argue for the relatively lesser importance of two of their six factors, they also "giv[e] priority to one of the six spheres, political-economic change, in explaining fundamental transitions in behavior over the very long term."[4] By this they mean that the transitions from hunter-gatherer society to agricultural society to industrial society have brought about the biggest changes in warfare. To this observation a historian's natural response is: "Of course. Those changes are so big that they carried in

---

[2] The concluding sentence of *The Arc of War* is: "The only thing we can be sure of is that coevolutionary processes will continue to shape and reshape warfare and related phenomena." (217)

[3] On p. 151, Levy and Thompson note "that innovation in military technology, while important, has not been the *only* factor driving the contemporary process [of the evolution of war], and *not always* the most important." (Emphasis added) That is certainly true, but the implication of the phrase is that changes in military technology are at least among the *more* important drivers of change.

[4]Levy and Thompson, 14, also 54.

their wake huge transformation in *all* aspects of human society." Marrying that observation to Levy and Thompson's fourth argument (about the three macro-accelerations in change in warfare), however, raises the same sort of issues outlined in my previous paragraph. If the second and third periods of more rapid change in warfare are circa 500-1 BCE and 1500-2000 CE, that means there was relatively little change circa 1 BCE to 500 CE, or 500 CE to 1000 CE, or 1000 CE to 1500 CE. If we limit our observation to the "Western trajectory" (which for Levy and Thompson includes the Middle East during the period of the first two accelerations but not the third),[5] it could, however, very plausibly be argued that the difference between an Assyrian army fighting an Assyrian war in 700 BCE and a Roman army fighting a Roman war in 1 BCE represents much *less* change than the contrast between the latter and a Frankish army fighting a Frankish war in 500 CE. Similarly, I have argued (in an article Levy and Thompson refer to, and seem largely to accept) that European warfare experienced more revolutionary change in the two centuries *before* 1500 (with the Infantry Revolution, the Artillery Revolution, and rise of paid standing armies) than in the two centuries *after* 1500.[6] The difference of a mere two hundred years does matter in this case, because Levy and Thompson's argument that "the third acceleration was very much a product of the industrial era" (15) was already on shaky ground, since causes must come before their effects. If the most recent macro-acceleration in military change began around 1300 rather than 1500, the connection to the second grand shift in political economy (from agrarian to industrial) becomes even more problematic, undermining a major theme of the book.[7]

The second part of Levy and Thompson's third argument—which is really much more central to the book than that description implies—is that the broad "arc of war" from the dawn of history to the mid-twentieth century has been a steady increase in the cost and severity of war (measured in "battle deaths")[8], which from the fifteenth century CE onward led to a steady decrease in the frequency of war between Great Powers. The problem here is again the incompleteness and uncertainty of the data. Even though we of course know infinitely more about historical events in 1500 CE than in 1500 BCE, the data for any period

[5] See Table 3.8, p. 81.

[6] Clifford J. Rogers, "The Military Revolutions of the Hundred Years' War," *The Journal of Military History,* 57 (April, 1993), pp. 241-278. Reprinted with revisions in C. J. Rogers, ed. *The Military Revolution Debate* (Boulder: Westview, 1995).

[7] It is true that an argument could be made for the beginning of the industrial economy being visible by 1300, with greatly increased use of wind and water-power, large-scale cloth manufacturing in Flanders, burgeoning iron production, etc. But most readers would think of the Industrial Revolution beginning in the eighteenth century, if not in the nineteenth century, and indeed Levy and Thompson themselves refer to "the British-led industrial revolution in the late eighteenth century." (15) Hence, if the authors wanted to make that case that the industrial era began much earlier, they should have done so more explicitly—especially since they treat many twenty-first-century nations of the present-day "global south" as "agrarian" or "nonindustrial" (16, 75-6), though many of them have much larger industrial sectors than, say, fourteenth-century England.

[8] P. 219 n. 9 and figure 1.2 (p. 7).

before the nineteenth century is still so problematic (due to coding variations, observational biases, and incomplete records) that the quantitative sorts of analysis undertaken by Levy and Thompson cannot produce reliable results, even for identifying very broad trends.

For example: what is the pattern in the frequency of Great Power wars since 1500? The data set there should be incomparably easier to establish than the data-set of battle-related deaths over the same period, much less the level of severity of war during the second millennium BCE. Thompson and Levy argue that the frequency of Great Power wars (GPWs) has steadily declined since the sixteenth century, with the exception of an increase in the first half of the twentieth century, apparently based on the following sequence (derived from Figure 1.3):

| Years | #GPWs |
|---|---|
| 1500-1549 | 12 |
| 1550-1559 | 13 |
| 1600-1649 | 7 |
| 1650-1699 | 10 |
| 1700-1749 | 6 |
| 1750-1799 | 5 |
| 1800-1849 | 1 |
| 1850-1899 | 2 |
| 1900-1949 | 4 |
| 1950-1999 | 1 |

It is questionable whether this shows that "the frequency of great power war declined continuously" over this period, considering that of 9 changes from half-century to half-century, 4 are up and 5 are down.[9] Still, an overall downward trend does seem discernible. Even that generalization, however, depends on debatable choices about how to aggregate wars and which states to count as Great Powers, among other concerns. This means that the margin of error in the data-points caused by potential counting variation, for the period 1700-1949, is about equally as large as the variations in the data points that

[9] "The frequency of great power war declined continuously in each of the last five centuries, the only exception being the increase in the first half of the twentieth century." Levy and Thompson, 144;ee also 130.

create the pattern to be explained. For example, the "decline" from 1700-1749 to 1850-1899 is 4, and (as will be demonstrated below) the variation in the number of Great Power wars that could be counted in 1850-1899 is at least 5.

The footnote attached to the table from which the above chart was constructed directs us to two sources "for data on patterns and trends in great power war during the last five centuries," namely Levy (1983) and Levy, Walker and Edwards (2001).[10] But those two sources do not precisely agree with each other, or with *The Arc of War:*

| Years | #GPW L/T (2011) Fig 1.3 | #GPW L/T/E (2001) Fig.1 | #GPW Levy(1983) Table 4.1 (wars starting in) | #GPW Levy (1983) Table 4.1 (wars occurring at least partly in) |
|---|---|---|---|---|
| | | | | |
| 1500-1549 | 12 | 11 | 13 | 13 |
| 1550-1599 | 13 | 11 | 13 | 14 |
| 1600-1649 | 7 | 4 | 7 | 9 |
| 1650-1699 | 10 | 7 | 10 | 11 |
| 1700-1749 | 6 | 4 | 6 | 6 |
| 1750-1799 | 5 | 4 | 4 | 4 |
| 1800-1849 | 1 | 1 | 1 | 2 |
| 1850-1899 | 2 | 4 | 4 | 4 |
| 1900-1949 | 4 | 6 | 4 | 4 |
| 1950-1999 | 1 | 1 | 1 | 1 |

Look particularly at the data from 1700-1949. For that period, only one of the four columns, the first, could reasonably (though imperfectly) be described as showing steady decline until an upswing in the twentieth century. But that first column contains one clear-

[10] Jack S. Levy, *War in the Modern Great Power System, 1495-1975* (Lexington, KY: University Press of Kentucky, 1983); Jack S. Levy, Thomas C. Walker, and Martin S. Edwards, "Continuity and Change in the Evolution of War," in *War in a Changing World,* ed. Zeev Maoz and Azar Gat (Ann Arbor: University of Michigan Press, 2001), 15-48.

cut error in a key position for interpreting the overall trend: the count of Great Power Wars 1850-1899, which is given as two, should surely be at least four, including the Crimean War (1853-1856), the 1859 War of Italian Unification (with France on one side and Austria on the other), the Austro-Prussian War of 1866, and the Franco-Prussian War. Indeed, a strong case could be made (based on including China as a Great Power) that it should be seven.[11] Then the only real sign of a clearly significant decrease in the frequency of Great Power wars within the period 1700 to 1949 would be in the first half of the nineteenth century. That, however, seems to me to be a mirage based on another counting error, namely Levy's decision to treat the Napoleonic Wars of 1803-1815 as a single war, rather than as comprising six Great Power Wars (the Franco-British war of 1803-1814 and the Wars of the Third, Fourth, Fifth, Sixth, and Seventh Coalitions).

It is thus impossible to be confident that the pattern Levy and Thompson seek to explain *exists in the first place*, which makes it seem a questionable endeavor to try to explain it or to fit it to a grand "arc" stretching back into prehistory.

Levy and Thompson's argument is on comparably shaky ground in the other half of the formula that "great power warfare has steadily increased in severity while declining in frequency" (8). It is obviously vastly more difficult to measure the 'severity' of Great Power warfare than to merely count wars, which as we have already seen is difficult enough. This remains true even if we define 'severity' in a readily quantifiable, single-variable manner, which is what Levy and Thompson attempt to do. However, they do not quite manage that feat of definition, much less the far more difficult task of producing reasonably reliable data in accordance with the definition. They refer to the "severity of warfare" as "defined in terms of the number of casualties" but also as "defined in terms of battle-related deaths" (both on 7).[12] There is a big difference, however, between *casualties* and battle-related *deaths*. For example, for the U.S. in the Second World War the number of casualties was more than triple the number of battle-related deaths.[13] Importantly for Levy and

[11] Ranking China as a Great Power in this period seems reasonable given its population and economic strength and its international status; it should be recalled that it was only *at the end of* the Second Opium War in 1860 that China accepted Britain as a diplomatic *equal*. In that same year, according to Table 6 (149) in Paul Kennedy's *Rise and Fall of the Great Powers* (New York: Random House, 1987), China's manufacturing output surpassed that of the Austrian Empire, France, the German States, and Italy *combined.* If we count China as a Great Power in this period then we should also count Japan in 1894-5, since it defeated China in the first Sino-Japanese War. We would then add to the count of Great Power Wars in this half-century the Second Opium War, the Sino-French War of 1884-5, and the 1894-5 Sino-Japanese War.

[12] Also given as "battle deaths" (Figure 1.2 and p. 219 n. 9). Depending on definitions, there can also be very large differences between "battle deaths" and "battle-related deaths," if the former means only 'Killed in Action' and the latter adds 'Dead of Wounds.' For example, in the American Civil War, by those definitions and Thomas Livermore's tallies, the Union's "battle- related deaths" totaled 110,070, vs. "battle deaths" of only 67,058. Michael Clodfelter, *Warfare and Armed Conflicts: A Statistical Reference to Casualty and Other Figures, 1500-2000*, 2d. ed. (London: McFarland & Co., 2002), 331-2, drawing on Thomas L. Livermore, *Numbers and Losses in the Civil War in America 1861-65* (Cambridge, Mass.: Riverside Press of Houghton, Mifflin and Company, 1901).

[13] Clodfelter, 82, reports "Battle Deaths" (including KIA, DOW, and MIA [Presumed Dead]) as 292,131 and WIA as 671,801, for total casualties of 963,932. If we added soldiers captured and dead of disease,

Thompson's purposes, moreover, the relationships between those two different measures are not steady over time, and can also vary greatly even at one point in time between one country and another, because of technological and cultural differences. Differences that exist in reality can be made even greater by differences in counting methodologies from nation to nation or period to period.[14]

The problem is not simply one of loose use of language—of saying 'casualties' when the more restrictive category of 'battle deaths' is meant. Levy and Thompson's data series on the 'severity' of war is borrowed from Levy (1983), which generally relies on Pitirim Sorokin's numbers for *casualties* up to 1815, and J. David Singer and Melvin Small's numbers for *battle-connected deaths* for the period starting 1815.[15] The numbers provided therefore would not properly allow for comparison between the wars of the nineteenth and twentieth centuries and those of earlier periods, which is what Levy and Thompson try to do with them, even if we were to accept the basic validity of the numbers for what they profess to measure. Still, if Levy and Thompson's generalization is correct, we should see a steady rise in casualties incurred during great power warfare from the sixteenth to the seventeenth to the eighteenth century, for which their numbers are all based on the same data series (of casualties). That pattern, however, is not at all obvious from Figure 1.4 (p. 8), "Severity of great power war by quarter century," which actually seems to show a sharp rise from the sixteenth century to the seventeenth century, but no great difference between the seventeenth and eighteenth centuries. And indeed, going to the underlying data in Levy (1983), we find a total Great Power war casualty figure of 3,824,000 for the seventeenth century and 3,491,300 for the eighteenth, a slight fall rather than a steady increase.[16] Thus, it is difficult to accept Levy and Thompson's generalization even if we grant their numbers. That may be moot, however, because in my opinion we should not accept their numbers.

Some inaccuracy is inevitable and acceptable. Levy writes that "the error in these fatality estimates may be as great as 20-25 percent in some cases. Nevertheless, they are adequate for the present purpose, given the five-century span of this study. A slight systematic bias tending to exaggerate the battle fatalities in the earlier periods is marginal compared to the

---

accidents, and other categories, the casualties would be substantially larger, since for the army alone the total of POWs was 124,079 and the non-battle deaths were 115,185. (*Ibid*., 585, 586.)

[14] For example, in WWII, according to one set of figures, the British ratio of battle-related deaths to casualties was 1:2.4, whereas for the Japanese it was 1:1.2. Clodfelter, 582.

[15] Levy, 85; Pitirim Sorokin, *Social and Cultural Dynamics: A Study of Change in Major Systems of Art, Truth, Ethics, Law and Social Relationships,* 4 vols. (New York: American Book Company, 1937-41). It is a lesser concern, but still worth noting, that despite describing their series as "battle-related deaths," Singer and Small (and therefore Levy and Thompson) include troops dead of diseases "contracted in the war theater" though that is rather different from what the word "battle-related" means in normal English. J. David Singer and Melvin Small, *The Wages of War, 1865-1965. A Statistical Handbook* (New York: John Wiley & Sons, 1972), 49. Including "dead of disease" casualties in "battle deaths" produces quite different numbers, with different variations at different times, but is never noted (much less justified) by Levy and Thompson.

[16] Levy, Table 4.1.

differences between centuries and the changes over time, two of the important questions of interest here."[17] That would be fair enough if the error in the numbers was indeed usually less than 20%. But counting all casualties pre-1815 and only deaths after 1815 creates not a "slight" but a very large systematic bias overstating the numbers for the earlier period. For example, Sorokin's casualty numbers for the Franco-Prussian War are 663,864, while Levy's battle-related deaths number is 180,000.[18] Considering that for these relatively recent wars the data are far superior to what can be obtained for earlier centuries, the fact that the different methodologies produce results with a 271% variance indicates that a data series combining the two sources is so flawed as to be of little use. Moreover, the bias is not entirely consistent in overstating the figures for the first period vs. the second: for the First Huguenot War (1562-4), Sorokin's figure for combined French and English *casualties* is only 3,850, whereas Levy gives 6,000 for battle-related deaths—a variance of 56%, but in the opposite direction than one would expect![19]

That the level of measurement error in the data series is also much greater than 20-30% (Levy, p. 87) even for the much better-documented post-1815 period is suggested by comparisons between Levy's data and numbers from other sources. (This is not to say that the discrepancy indicates Levy is wrong and the other sources are right; rather, the point is that the uncertainty of the numbers is very great.)[20]

| War | Battle-related Deaths per Levy (1983) | Battle-related Deaths per Clodfelter (2002) | Variation (as % of smaller) |
|---|---|---|---|
| Crimean War | 217,000 | 118,895 | 83% |
| Italian Unification (1859) | 20,000 | 19,599 | 2% |
| Seven Weeks War (1866) | 34,000 | 16,357 | 108% |

[17] Levy, 86-7.

[18] Levy, Table 4.1 (though Singer-Small have 187,500 [Table 4.2]); Sorokin, 3:569, 553. Levy, 85, seems to be incorrect when he says that Sorokin's *casualty* figures are less than one-third the Singer-Small *battle-related death* figures for WWI (the opposite of what one would expect)—cf. Singer-Small Table 4.2 and Sorokin Table 16—but if that had been true, it would have suggested that the two date sets cannot be combined.

[19] Levy, Table 4.1; Sorokin, 3:561, 550.

[20] I do not show data from before 1815, where the discrepancies are even larger but can be attributed in large part to Levy's use of "casualty" figures for "battle death" purposes. For example, Levy's figure for the Thirty Years War is 492% higher than Clodfelter's number for estimated battle deaths.

| | | | |
|---|---|---|---|
| Franco-Prussian War | 180,000 | 105,306 | 71% |
| Russo-Japanese War (1904-5) | 45,000 | 111,199 | 147% |
| Vietnam War | 56,000[21] (1965-73) | 1,033,686 (1966-73) | 1746 % |

When the data available are this problematic, it is time to accept what Marc Bloch called "the sternest duty of the savant," and to admit we do not know the truth with enough accuracy or precision to be able to draw valuable conclusions.[22]
If problems with counting procedures and unreliable data (especially before 1815) make it impossible to identify quantifiable patterns in the evolution of warfare on the grand chronological scale that Levy and Thompson attempt, *The Arc of War* nevertheless does successfully contextualize and draw attention to two recent changes that are so large that their magnitude and their discontinuity with the past overwhelms any doubts arising from the sorts of problems just noted: first, the huge escalation in the costs of war in the first half of the twentieth century; second, the equally remarkable decline in the frequency of Great Power war thereafter (a decline even more striking if we eliminate the dips in frequency Levy and Thompson show in the nineteenth century, which I argued above were the result of counting errors) Of course, most historians and political scientists are already well aware of the extraordinary character of the two World Wars and of the 'Long Peace' (among Great Powers) which has followed them.

Levy and Thompson's fifth and sixth arguments, developed in Chapters 6 and 7, deal with the "bifurcation" of warfare into two quite different patterns in the post-1945 period. The evolution of warfare over the last half-century is still an immense topic, but thanks to its smaller scale and better quality of available data it is at least relatively manageable. I found this section of the book the most interesting and valuable. The basic point is that what they call the "nonindustrialized" (17, 186, 208) states of Africa, Latin America, and the Middle East missed out on the competitive "escalatory spiral" (17-18, 208; 143-4, 186) described in Chapter 6-- by which European states, starting around 1500, evolved strong governments that effectively monopolized the use of violence within their territories through the medium of disciplined standing armies; developed and harnessed nationalism; and built up strong, industrialized economies capable of producing mass quantities of advanced weaponry. The same experiences, however, taught the European states (by 1945) that the increasing severity of industrialized inter-state war had raised its costs

[21] It seems that Levy's data, and hence Levy and Thompson's figure 1.4 (labeled "severity of great power war," with "severity" defined on page 7 "in terms of the number of casualties" or "in terms of the battle-related deaths" actually limits the numbers not to the count of casualties/deaths, but rather to the number of casualties/deaths *in the armies of the Great Powers*, not including military losses to other states also involved in the wars.

[22] Marc Bloch, *The Historian's Craft* (Manchester: Manchester U.P, 2004), 49-50.

beyond any gains that could reasonably be expected. Strong national communities made large-scale internal warfare equally unlikely. The combination of these two developments means that substantial zones of the globe have experienced an unprecedented freedom from the clash of armies for over 50 years, with good prospects for sustaining that peace into the future. The states that did not share the experiences of the third acceleration in warfare, however, now tend to be weaker, in terms of their ability to fight enemies both foreign and domestic. External weakness makes the new states of the "global south" less likely to fight high-intensity inter-state wars; internal weakness makes them less able to suppress rebellions or separatist movements, and so leads to more internal, low-intensity warfare. (16, 208, 215.)

Interesting though this analysis of the past half-century may be, however, *The Arc of War* ultimately does not succeed in demonstrating that the post-war period represents a sharp turn following a five-hundred-year inflection of a five-thousand-year-long "arc of war."

## Author's Response by Jack S. Levy, Rutgers University and William R. Thompson, Indiana University

In constructing a model of nearly ten millennia of human warfare in *The Arc of War*, we have engaged historians, anthropologists, archaeologists, evolutionary theorists, and scholars from other disciplines as well. Interdisciplinary work of this kind is intellectually exciting and offers many potential benefits, but it is also challenging and has many perils. Scholars in different disciplines use different concepts, theories, and methodologies. This creates both the possibility of broadening one's intellectual horizons and the potential for miscommunication. We see each in these H-Diplo reviews. It is rewarding to learn, for example, that R. Brian Ferguson, a leading figure in the anthropology of war, finds that our analysis of the origins of war is for the most part consistent with the evidence. The critiques of military historians Stephen Morillo and Clifford J. Rogers are less flattering.

We think the tenor of these latter responses is due in part to the different orientations of many (but not all) historians and political scientists. As Morillo notes in his review, there are "differences(s) between historians' particularist explanations of individual wars and political scientist's desire to explain wars in general."[1] Although Morillo concedes that generalized, model-based approaches offer the potential for cross-cultural comparisons, many of his critiques reflect a particularist standpoint and criticize our broader theoretical generalizations. The same is true of Rogers' review. Perhaps the gap between particularism and generalization is too great to overcome in certain areas, though we have certainly profited enormously form our interactions with diplomatic historians over the years, including some recent conferences and workshops. Whatever the case, we do not plan on abandoning our reliance on particularist studies. We rely both on the 'data' they provide to help shape our generalizations and test others, and on the analytic insights of historians (which, we might add, some political scientists tend to underemphasize). We had hoped that some of our interpretative errors – which surely must be made in any study like ours - might be corrected by scholars more familiar with specific time periods and places. We received some useful feedback of that nature from these reviews. We cannot respond to all of the criticisms raised in these reviews, but focus instead on what we regard as some of the most important ones and those raised in more than one review.

We begin with Ferguson's review. Given Ferguson's stature in the study of the anthropology of war, we are particularly pleased that he believes that our coverage of the anthropological literature is "well informed and even handed," and that the earliest evidence of warfare – an area in which he has long specialized – is compatible with our explanation of the origins of war. We are encouraged by his conclusion that our own treatment of the origins of war is consistent with the argument he lays out in his

[1] For a political scientist's take on this particularist/generalist divide, see Jack S. Levy, "Explaining Events and Testing Theories: History, Political Science, and the Analysis of International Relations," in Colin Elman and Miriam Fendius Elman, eds., *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations* (Cambridge: MIT Press, 2001), 39-83.

development of the theory of cultural materialism pioneered by Marvin Harris.[2] As he notes, however, we limit our attention in chapter 2 to the specific question of the origins of war, and then apply the same model to subsequent escalations and transformations in warfare, whereas Ferguson aims to construct a more general model of sociocultural change that subsumes an explanation for war. We agree with his comment that his approach probably would not work all that well in accounting for the long arc of war, and that our approach was not designed to explain the more general phenomena that he tries to explain. We benefited from Ferguson's further development of his cultural materialism approach in his essay, and his attempts to link it to parts of our conception of the evolution of warfare.

Ferguson concludes on a more critical note with respect to our argument about the link between weak states in the contemporary system and the frequency of internal war. We argue that the third acceleration in the co-evolution of warfare generated strong states among the survivors of a highly competitive interstate system, and weaker states that emerged much later in a less competitive system with stronger norms against territorial conquest. The strong states built massive military establishments, industrialized, and faced rapidly increasing costs of war against each other, which led to a decline in major power warfare. Weaker states had fewer resources to deal with domestic threats and were more prone to internal warfare. Ferguson argues that our argument overlooks the contribution of colonialism to non-western state weakness. He raises an interesting question: What are the relative explanatory contributions of transformative war experiences and imperial exploitation to contemporary state weaknesses, and to what extent are they independent phenomena? One might argue that the co-evolutionary changes in the third acceleration set up or at least greatly facilitated the emergence of the colonial regimes that followed.[3] This is a topic that deserves more consideration.

We now turn to Morillo's review. Although Morillo regards our chapter on the origins of war to be the strongest in the book, he says that it is not clear how that chapter connects to the rest of the project. Our intention was to account for the origins, escalation, and selective transformations of warfare. Each of these three foci requires different explanatory emphases but needs to be linked to a general overall model. Our most general model encompasses the co-evolution of war, weapons, military organization, political organization, political economy, and threat environment. To explain origins, we stress weapons, military organization, group segmentation (a form of very early political organization), political-economic change and scarcity, political-economic/ organizational expansion, and changes in threat environment. The fact that our model works reasonably well in explaining the origins of war as well as its subsequent escalation and transformation gives us additional confidence in the validity of the model.

[2] Marvin Harris, *Cultural Materialism: The Struggle for a Science of Culture* (New York: Random House, 2001).

[3] However, we do not see the link between military changes and colonialism as unqualified. See William R. Thompson, "The Military Superiority Thesis and the Ascendancy of Western Eurasia in the World System." *Journal of World History* 10 (March, 1999): 143-178.

One of Morillo's primary critiques of our book is that "we remove culture and cultural perceptions from the analysis." Although Morillo is correct to raise the issue of the causal role of culture, it is somewhat misleading to suggest that we eliminate culture from the argument. For one thing, our very definition of war as "sustained, coordinated violence between political organizations"[4] incorporates a cultural component, because one cannot talk about violence between political organizations without discussing the prior formation of group identities. This involves group segmentation, which has a substantial cultural component and which we emphasize (as Morillo notes). In addition, we contend that each of the six co-evolving factors has its own cultural components. For instance, very distinctive cultural elements are likely to develop within the context of the military organization category. Aztec warriors developed codes and belief systems that were markedly different from those developed by Japanese samurai, Greek hoplites, or Egyptian Mamluks.

We argue, however, that cultural elements need to be analytically subordinated (rather than eliminated) if we are to tackle the question of the evolution of warfare, and particularly warfare among the leading states in the system, over a ten-millennium span. Culture may have a greater impact on the causes of particular war, as many political scientists and others have argued. There are all empirical questions, and Morillo is right to raise them. Testing cultural hypotheses is often not easy, but it needs to be done. One specific point of criticism that Morillo raises concerns our argument that a prime factor underlying the decline of major state warfare in the late twentieth century is the enormously increasing costs of war due to the development of nuclear weapons and other advanced technologies. Morillo argues, to the contrary, that the true explanatory factor is "shifts in attitudes towards the costs and benefits of war." Though we concede that these ideational and cultural attitudes are important, we think that in this case they are to a significant extent endogenous to the changing material costs of war arising from nuclear weapons and other factors. The trick is to consider what kind of evidence is needed to adjudicate between these competing perspectives.

After complaining that we unfairly minimize the role of culture, Morillo then suggests that our arguments are somehow too complex and contingent to make any predictions. We have made a serious effort, however, to generate testable generalizations. Among those that we focus on most in this book are the contemporary bifurcation of states into relatively strong and weak categories and their differential propensities to types of warfare. We argue that warfare between industrial states has become less likely and that non-industrial states are more likely to become involved in intra-state warfare and less likely to engage in inter-state warfare. These eminently testable propositions are not novel, but they can be derived from our theoretical arguments and then tested against the evidence. Such tests should also apply the same standards in testing alternative explanations for the same phenomena, including Morillo's cultural arguments.

[4] We elaborate on the logic underlying the definition in Jack S. Levy and William R. Thompson, *Causes of War* (Chichester, UK: Wiley-Blackwell, 2010), 5-11.

Finally, Morillo concludes his critique with a long section on our designated accelerations of change in co-evolutionary warfare and what we refer to as the western military trajectory. Morillo states that our identifications of the first two accelerations (which he describes as "the creation of urban-based military forces in the third millennium BCE," and then the development of "administratively and tactically more complex armies and empires in the second half of the first millennium BCE") are plausible. He goes on to say that some of our specific interpretations about these accelerations are contestable. Morillo may be right, but he is not too specific about the contestable aspects of our first two accelerations, so it is rather difficult to respond.

Morillo is more explicit about the third military acceleration, which we argue pushed the western military trajectory ahead of the other trajectories. Our argument is that the acceleration began around 1500 CE and has persisted into the current era. Morillo contends that the acceleration only began after 1800 CE with the advent of industrialization. *The Arc of War* makes it clear that we appreciate the role of industrialization. We suggest, however, that a series of military revolutions in early modern Europe ushered in the third acceleration, which was then greatly affected by industrialization. Historians have debated these early modern military revolutions – largely in terms of which one was most or sufficiently revolutionary and where it started.[5] Many agree with Morillo, and many agree with us. We make no attempt in the book to resolve these debates about changes in European politics and warfare. We think, however, that these competing perspectives are testable in principle, and that further work on this debate would be useful.[6]

Curiously, Morillo conflates this disagreement about the timing of accelerations with a second disagreement about the western military trajectory. Our position is that the western military trajectory began in ancient Mesopotamia and Egypt, and moved across the Mediterranean to the Greeks and Romans, and then on to Europe and its former colony, the United States. This is an assertion on our part that is testable in its own right and one that we explore in various parts of the book. However, establishing spatial and temporal boundaries for the multiple military trajectories that we see in history was not one of our goals in this book. The term "western" is simply a geographical designation for a trajectory that we think started in southwest Asia, moved across the Mediterranean into Europe, and later moved across the Atlantic. How that happened, and the various twists along the way, including how the "medieval" Middle East broke away with slave soldiers, is an interesting story and one that deserves its own book.

[5] Clifford J. Rogers (ed.), *The Military Revolution Debate* (Boulder, CO: Westview, 1995).

[6] For additional empirical evidence on this question, see William R. Thompson and Karen Rasler, "War, the Military Revolution(s) Controversy, and Army Expansion: A Test of Two Explanations of Historical Influences on European State Making." *Comparative Political Studies* 32 (February, 1999): 3-31.

Regardless of whether this assertion of a long trajectory makes sense or not, it is not clear how it matters all that much to the more important assertions about the contemporary differentiation of military and political power in western and non-western trajectories. The third acceleration, whether it commenced in 1500 or 1800, was centered initially in western Europe and later in North America. The main question is not whether the Sumerians were linked to the Greeks, who, in turn, were linked to the changes in European infantry practices after 1500. The main question is whether, or to what extent, the third acceleration is primarily responsible for the wide disparities in political-military and economic power today.

There is some overlap on a number of points between Rogers' take on the book and Morillo's, but their criticisms are different. Rogers' position on the origins of war is straightforward. Since we do not know exactly where or when war started, we should not speculate about that question, and it would have been better for us to skip it altogether. Rogers raises some important issues, not only for our own work but of an entire field of study on the origins of war that cuts across several disciplines, but his argument about ignoring the question of the origins of war goes too far. While it is true that we cannot pinpoint the first event that we might all agree was war-like, Rogers overstates our lack of information. Up to a certain point in time, there is little, if any, evidence of war-like behavior. After a certain point in time (which varies by region), the evidence becomes stronger, as presumably does the propensity to engage in warfare. We can develop a plausible model of war origins even if we may never be in a position to fully test it, especially if the model generates other observable implications that can be tested for other periods, as our model does. Our intention was to tell a theoretical story that was reasonably consistent theoretically across origins, escalation, and selective transformations/terminations. It would have been awkward to begin with escalation with no discussion of possible origins.

Whereas Morillo rejected our co-evolutionary model because it does not stress culture enough, Rogers rejects it because he thinks it is so general that, in essence, it does not generate observable implications that can be tested. If the argument is that six spheres of activity co-evolve in the sense that a significant change in one tends to lead to significant changes in the other spheres, Rogers concludes that the rival argument is that changes in the six spheres are entirely independent. Since that much independence is unlikely, co-evolution must be accurate but meaningless because it is the only possibility. We find this to be a peculiar interpretation, and we disagree with the contention that it does not generate observable predictions. In fact, Ferguson, Morillo, and Rogers each found plenty of specific predictions they concluded were inconsistent with the historical evidence.

It is also important to note that for us the model is a starting point, and only one of several theoretical arguments that we advance. We think it is a useful starting point because the model 1) suggests that other spheres of activity (for instance, climate, demography, or personalities) are less likely to be responsible for the changes in which we are most interested; 2) allows for equifinality – that is, changes can begin in any of the six spheres in different places and different times and still lead to major transformations; 3) potentially standardizes how we tell change stories in different times and places without falling back on proper place names as part of the explanation; and, as noted, (4) generates hypotheses that can be tested across different historical periods.

Rogers also suggests that our identification of three periods of accelerated change implies that there was no change in non-periods of acceleration. This raises the important point of the relationship between revolutionary and evolutionary change, which we grappled with in the book. We emphasize both revolutionary changes within accelerations and changes of lesser magnitudes between those accelerations, much like Rogers' own "punctuated equilibrium" model.[7] There are important differences, however, between Roger's punctuated equilibrium model and our co-evolutionary model. It would be useful to further specify these differences theoretically and then test these alternative explanations of the evolution of warfare over an appropriate temporal period.

Another issue raised by Rogers concerns the relationship between our arguments about the three accelerations in the intensity of warfare and our argument that political-economy is probably the strongest driver among the six components of our co-evolutionary model. We think that Rogers mistakenly confounds these two distinct arguments. Our first two accelerations took place in the agrarian era and the third began (in our view) prior to the advent of industrialization. Thus we do not set the timing of the accelerations to correspond to transitions from hunting-gathering to agrarian to industrial eras. As noted earlier, our third acceleration was profoundly affected by industrialization, but that acceleration began before the British industrial revolution in the eighteenth century.

Rogers then takes on our argument that the increasing costs of war have led to the diminishing frequency and probability of great power war. He characterizes our argument as saying that "from the dawn of history to the mid-twentieth century [there] has been a steady increase in the cost and severity of war… which from the fifteenth century CE onward led to a steady decrease in the frequency of war between Great Powers." This is misleading, though it depends on what one means by "steady" increases and decline. There have certainly not been monotonic increases, but instead some fluctuations around increasing and decreasing trends. If one looks at the trends by century, the results are clear for the declining frequency of great power war: sixteenth century, 25 wars; seventeenth century, 17 wars, eighteenth century, 11 wars; nineteenth century, 3 wars; twentieth century, 5 wars. This argument about the decline of major war is also fairly standard in the field, with most of the debate not about the descriptive accuracy of the statement but about how best to explain those trends.[8] Here and elsewhere in his essay, Rogers raises some important issues regarding various issues of measurement of wars, their frequency, and

[7] Clifford J. Rogers, "The Military Revolutions of the Hundred Years War," in Rogers, *The Military Revolution Debate*, 55-93.

[8] The debate goes back many years but was reignited by the publication of Steven Pinker, *The Better Angels of Our Nature: Why Violence Has Declined* (New York: Viking, 2011). For an exchange see Nils Petter Gleditsch, Steven Pinker, Bradley A. Thayer, Jack S. Levy, and William R. Thompson, "The Decline of War," *International Studies Review* 15, 3 (September 2013).

their severity. We have addressed these issues elsewhere, and those issues have received extensive treatment in the literature, and we refer the reader to those sources.[9]

Developing a model of the evolution of warfare over the last ten millennia, and engaging scholars working in their areas of expertise in a number of different disciplines, has been a daunting task. We anticipated that we would receive a fair amount of criticism but hoped that we could learn from it. We plan to incorporate or more fully respond to many of these criticisms the next time that we do work in this area.

**Copyright © 2013 H-Net: Humanities and Social Sciences Online.** H-Net permits the redistribution and reprinting of this work for nonprofit, educational purposes, with full and accurate attribution to the author, web location, date of publication, H-Diplo, and H-Net: Humanities & Social Sciences Online. For any other proposed use, contact the H-Diplo Editors at h-diplo@h-net.msu.edu.

[9] Jack Levy discusses some problems involved in the identification of war and the measurement of the frequency and severity of war in *War in the Modern Great Power System, 1495-1975* (Lexington, KY: University Press of Kentucky, 1983); and "Analytic Problems in the Identification of Wars." *International Interactions,* 14, 2 (1988): 181-186. His datasets have been updated over time in response to new information. See also the excellent discussion of measurement issues in Pinker, *Better Angels*. It is worth noting that Pinker uses, as we do in *The Arc of War*, the absolute number of battle deaths as an indicator of the severity of war. Morillo is highly critical of this practice. Each side of the debate is well represented in the literature.