

A Roundtable on Grant Madsen, *Sovereign Soldiers: How the U.S. Military Transformed the Global Economy After World War II*

Laura Hein, Michael J. Hogan, Aaron O'Connell, Carolyn Eisenberg, Curt Cardwell,
and Grant Madsen

Introduction, Roundtable review of Grant Madsen, *Sovereign Soldiers: How the U.S. Military Transformed the Global Economy after World War II* (Philadelphia: University of Pennsylvania Press, 2018)

Laura Hein

I conduct this exercise by concluding that the format is a case of “be careful of what you wish for ...” All authors want their work to be taken seriously and all journal readers prefer lively, engaged material. You, dear readers, will be satisfied on both accounts. On the other hand, assembling five very well-informed reviewers to train their full attention—publicly—on a younger scholar’s first concerted effort is a lot of muscle. I suspect I am not the only member of the firing squad to be glad that no sly editor offered me this devil’s bargain of an opportunity back when my first book appeared.

Grant Madsen started with a really great question: How and in what ways did military leadership of American occupation governments affect economic policy? His thesis was that a coherent economic policy emerged when U.S. military governments led by Army generals Dwight D. Eisenhower, Douglas MacArthur, and Lucius Clay remade the economies of various territories under their command. Further, they applied those lessons to the postwar American and global economies during the Eisenhower administration. But then, his own research suggested that the actual answer was, “it depends on too much else to attribute over-all economic policy, let alone economic outcomes, to these actors.” Good researchers frequently find themselves in this position, and writing one’s way out of it is one of the hardest tasks I know. As his response to the other four reviewers shows, Madsen’s ideas are still evolving, but the book itself reveals a scholar who had not fully integrated the implications of the research with the framing argument when it went to press. To cut to the chase, this book does a number of interesting things but does not succeed in its main goals.

Madsen was tripped up by three general problems, all flagged by the four reviewers in different ways: Who are the key actors? What is the relationship between economic theory and economic policy? What role does politics play in this narrative and its reception?

The book begins by introducing Eisenhower, MacArthur, and Clay as the crucial figures around whom the book will be structured. They are famous and powerful men, they operated in contexts where their orders were frequently obeyed, and they had long and impressive careers. Nonetheless, they just don’t shoulder the task that

Madsen assigned to them. First of all, they don’t work as a set. Although all three were U.S. generals, they shared little else in terms of temperament, management style, assumptions about the foreigners under their control, or even priorities regarding U.S. national security after 1945. MacArthur is particularly intractable when it comes to arguing that he shaped Japan’s economy, a subject he found of little interest. His own pronouncements tended to stress things like “eliminating feudalism,” which functioned more as talking point than as a policy blueprint.

Second, all three men quickly delegated economic planning to their economic advisors, who were not career military men. This is what irks Aaron O’Connell, who, after reading the first 60 pages, expected the book to show how specific economic ideas or policies were connected to the experience of military leadership—of running occupied territories rather than waging war. I shared O’Connell’s experience of misdirection and his reaction that Madsen’s question would have been better answered by looking at fully military-run governments, such as in Haiti, rather than the occupations of Germany and Japan, which were staffed at the upper levels largely by men who had joined the war effort after substantial civilian careers. Indeed, Madsen would not have needed to travel far: the Army was running its own shows in Korea from 1945 until 1948 and in Okinawa until 1972 with far less civilian input than in neighboring Japan. Neither episode was known for good economic management, however, suggesting that career military men, as they repeatedly say in this book, were poorly trained for that task.¹ I would add that both Madsen and O’Connell are a bit too quick to assume that Big Men who stood atop a chain of command had the most effect on outcomes. That may be why Madsen did not reframe his book by introducing his key civilian actors at the outset together with his military ones, which would have better helped his readers understand his true focus.

In fact, my own work argues that the key players were often not Americans at all but included economists among the Germans and Japanese whom they ostensibly governed, a claim substantiated but not highlighted in the book. There were profound policy disagreements but they usually did not line up as Japanese versus American views. Takemae Eiji, Mark Metzler, Aiko Ikeo, Scott O’Byrne, Tsuru Shigeto, and W. Elliott Brownlee join me in pointing out that the Americans interacted with Japanese experts throughout their years there and that all of them experienced those engagements as deeply collaborative, although sometimes also frustrating.² Some of these Japanese individuals are mentioned in Madsen’s tale, as are several German planners, but only episodically. Madsen is clearly developing

this point when he somewhat aspirationally states in his “Response to Reviewers” that “I claim that a group of non-Nazi German economists ultimately provided the basic blueprint that became that [transnational postwar] policy regime.” The book does not do so, but perhaps his future work will.

I share Curt Cardwell’s pleasure that Madsen and his cohort are returning to the study of political economies. Madsen focuses on currency conversion, inflation control, banking, and taxation, all truly important topics. He sheds his coyness about why in his Response—this is a critique of Keynesianism—which answers Carolyn Eisenberg’s question of why he ignores equally important aspects of the economy, such as industrial relations, decartelization, and reparations. (The book is most explicit on this in the middle of Chapter 11.) Yet, here too Madsen’s facts don’t really run in parallel with the argument. As Cardwell explains, Madsen’s actors were operating in a larger context that was profoundly Keynesian, as the U.S. government poured resources into the Marshall Plan, domestic and international military spending, and highways. Madsen disagrees but incoherently.

Separately, I think the chief reason that none of us found Madsen’s economic arguments persuasive is that he focused on theory *per se* when the historical debate was actually about what was happening in the real world—that is, how to *apply* theory. All economists, including Keynes himself, see inflation under most conditions as a problem and all of them know that fighting wars encourages governments to cause excess inflation. (Keynes first made his name analyzing the inflationary effect of German post-World War I reparations.) The challenge postwar policymakers faced in Germany, Japan, and the United States was not whether inflation was bad in some abstract sense but whether the tangle of problems they faced meant that it should be tolerated a little longer to meet other economic goals. Similarly, Keynesian and neo-classical economists alike share the theoretical assumption that economies grow when firms invest in their workers and in new technology, they just differ on the policy question of whether tax cuts are the best way to encourage firms to make that investment. Madsen’s periodic forays into the musings of individual economists would more effectively advance his arguments if he had more often included their assessments of actual economies in specific times and places. His Response begins to do so but only by changing the subject to preparation for international trade, a topic that is almost invisible in the book itself.

Madsen very usefully makes clear the vast distance between fiscal conservatism of the 1950s and either the Tea Party or Trumpist conservatisms of today, as well as his own admiration for the former. In the 1945-55 decade, it was impossible to argue that the U.S. government was an inherently incompetent economic actor, given its recent victory in an enormous multi-front war. I believe Madsen when he tells us that his protagonists became less attracted to interventions such as price and wage controls over time (although Richard Nixon still supported them in the 1970s), but Madsen reads an ideological critique into Hogan’s comments that strikes me as paranoid and, in the claim that Hogan paints Germans as passive, incompatible with Hogan’s own scholarship. Frankly, I read that review as a determined attempt to be kind by using most of his real estate to summarize (quite usefully) Madsen’s book and by critiquing it primarily as too ambitious rather than internally incoherent, polemical, or misleading.

Notes:

1. Bruce Cumings, *The Origins of the Korean War: Liberation and the Emergence of Separate Regimes* (Princeton, 1981); and Yoshida, Kensei, *Democracy Betrayed: Okinawa Under U.S. Occupation* (Bellingham, WA, 2001).

2. Laura Hein, *Fueling Growth: the Energy Revolution and Economic Policy in Postwar Japan* (Cambridge, MA, 1990); Hein, *Reasonable Men, Powerful Words: Political Culture and Expertise in 20th Century Japan* (Berkeley, CA, 2004); Mark Metzler, *Capital as Will and Imagination: Schumpeter’s Guide to the Postwar Japanese Miracle* (Ithaca, NY, 2013); Aiko Ikeo, *The American Economist Martin Bronfenbrenner (1914–1997) and the Reconstruction of the Japanese Economy (1947–1952)*, CHOPE Working Paper 2011–11 (Durham NC: CHOPE, June 2011). Available at: <http://hope.econ.duke.edu/sites/default/files/Ikeo20110616c-Letter-BronfenbrennerJapan.pdf>, accessed November 4, 2018; Scott O’Byrne, *The Growth Idea: Purpose and Prosperity in Postwar Japan* (Honolulu, HI, 2009); Tsuru Shigeto, *Japan’s Capitalism: Creative Defeat and Beyond* (Cambridge, 1993); and Laura Hein and Mark Metzler, “Raising Taxes for Democracy: The Japanese Policy Environment of the Shoup Mission,” and W. Elliot Brownlee, “Tax Reform during the American Occupation of Japan: Who Killed Shoup” in W. Elliot Brownlee, Ide Eisaku, and Fukagai Yasunori, eds., *The Political Economy of Transnational Tax Reform: The Shoup Mission to Japan in Historical Context* (Cambridge, 2013).

Review of Grant Madsen, *Sovereign Soldiers: How the U.S. Military Transformed the Global Economy After World War II*

Michael J. Hogan

Grant Madsen’s interesting book provides the reader with several different, though related, stories. He begins in the Philippines, with a brief look at the American occupation after the Spanish-American War through the Organic Act of 1902 and the Jones Act of 1916. During this time, the Philippines had no prospect of statehood in the American union; nor did Filipinos have any hope of American citizenship. They were promised eventual independence, to be sure, and came to enjoy many of the rights, privileges, and protections enjoyed by American citizens under the Constitution. They could pass their own laws, for example, and negotiate their own treaties.

The exercise of such rights, however, was subject to American review and approval, creating what Madsen, borrowing from Robert Latham, calls an American “external state.” By this he means a set of governing institutions, basically military institutions, tied to but functioning outside the United States and used to govern non-American people. The external state in this case administered the American occupation of the Philippines. From there, the new state would be transplanted to Panama, Haiti, Nicaragua, Mexico, and elsewhere, including Japan and Germany, to which Madsen devotes much of his attention.

The second story is something of a collective biography of the officials, most of them Army officers, who administered the American external state, both from Washington and in the occupied areas. Here Madsen focuses largely on Douglas MacArthur, Lucius Clay, and Dwight Eisenhower, as well as some of their key associates, especially the Detroit banker Joseph Dodge and General William Draper. He traces the careers of these men and the lessons they learned as they marched from assignment to assignment across the map of the American external state, from World War I through the interwar years.

We learn, for example, of their disappointment with the rapid postwar reduction of American force levels and defense spending, which led the army to streamline its command structure but left it unprepared for the next war. The War Industries Board notwithstanding, they did not think the United States had an effective war-time plan for controlling and coordinating industry. Without a plan, the American economy underperformed its potential in World War I. It seldom produced what mattered most for the war effort, suffered from inflation, and, when the figures were adjusted for inflation, experienced no real growth in the

years between 1914 and 1920.

Finally, these men agreed with Wilson's vision of postwar international cooperation and free trade, but they condemned his position on war debts and reparations, which led inevitably to economic collapse, depression, and war. From their limited experience as an occupying force in the Rhineland, and their postwar duties in the Philippines and Panama, they saw the benefit of empowering local governments, so far as possible, and the need to stimulate production and trade, balance budgets, and control inflation by means other than government regulation.

As these views suggest, the lessons they learned from wartime mobilization and their experiences with the American external state drove MacArthur, Eisenhower, Clay, and their associates away from key elements of the Keynesian approach to economic recovery that became so fashionable in the 1930s, toward a more conservative strategy. Although both sides saw free and fair trade as essential to global growth and stability, Keynesians were more anxious to promote political and social as well as economic reforms in the occupied areas, more tolerant of deficit spending to prime the pump of the economy, and more inclined to use wage and price controls to tame inflationary pressures—all of which were at odds with the monetary and fiscal strategies favored by Clay, Eisenhower, and other more conservative policymakers. These and related differences came to a head during the occupation of Germany and Japan after World War II, which is the third part of Madsen's multistoried narrative.

In Germany, for example, these policymakers rejected a strategy that put political and social reforms before economic recovery. This had been Woodrow Wilson's approach after the First World War. He emphasized smashing the large German industrial conglomerates and, if necessary, averting starvation and civil unrest through deficit spending and economic controls to dampen inflation. It was also the approach embedded in the Morgenthau Plan and JCS 1067, which emphasized the deindustrialization of Germany and the decentralization and denazification of its political and economic structures.

Clay and his colleagues wanted instead to stimulate long-term economic growth and stability, even if it meant slowing or suspending denazification, working with existing German authorities, and letting decentralization take a back seat to restarting German industry. Most important, they wanted to control inflation, devalue Germany's inflated currency, balance its budget, and limit the power of German trade unions, even if these and other reforms had an inequitable impact on certain elements of the German population. They believed this was the route to real economic recovery and freedom, as opposed to a Keynesian strategy, which sought political justice ahead of economic stability, they said, but ended inevitably in deficit spending, inflation, crushing taxes, price controls, rationing, and other state encroachments that would lead inexorably to economic regimentation and the end of private enterprise and democracy.

They followed this course not only in Germany, but also in Western Europe as a whole. The Marshall Plan, according to Madsen, started in Germany. He stresses that the plan's emphasis on economic integration across Western Europe, the revival of free trade, the stabilization of currencies, the elimination of economic controls and deficit financing in favor of balanced budgets, and other aspects of American policy had all been field tested in Germany and, before that, in experiments in the American external state. What is more, they were being applied at the same time in the American occupation of Japan.

Initially, at least, the occupation of Japan was run by Keynesian New Dealers under the leadership of Douglas MacArthur, whose primary emphasis was on political reforms rather than economic recovery. Not surprisingly,

the New Dealers were hostile to industrial monopolies, which they saw as the principal source of militarism in Japan, as they had been in prewar Germany. They wanted to break them up and at the same time promote a variety of reforms, including women's suffrage, freedom for political prisoners, support for labor unions, and an end to child labor, all while fighting starvation and supporting high levels of employment. To accomplish these goals, they were willing to tolerate budget deficits, inflation, and economic controls.

Things began to change when William Draper became undersecretary of the army. With authority over all occupation policy, he quickly began to apply the lessons he and Clay had learned in Germany to the occupation of Japan. Specifically, he put earlier efforts to break up Japanese monopolies on the back burner, convinced that they created uncertainty and slowed industrial revival. Most important, he turned to more conservative fiscal and monetary policies, as he believed that New Deal reforms and Keynesian policies had fueled inflation. He slowed the pace of reform and urged austerity. He put limits on credit and restraints on labor, eliminated government subsidies, and pushed for faster progress toward a balanced budget. The Japanese, he said, had to depend less on American aid and inflationary government spending to deal with shortages of all sorts, including food shortages, and earn their own way through the revival of Japanese trade.

These instructions would also guide Joseph Dodge, another veteran of the German occupation, whom Draper dispatched to Japan as the U.S. minister. Dodge was to function more or less as an economic czar in this corner of America's external state. Under his direction, Japan reduced taxes, curbed government spending, and eliminated price and wage controls. According to Madsen, the new approach worked. Over time wages began to increase, unemployment declined, inflation dropped, and budget deficits moved toward surpluses. At the same time, however, labor suffered. About 126,000 railway workers lost their jobs, and government employment fell by another half million.

Given these and other shortfalls, it is not surprising that Dodge's reforms remained unpopular with the Japanese government, labor unions, and the press. Even the American State Department complained that economic gains were minimal and hardships substantial, and they warned that economic and political collapse was inevitable if Dodge's reforms were not reversed. That is exactly what happened when Matthew Ridgway replaced MacArthur as head of the American occupation and when the outbreak of the Korean war led American officials to prize Japanese loyalty over Dodge's conservative economic policies. The irony, according to Madsen, is that Dodge's reforms were actually succeeding: exports were climbing, prices were stabilizing, inflation was declining. Even after much of Dodge's work had been undone, the Japanese government still embraced the idea of a balanced budget and avoided any increase in the national debt for years to come, even as it once again pumped additional funds into the economy in order to spur growth and higher levels of employment.

Having covered this story, Madsen starts over with a fourth narrative, turning his attention back to political and economic policy in the United States through the Eisenhower administration. During his years as chief of staff and NATO commander, Eisenhower, as noted earlier, was moving away from Keynesian theory as the best way to balance defense spending against domestic social and welfare programs, while the New and Fair Deals moved toward it. Truman's management of the economy during the Korean War reinforced his thinking. Truman signed off on the massive military buildup envisioned in NSC 68 and financed the expansion through a frankly Keynesian strategy that accepted large budget deficits. New debt,

piled on top of debt accumulated during World War II and financed by buying government bonds at suppressed prices, pumped too much money into the economy, caused inflation, and led to wage and price controls that, as Eisenhower saw it, threatened private enterprise and the loss of economic liberties.

When he became president, Eisenhower sought a new balance that would pair economic with military strength. Applying the lessons he learned in Europe from the German occupation and the Marshall Plan, he aimed to curb inflation, balance the budget, reduce taxes, and put an end to wage and price controls, all while protecting key social programs inherited from his Democratic Party predecessors. To achieve these goals he had to overcome strong opposition from conservatives in his own party who wanted deeper and faster cuts in wartime taxes, much less spending on both defense and domestic programs, and a balanced budget as soon as possible. Ike's answer was the New Look, which envisioned a capital-intensive strategy of defense that relied less on expensive manpower and more on airpower and nuclear weapons to guarantee the nation's security. This strategy, together with the end of the Korean War, allowed Eisenhower to curb the growth of military spending, which in turned cleared a path toward a balanced budget and a modest reduction in taxes.

Despite the recessions that began in 1953 and 1957, Madsen considers Eisenhower's program a success. It succeeded in large part because the president was not a conservative ideologue. He favored limited tax reforms over massive tax cuts and the New Look over the strategy of "flexible response," which took hold under President Kennedy in the 1960s. He was also willing to protect, and even slightly grow, New Deal social and jobs programs, like social security and the Highway Act of 1956. His greatest achievement was slowing the pace of defense spending, which Madsen describes as reversing the Keynesian strategies of the past, and making progress toward a balanced budget, not locking one in place.

However brief this summation, it gives the reader a good sense of the sweep of Madsen's account. Its breadth is commendable, but it is also something of a problem. Consider, for example, his treatment of the American "external state." He is right to suggest at one point that a comprehensive exploration of this concept, built on the many fine works that have already covered the whole of the American experience as an occupying power, would make an exceptional contribution to the literature. However, his narrative dips into concrete experiences only here and there. The result is a somewhat sketchy account that is limited by its focus on just a few of the policies and policymakers involved. Often, for example, he notes only that certain individuals built bridges, roadways, and harbors during their stints in the Philippines or in Panama or Germany after World War I. Even his more detailed treatment of the German and Japanese occupations is limited, as are his efforts to tie German occupation policy to the Marshall Plan (although there are definite connections).

In short, while Madsen examines some episodes in more detail than others, no episode is treated in depth. And nowhere is it clear that conservative fiscal and monetary policies alone accounted for whatever success the Americans enjoyed. In Japan, for example, he has to concede that Dodge fell short of his objectives and his gains were largely reversed. Something similar can be said about Madsen's collective biographies. For example, his treatment ignores important differences between the military leaders involved. In Japan, to cite just one case, MacArthur seemed quite comfortable with the New Dealers who ran the

occupation in its early years, before he switched his loyalties to Dodge, for reasons that are not fully explained. Most important, it is impossible to gauge the influence of these conservatives without measuring it against that of other American officials, in Washington and elsewhere, who also played central roles in the occupation. In sum, Madsen may have tried to do too much, or too much too quickly.

What is more, Madsen often makes claims that are hard to defend. This is the case with his claim that Dodge's hard line succeeded in Japan, despite the dislocations, unemployment, and protests it provoked, its repudiation by the State Department, and its reversal by the Japanese government. To cite another example, his effort to distinguish Truman's thinking from Eisenhower's is forced. Until the Korean war, Truman had worked day and night to combat pressure for increased defense spending coming from his service chiefs and hawks in his administration. He defended the goal of a balanced budget and drove defense spending down from the record levels achieved in World War II. Even after the outbreak of the Korean war, Leon Keyserling and other Keynesians notwithstanding, he refused to spend as much as many of his military advisers wanted and tried to drive the budget backwards as the war began to turn. When it came to military budgets, in other words, he and Eisenhower had more in common than Madsen seems to realize, including a shared fear of turning the United States into a garrison state.

In truth, Madsen's account raises doubts about how committed Eisenhower was to some of the conservative strategies the author appears to celebrate. He admits that Eisenhower never took conservative economic policies as far as Robert Taft and congressional conservatives wanted, even though he resisted the drift toward Keynesian solutions. Among other things, Ike lived with budget deficits when he had to; supported the interstate highway program, which was basically a jobs program; limited tax cuts; and endorsed new funding for social security. Madsen may not see Eisenhower as a Keynesian liberal, but neither does he see him as a conservative ideologue of the sort we are all too familiar with today.

If space permitted, it would be possible to mount a more thorough challenge to Madsen's celebration of the conservative monetary and fiscal policies that he attributes to Eisenhower, Clay, MacArthur, and other veterans of the American external state, and that he sees as succeeding, more or less by themselves, in rebuilding Japan, Germany, and all of Western Europe. But I would rather close on a more upbeat note. Despite the limits of his analysis, not to mention his somewhat hyperbolic title, Madsen attempts a big book. It covers a serious subject—the external state, as he calls it—over a long period of time; deals with difficult and complicated economic issues of the sort that most diplomatic historians choose to avoid; advances a conservative interpretation in a scholarly field not known for its conservative views; and is clearly written and based on solid research.

Eisenhower's insistence on moderation may have been the key to his success and may be the main reason why he is more or less ignored by Donald Trump's Republican Party. He had his convictions, of course, but he could live with reasonable compromise and had a pragmatic approach to policymaking, as Madsen shows in his discussion of the Highway Act of 1956. When all is said and done, it seems clear that Madsen really means to celebrate Eisenhower himself more than any particular approach to economic policy. He is what we used to call an "Eisenhower revisionist."

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Review of Grant Madsen, *Sovereign Soldiers: How the U.S. Military Transformed the Global Economy after World War II*

Aaron O'Connell

It is always painful to review a first book negatively, particularly a book that emerged from a doctoral dissertation. Who among us does not remember the years of research, the search for a meaningful argument, the competition between themes and theses, and the advice from committee members (which sometimes exacerbates rather than settles the competition between themes and arguments)? We were all that student once, and our memories of those difficult years might tempt us to be more charitable than we should be when holding our graduate students to high standards.

Writing bad reviews is even harder with books that span more than one subfield, as this one does by moving between economic history and military history. I know nothing about the former, but something about the latter. After two careful readings, I must conclude that *Sovereign Soldiers* may be very good economic history—that is for others to judge—but as military history, it falls far short of the mark.

Sometimes a book title promises more than it can deliver, and with this book, the promise came in the subtitle. Before reading *Sovereign Soldiers*, I had read a number of books and articles on the U.S. occupations of Germany and Japan, but I had encountered no work that argued—or even suggested—that the economic policies developed in occupied Germany or Japan came home to the United States, let alone “transformed the global economy.” As someone who is always interested in the hidden military origins of everyday items, I was a bit cowed by the book’s focus on economics, but decided to risk it anyway. If the broad outlines of today’s economic common sense—an insistence on low inflation, readily available credit, and (until recently) free trade, all managed by an interventionist federal government—had military origins, then I wanted to know about it. The book’s angle seemed fascinating; the arguments seemed timely; and the entire project looked impressive, important, and ambitious.

In fact, it was far too ambitious. Worse still, I think Madsen was not served well by those who steered the project to publication, because the final product is poorly organized, unfocused, and in need of a good editor. There are no problems with the prose, which is clear and direct, but there are numerous issues with basic organization, argument, chapter structure, evidence, and even a few problems with footnotes. These problems make it impossible to review *Sovereign Soldiers* positively.

The issues begin in the introduction with the author’s explanation of the book’s purposes, which diverge markedly from the argument promised in the title. Madsen explains that *Sovereign Soldiers* has three goals. First, it hopes to offer “an institutional history of military government starting after the Spanish American War” to explain “how the army found itself capable of governing a foreign people” after World War II (2). Second, it chronicles the “intellectual history of the political economy that military governments created during the occupations of Germany and Japan” (2). Third, it explains how the economic lessons learned in the occupations “came to dominate not only postwar Germany and Japan, but ultimately the United States in the 1950s” (3). All of this will be revealed, readers are told, by a careful study of the military careers of the generals who ran the

occupations: Douglas MacArthur in Japan, and Dwight Eisenhower and Lucius Clay in Germany.

Madsen makes it clear that he will not discuss every U.S. occupation; rather, he will focus only on the army and on the people and the moments that “link military government as an institution with the economic policy that came out of military government and returned to the United States in the middle decades of the twentieth century” (4). This seems a narrow scope for a book that promises an institutional history of military government, particularly since some of the Navy’s and Marine Corps’ occupations—Haiti (1915–1933), the Dominican Republic (1916–1924), and Nicaragua (1926–1933)—were some of the longest in U.S. history. But fair enough. Surely the Army learned specific lessons in the Philippines, the Rhineland, and the Panama Canal Zone. Perhaps those lessons made their way forward to World War II. Perhaps they even came home to the Eisenhower administration and had some effect on the world economy thereafter.

If so, we never learn what they were. Instead of a focused history of U.S. military government in three countries (or even the economic components of those occupations), readers get a jumble of different topics: summaries of the 1898 Battle of Manila, the Root reforms in the U.S. Army, logistics failures in World War I, and a number of other matters that have nothing to do with military government or economic policy. Nowhere are we told what the specific military missions were in the Philippines, Panama, or the Rhineland (and this omission is important, because the missions weren’t all the same). We

never learn how many soldiers served in the occupations, what they did there, how the occupations were organized or led, or whether the missions succeeded or failed according to the goals set by the commanders and the president.

Interspersed throughout the book are the outlines of MacArthur’s, Eisenhower’s, and Clay’s professional biographies, but there is very little new information here. And instead of serving as a vehicle for the argument, the biographical sketches are at best colorful vignettes, and at worst, major distractions. In the early chapters, they cover the officers’ time at West Point, their first assignments, Eisenhower’s early interest in tanks, his work on the Army Battle Monuments Commission, the influence of General Fox Conner, Eisenhower’s time at the Army Command and Staff College, and his strained relationship with MacArthur in the Philippines. There is occasional mention of how each officer understood economics (really just Eisenhower), but this information comes mostly from memoirs and biographies, and precious little of it seems to rise above commonsense counsel about the dangers of rampant inflation or massive debt.

A quick tour through chapter 3, “The Army in a Time of Depression,” reveals the book’s organizational problems and basic lack of focus. The chapter begins with seven pages on the 1920s, Versailles debt, the 1929 crash, and the ensuing depression, with short explanations of how economists interpreted these events as they happened. This segues into a narration of Roosevelt’s 1932 election victory and Army Chief of Staff MacArthur’s spats with the president, neither of which concerns economics or occupation policy in any real sense.

Five pages later, MacArthur and Eisenhower are off to the Philippines, with Lucius Clay soon to follow. But of course, by 1935, the Philippines were not under U.S. military government at all; the country had a Filipino constitution and a Filipino president with broad executive authority, as well as a Filipino-led national assembly and Filipino-led

supreme court. General MacArthur served as the military advisor to President Manuel Quezon, and Eisenhower had important duties—namely, to help the Philippines build a thirty-division army—but none of this can be properly called military government.

It is admittedly fascinating that three future leaders of postwar occupations all lived in the same hotel in Manila, but the only lessons Madsen captures from their time together concerns personality: MacArthur was aggressive, confrontational and willing to accept enormous risk; Eisenhower was practical and attuned to means and ends; Clay preferred Eisenhower to MacArthur and liked building dams. All of this is interesting, but none of it coheres into an argument about the army in the Depression or how the Philippines experience shaped the thinking of these future leaders in uniform or as civilians.

Chapter 4 is titled “The Army, the New Deal, and the planning for the Postwar,” and here, Madsen turns to the economic theories that would influence the postwar occupations. But again, there is a lack of focus, and the titles are misleading. We get eight pages on Keynesianism, Bretton Woods, and Secretary of State Cordell Hull’s thoughts on the relationship between economics and security, which segues into a discussion of Treasury Secretary Hans Morgenthau’s plans for de-industrializing Germany. Generals Eisenhower and Clay don’t enter the story until three pages before the chapter’s end, and they get a total of two paragraphs, most of which is just a summary of a conversation with Secretary Morgenthau. The army doesn’t come into the chapter at all.

From a military history perspective, chapter 4’s failure to address how the army prepared for postwar occupations is perhaps the book’s biggest omission, because there is a history here and it is one of the reasons the post-WWII occupations succeeded and later ones in Iraq and Afghanistan did not. In 1942, the army established the School of Military Government (SOMG) in Charlottesville, Virginia, in order to train officers in the precise subject of Madsen’s book: running foreign occupations. The SOMG took civil servants and businessmen from civilian life, offered them direct commissions in the army, and put them through months of coursework in preparation for overseas service.¹

When the need for occupation officials grew after the Italian surrender, the army also launched the Civil Affairs Training Program (CATP) – a separate course of study for officers running military occupations, which was hosted by ten universities around the country (including the University of Chicago—Madsen’s own alma mater). Much like the School of Military Government, the CATP drew on existing faculties from the humanities, area studies, and social sciences—including economists—and gave military officers a four-month-long orientation on the history and culture of the countries they would later be deployed to, ten hours of which were devoted to economics.² By mid-1943, the CATP was graduating 450 officers per month.

The thousands of officers that came through the School of Military Government and CATP all went on to hold staff or field positions in every occupation the army ran, and some served in the most senior positions. (Indeed, when Lucius Clay took over in Germany, he replaced General Cornelius Wickersham—a former director of the School of Military Government.) Record Group 389, “Records of the Office of the Provost Marshall General,” in the National Archives and Records Administration has a five-volume history of military government training and numerous training manuals and curricula. How can these stories and sources be absent from a book that seeks to explain “how the army found itself capable of governing a foreign people” after World War II?

Chapters 5 through 10 deal with the fiscal and monetary policies of the German and Japanese occupations, with

three chapters devoted to each country. Here, readers will find the same problems of distracting biography (MacArthur’s failures in the 1948 Wisconsin presidential primary are not relevant to how he ran the Japanese occupation, nor are Commodore Matthew Perry’s visits to Tokyo Harbor in the 1850s), but the bigger problem is that the book’s core argument doesn’t survive basic scrutiny. Yes, Germany and Japan faced serious economic problems after the war—inflation, debt, currency transition issues, and (particularly in Japan), deficit spending and corporate monopolies (the *zaibatsu*)—and it is true that by the end of the U.S. occupations, those problems had largely subsided. But the entire premise of the book is that the *U.S. Army* fixed these problems and even brought those lessons back to the United States in the 1950s. It didn’t; civilians did.

Joseph Dodge, then the president of Detroit Bank, is far and away the most important character in that story: he was the economic prime mover in both occupations, and his “Dodge Line” became a synonym for the economic policy of the Japanese occupation. But he was also a civilian and never served in uniform. The other drivers of economic policy in the German occupation were also civilians: Lewis Douglas, Lewis Brown, Robert Murphy, Bernard Bernstein, Gerhard Colm, Raymond Goldsmith, and indirectly and from afar, John Kenneth Galbraith and Adolph Weber (brother of the sociologist Max Weber). These are the key voices in the intellectual history of the political economy of the occupations, and almost none of them are military ones.

The same was true in the headquarters of the Supreme Commander for the Allied Powers (SCAP), which ran the occupation of Japan. While there was a general in charge of SCAP’s economics section, Madsen admits that “the majority of staff serving MacArthur’s generals came from the State or Treasury” or from the private sector (131). And this makes sense, since fiscal and monetary policy were so complex and removed from military expertise that even MacArthur—a man not known for admitting what he did not know—pleaded with his staff to “find me somebody that knows something about the economy of Japan, because I don’t. And my military officers who are responsible for it don’t either” (142).

Were these civilians operating under army authority when they reigned in spending, slashed budgets, stabilized prices, and haggled with German and Japanese politicians over budgets and policies? Yes. It is fair to say that because of that, “the U.S. Military” revived the German and Japanese economies or “transformed the global economy”? No.

The book’s final three chapters all concern how the fiscal and monetary lessons of the occupations made their way back to the United States in the Eisenhower administration. I will leave it to economic historians to evaluate how well the book makes that case, but it seems unlikely to me that President Eisenhower’s economic approach during his presidency came exclusively, or even primarily, from the occupations. After all, his tenure as military governor in Germany lasted only seven months, and few—if any—of the major economic decisions were taken under his leadership. Getting American troops home, restoring basic safety, dealing with refugees and POWs, and preventing mass starvation were the tasks that undoubtedly filled his day. Thereafter, as Chief of Staff of the Army, he was consumed with the work of running the entire U.S. Army as it merged with the Navy Department to form the Department of Defense—no small task.

What specific economic lessons did Eisenhower take from the occupations? What is the evidence that he learned them there? It is true that some occupation officials came into the Eisenhower administration—Joseph Dodge was his budget director—but was Dodge channeling the occupations’ lessons or just sticking to economic principles that had served him well in a lifetime of banking?

First books are hard, and they are harder still when first-time authors get insufficient guidance along the way. Most of the problems enumerated above—overly ambitious claims, an overreliance on biography, unnecessary background and underwhelming evidence—should have been noted and corrected before the book went into print. The University of Pennsylvania Press did Madsen no favors in choosing (or allowing) a title that overpromises in several directions (both on the role of “the U.S. Military” in the story and on the claim that it “transformed the global economy”). Strong editing would have reined in the competition between narratives and excised unnecessary background. Sound copyediting would not have omitted footnote 24 on empire from the introduction or allowed the eight footnotes on the Rhineland occupation (35–36) to be drawn (erroneously, it seems) from www.worldwarone.com — a website that any scholar should be dubious of, and which does not contain the material cited in the notes.

It is possible these problems were noted but ignored, but the more likely explanation is that the publication process was rushed, and unfortunately, it shows. There is much still to be written on the U.S. occupations of Germany and Japan—and indeed, on the many other less successful attempts at military government that have occurred in U.S. history. *Sovereign Soldiers* is unfortunately not a significant contribution to that literature.

Notes:

1. Earl F. Ziemke, *The U.S. Army in the Occupation of Germany*, (Washington, DC, 1975), ch. 1.
2. Charles S. Hyneman, “The Army’s Civil Affairs Training Program,” *The American Political Science Review* 38, no. 2 (April 1944): 342–53.

Review of Grant Madsen, *Sovereign Soldiers: How the U.S. Military Transformed the Global Economy after World War II*

Carolyn Eisenberg

In this challenging book, the author takes on a long-neglected subject, namely the role of the U.S. military in shaping economic policy around the world. In the service of this objective, he has tied together the stories of three generals, each of whom occupied leadership roles in military governments after the Second World War: Douglas MacArthur, Lucius Clay and Dwight Eisenhower. Some of this biographical material will be new to readers, including the description of the generals’ relations with one another over decades.

Yet Madsen is less concerned with their individual personalities than with the policy questions they confronted and resolved. In the background of this inquiry is the author’s awareness of the American military presence in Afghanistan and Iraq. And while he does not engage these experiences directly, he discerns a continuity in such activities that goes back to the American occupation of the Philippines at the turn of the century.

To their responsibility for military government, all three generals brought disparate understandings. Yet regardless of how they started, or the location of their actions, all three eventually arrived at similar conclusions about how U.S. economic policy should be applied. The author considers this evolution to have significant repercussions beyond the occupied areas. He contends that the insights gained in postwar Germany and Japan informed Eisenhower’s approach to domestic and national security policy once he became president.

With considerable detail, Madsen describes how

beleaguered occupation officials concluded that “future peace depended upon integrating Germany and Japan into the global system of trade and finance” envisioned at Bretton Woods (4). But they also understood that a prerequisite for this development was the stabilization of their individual economies. Casting aside the Keynesian approach to public finance that had taken root within the Roosevelt and Truman administrations, military officials adopted “a policy regime” that featured “balanced budgets, a zero-inflation monetary policy and investment-led growth” (4).

Madsen’s narrative brings to American foreign policy a useful angle of vision, and he is surely correct in maintaining that the role of U.S. military governments in the economic sphere has received insufficient attention. Moreover, in telling this story, he highlights consequential economic decisions implemented by military officials that are often overlooked when submerged in the more dramatic accounts of the early Cold War.

Despite its muted tone, *Sovereign Soldiers* is in its quiet way a “triumphalist” history in which the major decisions of the military authorities are portrayed as stellar achievements from both an economic and a political standpoint. While Madsen argues the case for the former, he treats as self-evident the idea that the economic choices made by the Americans furthered the cause of democracy in Germany and in Japan.

Although fulsome in his praise for General Clay, Madsen is more critical of the decision-making by General MacArthur. His complaint is that during the early period of occupation MacArthur granted too much latitude to the New Dealers on his staff and was slow to recognize the necessary course for Japan. Yet, fortunately wiser heads prevailed, as key figures from the U.S. military government in Germany pointed him in a more conservative direction. Madsen also seems unimpressed by MacArthur’s handling of the Korean War, but these failings are tangential to his main themes.

In discussing the American occupation of Germany, the author takes aim, as many scholars have done, at JCS 1067, the initial set of instructions given to General Eisenhower for the occupation of the country. Often treated in the historical literature as the unfortunate outgrowth of Secretary of the Treasury Henry Morgenthau’s desire for revenge and his wish to de-industrialize the country, its flaws have seemed obvious. Yet Morgenthau’s anger notwithstanding, JCS 1067 and subsequent directives also reflected a powerful reform impulse that administration New Dealers injected into postwar planning.

The reform agenda for Germany was not primarily about de-industrialization. It was a multi-faceted commitment to wide-ranging denazification, the encouragement of a vibrant labor movement, the decartelization and de-concentration of German industry, and the decentralization of banking. Of this list, the only policy seriously implemented was banking reform, but even that was watered down over time.

The reformers in military government were also in favor of substantial reparations from both capital equipment and current production. Unlike Secretary Morgenthau, who was mainly interested in the removal of capital equipment, most reformers saw the need for current production as well, which implied the retention of a significant industrial capacity. Underpinning this approach was the recognition that generous reparations were vitally important as a means of ensuring four-power cooperation in occupied Germany.

For the Soviet Union, the ravages of the German invasion and the costly effort to drive Hitler’s army back to Berlin had left them in urgent need of material compensation from their recent foe. Absent such deliveries, pledged by

President Roosevelt at Yalta, the planned experiment in quadripartite occupation was unlikely to work.

In relating the story of the military government in Germany, Madsen omits almost entirely the American relationship with the Soviet Union and the tragic division of Germany. Early in his book he states clearly that he will not “take up the causes or course of the Cold War.” Yet by sidestepping this topic, he precludes a more hardheaded assessment of American economic decision-making.

In Madsen’s narrative, the splitting of Germany is taken for granted, as something that American officials were powerless to affect. So minimal is the attention to this development that for stretches of the narrative, one might imagine that the eastern zone of Germany did not exist, nor for that matter did Eastern Europe. In repeated references to Europe and to the “global economy,” these places are mostly invisible.

Yet the division of Germany was not a given, nor is it self-evident that the Russians preferred this outcome. For the short term at least, they seemed to prefer a unified country and were willing to make significant concessions to achieve it. From the declassified records of the Allied Control Council, it appears that a series of U.S. and British decisions in the economic realm seriously undermined the quadripartite project.

These economic decisions were problematic even for France and the Benelux countries, all of which feared a revived West Germany that could re-emerge as a military threat. However, for these other countries the promise of Marshall Plan assistance significantly diluted their objections, as did the subsequent formation of NATO. Madsen correctly stresses the importance of currency reform in triggering a remarkable resurgence in the western zones. Not included is the fact that by the spring of 1948, the Russians in the Finance Directorate of the Allied Control Council had essentially agreed to the American proposal. Unfortunately, the State Department had instructed Clay in March that “the policy of this government is no longer to reach agreement on currency and financial reform.”¹ By that time the relationship with Moscow had already deteriorated and Washington was less amenable to compromise on other matters. Yet during the initial two years of Allied occupation, General Eisenhower and General Clay had believed that cooperation with the Soviet Union was both feasible and desirable.

While Professor Madsen focuses considerable attention on Clay’s actions and views about specific economic questions, he largely ignores the general’s recommendations about maintaining German unity. As late as spring 1947, Clay and some of his closest advisors were supportive of reparations from current production, believing this would give the Soviets sufficient incentive to keep the country together. In this matter they were overruled by Washington officials, who were more interested in West European recovery than in keeping Germany whole.

As the author explains from the outset, U.S.-Soviet relations, and by extension the division of the European continent, are not the subject of his study. However, these matters are not so easily disentangled from the issues that do concern him. Any assessment of the economic “success” of the U.S. military government in postwar Germany must somehow include the forfeiting of the East, the failure to compensate the Soviet Union for its devastating wartime

losses, and perhaps most important, the infusion of Marshall Plan aid, which provided a lucrative framework within which a range of policy choices could work.

In what sense can we consider the economic policies in Germany and Japan a “military” achievement? What about the outsized influence of American businessmen in the Economics Division of U.S. military government? A major figure in this regard is General William Draper, who became head of that division early in the occupation. While Madsen emphasizes his military service, he pays less attention to his role as a vice president for Dillon, Read, which had been heavily involved in marketing German securities during the 1920s. Along with Draper came a list of businessmen and bankers, many from firms with previous economic interests in Germany and hopes for the future. It seems likely that these business connections weighed more heavily on their thinking than a military uniform.

From early on, Draper and his colleagues in the Economics Division were opposed to a reform agenda and did what they could to create the kind of “balanced budget, zero-inflation monetary policy, and investment-led growth,” which could be integrated into a global system of trade and finance” (4). In these efforts General Clay was usually a reliable ally and an effective implementer of their ideas.

The situation in occupied Japan was more complex, reflecting the longer influence of New Deal reformers and the more mercurial General MacArthur. Yet here as well, when the time came to impose a “reverse course” in this theater, Secretary of the Army Kenneth Royall brought William Draper to Washington as his undersecretary with authority over the U.S. military in Japan. Along with Draper came banker Joseph Dodge and others from the business community, some of whom had also honed their skills in occupied Germany.

Madsen’s important claim is that in both places the military played a pivotal role in bringing a “global economy” to life. But whether the military should be regarded as the formulator of those policies or as the servant of others remains an open question.

Omitted from the discussion is the role of coercion. Inside the American zone, the military held a monopoly on the authorized use of force. To what extent was it acting in conformity with the public’s wishes? And was the relevant public the Americans, the Germans, or perhaps neither? Madsen’s ready assumption of a democratic purpose leaves this difficult matter aside. Yet on an array of subjects—the socialization and codetermination of industry, for example, and the structure of trade unions and political parties—German public opinion was ambiguous at best. The same holds true for the most crucial question: should the economies of the western zones be fused, and was 1946-49 the right time to create a West German state? These concerns notwithstanding, *Sovereign Soldiers* is a valuable contribution to the field. By considering US military government as a discrete phenomenon and identifying relevant similarities across time and place, Madsen impels us to think more deeply about the role of US military, as it has emerged during “the war on terror.”

Note:

1. Cited in Eisenberg, *Drawing the Line: the American Decisions to Divide Germany* (Cambridge, 1996), 382.

Review of Grant Madsen, *Sovereign Soldiers: How the U.S. Military Transformed the Global Economy after World War II*

Curt Cardwell

I am encouraged by Grant Madsen's book, *Sovereign Soldiers*, because I am a huge fan of political economy in the study of U.S. foreign relations history and history more generally. Political economy is what got me interested in diplomatic history in the first place and why I ended up going to graduate school and on to the professoriate as a student and teacher of it.

There are signs that political economy is coming back in vogue after three decades in semi-retirement. According to Sven Beckert, one of its foremost practitioners, one of the main achievements of the "new" history of capitalism sweeping through the history profession of late is that it "reinstalls political economy as a category of analysis," which is indeed encouraging because of the weight that this new history is currently carrying in the profession.¹ Brad Simpson of the University of Connecticut characterizes political economy in a recent essay as a "seriously neglected area of research and writing in the history of US foreign relations," which is true enough. His essay is a call for historians of U.S. foreign relations to reengage with it, and hopefully the call will be answered.² This is all to the good. *Sovereign Soldiers*, it is a pleasure to state, adds to a growing list of books emphasizing political economy.

Since this is a roundtable there is no reason to offer an extensive summarization of the book's argument. Fortunately, and to his credit, Madsen lays out his thesis succinctly, so a brief summary is readily available. *Sovereign Soldiers* is an institutional history of military government as an occupation force rather than a fighting force. This is important to Madsen because, beginning with the Philippines, the military had to learn how to be an occupation force. But the book is also an intellectual history of the political economy that military government created through its experiences in the postwar occupations of Germany and Japan—a political economy that was, Madsen says, transferred to the United States via Generals Dwight D. Eisenhower, Douglas MacArthur, and Lucius Clay and had a deep impact on the domestic political economy of the 1950s.

To navigate what is well-traveled terrain in the realm of U.S. foreign relations history—the early Cold War period—Madsen employs the theory of the "external state," apparently developed by political scientist Robert Latham. As Madsen tells it, the external state of military government learned from the experiences of occupation, beginning in the Philippines and carrying on through the many occupations that military government engaged in throughout the early twentieth century through World War II and into the Cold War. Because no one really knew how to carry out such occupations, and because there was little direction from Washington, military government had to learn on the fly.

Madsen follows the careers of Eisenhower, MacArthur, and Clay to relay this history. He contends that, while Washington twiddled its thumbs, these top generals were forced to devise plans to operate the occupations of postwar Germany and Japan. However, they were well prepared to do so from their past experiences in Panama and the Philippines. He proceeds to analyze their actions through the lens of political economy, which he never defines but appears to accept as having a straightforward political ele-

ment and a straightforward economic element. The lens allows him (and us) to better understand the action of these men *vis-à-vis* Washington policymakers with whom they often did not agree. The views of Eisenhower and Clay become more central in the end because Eisenhower became president, with Clay as his close advisor, while MacArthur, as we know, merely faded away.

As a student of political economy, and especially of the time period in question, I find this a useful book. There is great factual information in it as well as primary sources that I find fascinating, intriguing, and confirming. Military government did have to act in ways that it was unprepared for, especially as occupation forces in the Philippines and Panama; and the history Madsen tells here is fascinating in that regard, if brisk. Its positioning of the U.S. military as key to the political economy of the global economy that emerged under the United States' watch and guidance is

As a student of political economy, and especially of the time period in question, I find this a useful book. There is great factual information in it as well as primary sources that I find fascinating, intriguing, and confirming. Military government did have to act in ways that it was unprepared for, especially as occupation forces in the Philippines and Panama; and the history Madsen tells here is fascinating in that regard, if brisk.

a needed addition to the new history of capitalism, where the military's role has thus far received short shrift. This is the book's greatest strength. It helps us identify with more specificity the role that military government played in the global economy's development after (and before) World War II, and that makes it an important contribution to the literature.

That said, the book, unfortunately, does not live up to its promises, and I mean that as one who sincerely wishes that it did. First, the concept of an external state is not satisfactory. Madsen admits that one of its primary advantages is that it "allows for a fresh take on the vast literature already written on the topic" (8). This is true. The concept provides a new window through which to view well-worn territory. However, even his own evidence does not bear out the existence of an external state at the level to which he wants to elevate it. The theory works fairly well in the beginning, when military government was an occupation force in the Philippines and Panama; at that point such occupations might have been new for the United States, and communications were still limited (although it can be argued that the army's war of conquest against the American Indians from 1865 to 1890 provided experience enough). To his credit, Madsen mentions this fact briefly in referring to MacArthur's father's (General Arthur MacArthur) experiences "oversee[ing] a regiment at Fort Wingate, New Mexico, through the 1880s" (129).

But when the book turns to its real concern—the post-WWII occupations of Germany and Japan—the evidence is less convincing. Although there is no doubt that military government had a degree of autonomy in the postwar occupations, often because of uncertainty and disinterest back home, and local events happening in real time, Madsen's own evidence demonstrates that, in the end, the directives largely came from Washington and were followed as strictly as possible. For instance, we learn that "while many Japanese believed that MacArthur made the occupation rules on his own authority . . . in reality he did his best to follow the written policy he received" (127). We are also told that MacArthur "planned to accomplish a radical transformation of Japanese society" but that "the orders he had already received from Washington told him to do the same thing" (130). And again, Madsen tells us that "in advancing these reforms [in Japan], MacArthur followed both the spirit and letter of the orders sent from Washington" (132). In October 1948, the National Security Council issued NSC 13/2, which forced MacArthur to "accept an 'ambassador' to spearhead economic reforms" (145). Undersecretary of the Army William Draper chose Detroit banker Joseph Dodge, who had earlier done extensive work on German

recovery. “Dodge,” Madsen writes, “would have the rank of minister and would ‘advise’ MacArthur. *But he would answer to Truman*” (147, my emphasis).

A stronger case can be made in Germany, but still, examples of a non-external state abound. Secretary of War Henry Stimson helped General Lucius Clay and General Dwight Eisenhower “maneuver against [Hans] Morgenthau” (87) to reinterpret Joint Chiefs of Staff directive 1067, which emerged from the Morgenthau Plan to punish Germany. Madsen does not mention this, but after meeting with Clay and Eisenhower, Stimson even met with Truman to discuss the problems with JCS 1067 and received Truman’s general agreement.

In January 1946 Washington sent a “special mission of technical experts on anti-inflationary measures” to Germany, against Clay’s wishes. Ultimately, the experts produced the Clay-endorsed Colm-Dodge-Goldsmith plan, officially known as the Plan for the Liquidation of War Finance and the Financial Rehabilitation of Germany. In the end, it was blocked by the joint State-War-Navy Coordinating Committee and went nowhere in Washington despite lobbying efforts by Dodge and Colm. Madsen claims that Bizonia, the combining of the American and British sectors of Germany into one unit, was Clay’s handiwork, which, if true, might stand as a prime example of an external state, but it is not. Clay pushed only for an economic bizonia with Britain, which he thought would bring the Soviets in (a fact Madsen ignores). The State Department mutilated Clay’s proposal and set out to create the bizonia as a unified economic and political entity that would pave the way for a separate West German state. That was the exact opposite of what Clay wanted.

There is also the Marshall Plan. It is certainly far too simple to argue, as Madsen does, that Secretary of State George Marshall’s visits to Clay in Germany in early 1947 were the basis for the “outline of the Marshall Plan” (111). The Marshall Plan came from many directions, and Clay was not always enthusiastic about it. After a recitation on what the Marshall Plan intended to do, Madsen concludes that “Marshall aid simply encouraged the rest of Europe to do what military government aimed to accomplish within Germany” (112). This is supposed to be a strong argument in support of the external state thesis, but one could certainly argue that it shows that Washington developed the Marshall Plan quite independently, with little input from military government in Germany. Both of these instances seem to be attempts to elevate the role that Clay and military government played in the Marshall Plan above and beyond what it actually was. Madsen makes a stronger case when he examines Clay’s role in enacting currency reform in western Germany, but, although the reform proved more successful than Clay or anyone else could have imagined, currency reform was not the be-all or end-all of German recovery by any stretch.

On all these issues concerning Germany, I am discouraged by the fact that Madsen makes so little use of Carolyn Eisenberg’s *Drawing the Line: The American Decision to Divide Germany, 1944–1949*.³ Eisenberg shows in painstaking detail the way in which military government was undermined by officials in Washington, often to the point where Clay, who supported trying to reach agreement with the Soviets so as to keep eastern Germany free until that alternative became untenable, would react with utter disbelief. Her point is that officials in Washington were out of touch with what was happening on the ground in Germany, but their decisions still overrode those of military government.

My point is that the external state, on close inspection, does not appear to have been as external as Madsen would like us to believe. It might be, but Madsen does not, to my mind, prove that in this book.

On another matter, I do not know what to make of a book on the political economy of U.S. foreign relations published in 2018 that sees “the turn to the idea of ‘empire’ to explain the way the United States functions in the global context” as “another recent development among scholars” (7, my emphasis). I think, were he alive, that would be news to William Appleman Williams, whose *The Tragedy of American Diplomacy* came out nearly sixty years ago, and his last book, *Empire as a Way of Life*, over forty. It would also be news to John Lewis Gaddis, still very much alive, who spent a career trying to refute Williams but whose 1993 book *We Now Know* concedes that the term “empire” applies to the United States (something theretofore rejected by conservative scholars), although

not in the way Williams explained it; and it might surprise Geir Lundestad, also very much alive, whose seminal article from 1986, “Empire by Invitation,” moves in the same direction.⁴ Oddly, Madsen cites Lundestad’s 2012 book *The Rise and Decline of the American “Empire”* but seems unaware of the earlier work.

Scholars such as Noam Chomsky, Gabriel Kolko, Joyce Kolko, Thomas McCormick, Lloyd Gardner, Walter LaFeber, and Marilyn Young have been using the framework of an

American empire since the 1960s. In the 2000s, even conservative scholars were adopting the term. Witness Andrew Bacevich’s *The American Empire* and Robert Kagan’s *Dangerous Nation*.⁵ I do not know if Madsen is actually unaware of this previous scholarship, but I can only go by what he wrote. Personally, I doubt it, but that raises another set of issues that space does not allow me to comment upon.

One further observation, rather than a criticism, is that the book seems to be a thinly veiled dig at Keynesian economics, almost as if that is its main goal. Madsen contends that MacArthur, Eisenhower, and Clay “missed what came to be called the Keynesian Revolution” because they were stationed overseas during the Depression years, when John Maynard Keynes wrote his famed treatise that all but ended classical economic theory for the next fifty years (61). He argues that in their occupation policies the three men (although it is hard to discern if MacArthur really felt this way) believed in what were essentially anti-Keynesian policies—balancing budgets, cutting taxes, abolishing price and wage controls, and curbing inflation even if doing so meant high unemployment—in direct contrast to the Truman administration, which stood by its Keynesian policies, particularly in and through the Fair Deal and NSC 68. This belief came from their experiences as occupation leaders, when, supposedly, they prevented deficit spending to put people back to work. Furthermore, he argues that these policies then informed Eisenhower’s and Clay’s actions when Eisenhower became president, thus linking the lessons of the occupations to the domestic political economy. In this endeavor, there can be little doubt that Madsen is a big fan of Eisenhower, Dodge, and Clay (he is less sure of MacArthur) and that he is essentially using them as historical actors to denigrate Truman and the Keynesians.

The problem is that he is ignoring a major aspect of the story that undermines this rosy view of the supposed non-Keynesians: the role that military spending played in the recovery of Germany’s and Japan’s economies and in jumpstarting, or at least priming the pump of, prosperity in the United States, which is often referred to as military Keynesianism. I am not questioning that Clay (through

Dodge) in Germany and MacArthur (through Dodge) in Japan adopted anti-Keynesian measures, particularly as regards deficit spending. But the Marshall Plan, first, then military spending under the Mutual Security Program, allowed Germany to avoid the deficit spending that otherwise would have been necessary, by Madsen's own admission. And, in Japan, which had no Marshall Plan, the anti-Keynesian Dodge Plan largely failed (again, by Madsen's own admission), even if Japan did avoid deficit spending. But it was military spending by the United States that allowed the Japanese to avoid that spending.

Turning to the domestic economy, we take it for granted that Eisenhower was not a Keynesian and that he rejected Truman's economic policies. He even managed to balance the budget several times during his presidency. But military spending did not shrink under Eisenhower in as dramatic a fashion as scholars often claim, especially when they are trying to compare him to the Keynesian Truman. For the fiscal year budget prior to NSC 68, Truman was seeking a defense budget of \$13 billion, down from \$14.5 billion from the year before, because he, too, was concerned about budget deficits (ignored by Madsen). Then, beginning in July 1950, military spending increased dramatically under the impact of NSC 68 and the Korean War. Eisenhower vowed to cut defense spending, down to about \$42 billion. He did so by relying more on nuclear than conventional weapons to "defend" the United States and its allies, a questionable choice when assessing the soundness of Truman's versus Eisenhower's policies. (Truman's approach was guns and butter; Eisenhower's was apparently guns and butter plus annihilation.) Still, a \$42 billion military budget was hardly close to the \$13 billion figure Truman had hoped for in 1949, and, what is more important, the budget mostly increased thereafter, right into the stratosphere.

When Eisenhower left office, military spending was close to \$50 billion and would go up ever after, with increases in the nuclear arsenal as well. Eisenhower did not have to pursue deficit spending (although he did do that too) because he benefited from an economy that was growing overall, as NSC 68 had predicted it would. Yes, good anti-Keynesian that he was, Eisenhower cut price and wage controls and things turned out wonderful for the American economy. But this was due, again, to the overall growth of the economy, which likely would not have happened absent NSC 68 or the rearmament program it and the Korean War engendered. Proof for these claims is not hard to find. It can be found, among other places, in my own book on political economy and the early Cold War, which I encourage interested parties to check out from their libraries.⁶

Notes:

1. Sven Beckert and Christine Desan, eds., *American Capitalism: New Histories* (New York, 2018), 10.
2. Brad Simpson, "Explaining Political Economy," in *Explaining the History of American Foreign Relations*, ed. Frank Costigliola and Michael J. Hogan, 3rd ed. (New York, 2016), 58–73.
3. Carolyn Eisenberg, *Drawing the Line: The American Decision to Divide Germany, 1944–1949* (New York, 1996).
4. William Appleman Williams, *The Tragedy of American Diplomacy* (New York, 1959) and *Empire as a Way of Life* (New York, 1980); John Lewis Gaddis, *We Now Know* (New York, 1998); Geir Lundestad, "Empire by Invitation? The United States and Western Europe, 1945–1952," *Journal of Peace Studies* 23:3 (1986): 263–78.
5. Andrew Bacevich, *American Empire: The Realities and Consequences of U.S. Diplomacy* (Cambridge, MA, 2002); Robert Kagan, *Dangerous Nation* (New York, 2006).
6. Curt Cardwell, *NSC 68 and the Political Economy of the Early Cold War* (New York, 2011).

Response to Reviewers

Grant Madsen

I would like to thank Andrew Johns for making this roundtable possible. When he told me whom he had invited to review my book, I will confess to freaking out. As will become clear below, I spent months—perhaps years—working my way through the scholarship of these reviewers (and our panel moderator, Laura Hein). I never imagined I would face them all at once, all in one roundtable! Given how well I feel I know their work, and how much I depended upon it in my research, simply thanking them for their reviews seems insufficient, so I would like, in addition, to express my gratitude to Laura Hein, Carolyn Rosenberg, Michael Hogan, Curt Cardwell and Aaron O'Connell for their wonderful scholarship, which has so informed my own feeble efforts to do justice to their work.

Before responding to their challenging reviews, let me explain a little about *Sovereign Soldiers*. The book crosses a lot of historical subdisciplines—economic history, policy history and American political development, diplomatic history, and military history. In selecting the reviewers, it seems clear that Andrew Johns tried to find scholars who could speak to individual aspects of my study, or so their reviews seem to suggest. Each takes up issues dear to his or her own research.

At root, however, *Sovereign Soldiers* is an intellectual history of economic policy. Its central claim is that the United States, Japan, and West Germany shared a distinct economic policy regime in the 1950s because a central set of political actors, working in and around military occupations (what I label the "external state"), developed that regime, first in occupied Germany, then in Japan, and ultimately in the United States. There is much more to the story that I will discuss below. But I wanted to establish my goal because, in reviewing *Sovereign Soldiers* from their individual vantages, only one reviewer mentioned what is, in my estimation, the central insight. When all is said and done, I claim that a group of non-Nazi German economists, provided the basic blueprint that became that policy regime.

The only reviewer to really acknowledge this part of the story is Aaron O'Connell, and he only acknowledges it to disqualify those who participated in it. "These are the key voices in the intellectual history," he writes of the German and American civilians working together in the occupation, "but almost none of those voices are military ones."

As for the policy regime itself, I summarize it as two questions. "Should a vastly empowered government function (roughly speaking) on a pay-as-you-go basis? Or should it accomplish its goals on credit" (6)? The military and civilian leaders I write about decided that "the answer to this question had enormous consequences," because "a pay-as-you-go approach tended to keep a balance between the interests of the state and citizenry" and avoided "over-commitments and broken promises that could lead to political upheaval later" (6). In other words, the men who came out of the occupation worried that loose monetary policy and deficit spending would lead (at best) to the kind of inflation and stagnation that in large happened in the 1970s. At worst, they feared that these policies would end in economic totalitarianism. So they avoided both.

Their assessment provides for some fine-grained distinctions that animate my response to Curt Cardwell on how we should understand Keynesian economics. It also raises questions about how the Bretton Woods Agreements should relate to domestic economies generally and Keynesian economics specifically (which does not come up in the reviews but plays an important part in my economic analysis).

Finally, I am influenced by a number of diplomatic historians who try to think beyond largely American concerns and methodologies. As I will discuss at greater length below, all the reviewers, with the possible exception of Carolyn Eisenberg, seem to have analyzed my book with concerns in mind that stem primarily from American historiography. Michael Hogan essentially reads my book as a return to Eisenhower revisionism and as a so-so defense of American conservatism; Curt Cardwell feels it fails to appreciate American imperialism and the role of America's military Keynesianism; and Aaron O'Connell sees it as failing to provide a clearly demarcated institutional history of the American military.

By contrast, my research is closer in spirit to a book like Daniel Rodgers's *Atlantic Crossings* (although in my story policy ideas also cross the Pacific before coming ashore in the U.S.). The military matters, because after World War II it sprawled around the world and therefore functioned as the transmitter of ideas or "circuit of exchange" (9). The subtitle of my book, "How the U.S. Military Transformed the Global Economy after World War II," speaks to how the military was uniquely positioned to communicate, influence, and implement ideas at that particular moment in history and how it created a kind of "external state" not completely tied to any one nation (although clearly more American than either German or Japanese).

While I will try to respond to each of my reviewers in turn, in places it makes sense to link them, given the overlapping concerns they have.

Of all the reviewers, Carolyn Eisenberg makes the best effort to engage my book on its own terms. This came as no surprise. In important ways, *Sovereign Soldiers* offers an extended response to her own *Drawing the Line: The American Decision to Divide Germany*. I take up many of the same issues she addresses and follow her methodology. Like her, I look at political and military elites working in and with military government. Thus, contrary to what Aaron O'Connell says, I follow Eisenberg in thinking of military government as concerning more than those in uniform. And like her (and unlike O'Connell) I found little help in the archives of the School of Military Government (SOMG) or the Civil Affairs Training Program (CATP). As Eisenberg notes, Henry Morgenthau began to eclipse the work done there when he involved himself in occupation planning. More to the point, Lucius Clay began to change his mind about many of his orders almost from the beginning. If one wants to understand the roots of, for example, Germany's currency conversion, one will search the records of the SOMG as well as the CATP in vain (which, sadly, I did).

Eisenberg is right to say that my narrative does not do justice to the original hopes of New Deal reformers seeking "a multi-faceted commitment to wide-ranging denazification, the encouragement of a vibrant labor movement, the decartelization and de-concentration of German industry." She is also correct when she notes that the story of reparations, "vitally important as a means of insuring four-power cooperation in occupied Germany," does not get as much attention as it might. Nor does the "relationship with the Soviet Union, and the tragic division of the country." But she essentially articulates my reason for these omissions when she writes that too often "consequential economic decisions implemented by military officials" have become "submerged in the more dramatic accounts of the early Cold War." That is exactly how I would say it. My particular story would get lost, I feared, in the drama of the Cold War.

Eisenberg is most on point in asking "in what sense can

we consider the economic policies in Germany and Japan a 'military' achievement? What about the outsized influence of American businessmen in the Economics Division of U.S. military government?" In many ways, this question prompted my own research years ago. I wondered whether it seemed fair to ascribe the genesis of so much policy to (for example) William Draper's experience with Dillon, Read. More to the point, why was Lucius Clay "usually a reliable ally and an effective implementer of [Draper's] ideas?" What was it about his prior experience that suggested he planned to set aside much of JCS-1067, design a currency conversion that would launch an economic miracle, or devise economic policy at all?

Thus, I started my research by investigating the prewar experience of the military and civilian leaders who led the occupations in Germany and Japan. Contrary to what Eisenberg (and, as I will argue below, Michael Hogan) contends, I found little to suggest a shared ideological commitment to what ultimately happened in the occupation. What Aaron O'Connell refers to as "at best colorful vignettes, and at worst, major distractions" serve to demonstrate not only what military leaders learned in prior occupation experiences, but also the biases they *did not* demonstrate. As I write, "Given the longer lens of this study, it seems unlikely Clay or Eisenhower had any strong ideological leanings at war's conclusion. If anything, they leaned toward the New Deal and not a nascent 'conservatism'—whatever that meant at the time. More than anything, they seem motivated by a pragmatic moralism" (283, n. 35).

The conclusion that Clay, et. al., did not necessarily come to the occupations with a particularly strong ideological bent opened up the possibility (as I describe particularly in chapter 6) that none of these figures played the critical intellectual part in the story. Indeed, I discovered that none of the key Americans really knew enough about what they were doing to implement a coherent economic policy. They did not understand why the German economy remained stagnant, how to rescue its worthless currency, or how to get industry to perform. Pragmatists that they were, they ultimately tried to solve these problems by turning to the source at hand: German experts who had lived through a similar experience after World War I.

In other words, when Eisenberg declares that "whether the military should be regarded as the formulator of those policies or as the servant of others remains an open question," I would argue that the word "servant" is both too pejorative and vague, but the essence of the statement is exactly right. From an intellectual history point of view, the policies coming out of the American occupation of Germany represented a collaboration of German expertise and American implementation. In short, the policy regime belonged to both.

This takes us to a second important issue that Eisenberg rightly raises and that, I must admit, I try to address only by implication: "the role of coercion" in relation to "the public's wishes." It is possible that the public (I think she means the occupied Germans) wanted a more robust labor movement with greater denazification than what emerged—although some might argue otherwise. At the same time, though, to have any legitimacy an economic system must "deliver the goods," so to speak. This is a very complicated topic; the debate over the relative merits of economic growth versus social equality has raged and will continue to rage for some time, and it will not be put to rest in this venue.

In writing *Sovereign Soldiers*, though, I argue against the assumption that a wide variety of economic arrangements

might have produced a stable economic outcome. I am skeptical, in other words, that Clay had many options at hand (a point I will return to in my response to Michael Hogan). Here the comparison to Japan is helpful. Japan followed more aggressively the political reforms Eisenberg mentions, only to see its economy go nowhere. American officials began to panic. One wrote home to complain—with some justification, I think—that a “sane democracy cannot rest on an empty stomach” (132).

Moreover, Japan (along with several other European countries) also performed currency conversions shortly after the war, none of which produced an economic miracle—which raises the obvious question of why. What other policies were in place in Western Germany that were not in place elsewhere? As subsequent military occupations suggest, it is no easy task to revive another nation’s economy. The effort is fraught with problems and potential pitfalls, and discovering a mix that accomplishes recovery often proves as elusive as it is instrumental in protecting the very political reforms (limited thought they may be) that Eisenberg recommends.

If Carolyn Eisenberg’s *Drawing the Line* gave me the thematic concerns that animate *Sovereign Soldiers*, Michael Hogan’s *A Cross of Iron: Harry S. Truman and the Origins of the National Security State, 1945–1954* provided the blueprint for how to write a history that brings together an analysis of state institutions, foreign policy, and economics. I appreciate his lengthy elaboration of the narrative arc and also his (tentative) approval of the scope of my “big book.” At the same time, he offers a few criticisms that deserve a response. He fears that even the “more detailed treatment of the German and Japanese occupations is limited, as are [my] efforts to tie German occupation policy to the Marshall Plan (though there are definite connections).” Where he says “limited” I would prefer to say “focused.” He is right that “no episode is treated in depth,” nor do I prove that the kind of “fiscal and monetary policies” I discuss “alone accounted for whatever success the Americans [I would add the Japanese and West Germans] enjoyed.”

Fair enough. But having said that, the book would have a hard time doing all these things without becoming unmanageable. Moreover, my goal lay in tracing the origins of policy and how it spread. While I think there is good evidence for giving credit for the solid economic growth that occurred in the United States, Germany, and Japan through the 1950s to the policy I discuss (and I cite the economist Michael Bordo to that end), I do not offer a thorough economic analysis.

Hogan suggests that I must “concede that Dodge fell short of his objectives [in Japan] and his gains were largely reversed.” Curt Cardwell agrees. The “anti-Keynesian Dodge Plan largely failed, again, by Madsen’s own admission.” To be clear, I don’t say this. True, Dodge thought (in 1952) that he had failed and that Japan would go back to a high-inflation, big-deficit economy. It didn’t. As for Dodge, he *always* thought that he had failed. He thought he failed Clay as finance director in the German occupation and Eisenhower as director of the Budget Bureau. It was part of his charm.

What I actually write is that “Dodge profoundly underestimated his impact” in Japan (181). More to the point, I argue that in the 1950s, in both Japan and the United States, “there were state institutions that could have been used along Keynesian lines. They could have facilitated deficit spending to spur growth in the name of full employment. Yet both countries adopted policy

regimes that eschewed government debt in the name of low inflation” (182). I discuss how the United States came to follow Japan in subsequent chapters, with Dodge’s help.

Along these lines, Hogan fears that I “often” make “claims that are hard to defend.” For example, he quotes me as saying that “Dodge’s hard line succeeded in Japan, despite the dislocations, unemployment, and protests it provoked, its repudiation by the State Department, and its reversal by the Japanese government.” There is a genuine debate about the economic consequences of Dodge’s program and whether it or the spending associated with the Korean War ultimately sparked Japan’s own economic miracle. Again, my first concern lay in showing how it took hold.

Having said that, though, perhaps I can ask a question posed to me by an economist friend who reviewed this chapter: “What unemployment?” As I show in chapter 10, the occupation’s statisticians never put the unemployment rate above two percent! That is an extraordinarily low number, and it raises at least one question: how should we judge economic “dislocations” that hardly affect broad economic indicators, or quite quickly affect them in a positive way? Indeed, many of the main indicators began showing improvement by early 1950, before

the Korean War began and at the very moment things supposedly looked bleakest for the Japanese economy.

I spent some time in chapter 10 talking about how American officials could not make sense of the way some Japanese officials complained about the Dodge Line when the economic data they collected told a very different story. I included this discussion as a metaphor for the similar disconnect between historians and economists today. Historians (Hogan included) have tended to generalize the complaints and take them at face value; the data, by contrast, seem to say something very different.

Finally, what policy reversal? To give a few more examples, Dodge set the exchange rate for Japan at 360 yen to the dollar. It remained there for two decades. He insisted that Japan develop an export-oriented economy based on that stable exchange rate. When did that change?

There is, however, a more important point to raise that is implicit in Hogan’s concerns, and here let me turn the tables just a bit. While the corporatist approach strongly influences my research, its interest in organizational forms sometimes undercuts the search for good explanations for policy formation. Ideology ends up compensating for this weakness, where labels like “conservative” and “liberal” stand in for the intellectual genealogy necessary to understand policy in its own right. Ideology explains the organizational form which explains the ideology which explains the form, etc. My research indicated a distinct policy regime coming out of the occupations that did not comfortably fit the terms “liberal” and “conservative” as commonly used. Knowing that readers might be tempted to apply those terms anyway, I tried to preempt the temptation by warning, for example, that “the ‘big government-small government’ debate can trap historians in categories that often obscure the many ways the American state has evolved both domestically and internationally” (6). Specifically, as I explain throughout, the occupiers accepted (and even promoted) big government even if they rejected the American interest in Keynesian deficit spending. In other words, they were not conservative in the typical American sense, although I did not give their distinct approach a name.

Thus, I avoid the terms “conservative” and “liberal” as

best possible, trying to keep the focus on the actual policies rather than on labels. For example, in the roughly 260 pages of my narrative, I employ the term “conservative” eight times, often in reference to American historiography. In Hogan’s ten pages of review, by contrast, it appears fifteen times. More to the point, at times he reads into my book a conservatism that is not there. For example, he writes that “Joseph Dodge, another veteran of the German occupation . . . reduced taxes, curbed government spending, and eliminated price and wage controls [in Japan].” This sentence is two-thirds true. Dodge curbed government spending and ultimately saw an end to wage and price controls.

However, as my book shows in detail through chapter 9, Hogan is wrong on taxes. Ikeda Hayato, Japan’s finance minister, desperately begged Dodge for even a small tax cut and Dodge refused. In fact, Dodge went on to reform tax collection in a way that effectively raised taxes by spreading the tax burden. (Prior to the reforms, tax collectors worked on a quota system; once they had achieved their target, they stopped collecting. Taxpayers could avoid payment if they could wait long enough for the collector to reach his goal.) Japanese officials, not Dodge, sought tax cuts.

Similarly, Hogan writes that “from their limited experience as an occupying force in the Rhineland, and their postwar duties in the Philippines and Panama,” the occupiers “saw the benefit of empowering local governments, so far as possible, and the need to stimulate production and trade, balance budgets, and control inflation by means other than government regulation.” Here, again, this sentence is only partly true.

As I point out in chapters 3 and 4, Eisenhower at least, readily embraced regulation before World War II. At that point enamored of Bernard Baruch, he wrote a report for Herbert Hoover that recommended price controls and rationing in times of inflation. Moreover, both he and Clay embraced the National Recovery Administration, which set prices, production levels, and wages for whole sectors of the economy. In addition, Clay made his reputation by integrating the Army Corps of Engineers into the Works Progress Administration, and Eisenhower hoped to join the Public Works Administration until his boss, Douglas MacArthur, vetoed the idea. Both of these agencies represented large public works efforts funded by borrowed money. Neither Clay nor Eisenhower raised any ideological objections at the time.

In a long and thoughtful review, these are perhaps small errors. But they are both telling. As if to illustrate my larger frustration with the way corporatism overuses ideology, nearly every time I see Hogan misstating what I wrote, he is doing so by asserting aspects of American conservatism that are not in my story, or I went to pains to demonstrate were not true.

I suspect this explains why he omits the key role played by the German economists in shaping occupation policy in Germany. He implies a ready-made “conservatism” sitting on the shelf, as it were, for Eisenhower, Clay, Dodge, Draper et al. to simply implement. Thus, when he does talk about the German occupation, he describes a Germany that is, in effect, a tabula rasa, prostrate before its American conquerors and ready to reproduce American politics on a new stage. “Although both sides saw free and fair trade as essential... Keynesians [the first side] were more anxious to promote political and social as well as economic reforms in the occupied areas, more tolerant of deficit spending to prime the pump of the economy, and more inclined to use

wage and price controls to tame inflationary pressures...,” while the other side “Clay, Eisenhower, and other more conservative policymakers,” had different views. As the occupiers discovered, however, there were more than “two sides.” Really, the whole question of “sides” fundamentally misunderstands what they faced. They had inherited a combination of economic problems without precedent in the American context—massive inflation on top of a well-developed but moribund industrial economy, sealed foreign borders (not to mention sealed regional borders between occupation zones), and huge relief issues generated by a massive influx of refugees. Worse still, the American economics profession had been (rightfully) obsessed with solving the Great Depression; economists had given much less thought to how inflation could cause stagnation (or what we now call “stagflation”). American experts had almost nothing to offer. Hence the need for a distinct mission to “analyze programs and results of anti-inflationary action taken by other European countries as well as analyze the German situation” (101).

This whole reliance on domestic political categories becomes most blatant in the last paragraph of his review. Hogan makes a gesture in the direction of what I argue for most of the book: “Madsen doesn’t see Eisenhower as a Keynesian liberal nor does he consider him a conservative ideologue.” But then, to my astonishment, it turns out that this should disappoint me, since my account “raises doubts about how committed Eisenhower was to some of the conservative strategies [Madsen] appears to celebrate.” Huh?

To reiterate, I had hoped to make this whole discussion beside the point. Eisenhower “never really engaged in the fight between American liberals and conservatives. His political economy fit a global context where he never

lost his focus” (244). Hogan may disagree, and in advancing his view he certainly carries a gravitas that I do not. But I think the need to characterize policies within the simple and very American binary of “liberal” and “conservative” has grown a bit tired, even in contemporary American politics.

It is nice to have an ally in Curt Cardwell, and I appreciate not only his review of my book but also his willingness to promote our shared interest in political economy. He dispenses with a summary of the

book to get to his concerns, so I will do the same.

Both Cardwell and Michael Hogan have reservations about the external state. While Hogan sees promise in a “comprehensive exploration” of it, he writes, that my effort “is a somewhat sketchy account that is limited.” Cardwell goes beyond Hogan in doubting its efficacy. While “military government had a degree of autonomy in the postwar occupations,” in the main, he says, my “evidence demonstrates that . . . the directives largely came from Washington and were followed as strictly as possible.” Specifically, he shows where my text demonstrates Washington choosing to halt, revise, or “mutilate” initiatives begun by MacArthur, Clay, or Eisenhower.

This criticism is well taken. I leave the term “external state” undertheorized. In all honesty, I am still not sure how “external” an institution must be to qualify as truly “external.” In fairness, though, total independence never fit the definition. The term refers to “those institutions functioning outside the formal boundaries of the United States, while still tied to it” (7). Still, this does not settle the question of how much independence is necessary for an institution to count as “external.”

In crafting the book, my challenge could be summed

up in this question: how can we apply scholarly work on nation states (work in the subfields of policy history and American political development, for example) to state-like institutions that do not belong entirely to a single nation or within its borders? Military government may have ultimately depended upon the nation state in many ways, but it also remained responsible to and for non-American subjects—and, in the German context, at times remained surprisingly open to input into the policy process from local citizens. The “external state” seemed the best shorthand for getting at that.

At the same time, I think Cardwell overstates the degree of Washington oversight. To give one example from his review, when I write Dodge “answered to Truman” (147), I meant only that Douglas MacArthur had no authority over him. As the subsequent chapter demonstrates, Truman never in fact instructed Dodge to do anything once he assumed his role in Japan. Indeed, Dodge had such a free hand that he openly implemented policies that directly contradicted what Japanese officials saw happening under Truman in the United States. “One of the greatest difficulties I have to overcome, and a growing one,” MacArthur complained to Dodge at the time, “is the basic difference in the policies we are imposing here and the policies being followed in the United States” (171). How should we characterize a situation where military government pursues a set of policies directly contradicted by those promulgated by Washington? Here, I felt the “external state” served as a handy analytic tool.

While I could well be mistaken, it feels as if Cardwell really just prefers the rubric of empire to “external state,” and that might motivate some of his criticism. That helps me understand his indignation at my incomplete footnote on the historiography of empire. It might also explain his puzzling concern about why I make such “little use of Carolyn Eisenberg’s *Drawing the Line*.” Given what I have written above, he must mean that my narrative should have accepted her conclusions in toto rather than using her book as a launching pad to strike off on my own.

Turning to the economic part of my story, Cardwell observes that my book “seems to be a thinly veiled dig at Keynesian economics.” To be clear, there is no veil (thin or otherwise) over what I say about Keynesian economics. This is a central argument of the book, and specifically an argument against historiographic accounts that summarize the postwar decades as part of a Keynesian revolution.

This leads us to Cardwell’s last paragraph, which raises a host of issues in a short space. Some of these issues involve some fine-grained interpretive arguments, and I will try my best to untangle what I can. Cardwell concludes that my book ignores “a major aspect of the story that undermines this rosy view of the supposed non-Keynesians”—specifically, “the role that military spending played in the recovery of Germany’s and Japan’s economies.” So far, so good. I agree that this spending was important. But then he adds that such spending is “often referred to as military Keynesianism.” That is also true in one sense: people do say that. But I don’t use the term, and people who do are wrong to apply it to Eisenhower’s years in office.

American historiography tends to call “Keynesian” any government activity designed to generate economic growth. Whether it is Marshall aid, or tax cuts, or perhaps even land-grants, as long as the state is doing something to spur the economy, it gets labeled “Keynesian.” Of course, this would make nearly every president and Congress Keynesian, starting with George Washington. But it would

make nearly all economists, from Milton Friedman to Paul Krugman, Keynesian as well, because nearly all agree that government spending *can* spur growth, at least in the short term. But that was never the issue for Keynes nor for those who followed him.

For economists, the debate usually turns on whether state spending crowds out private investment, whether it tends to generate greater long-term gains than private investment, and so on. But this is beyond my interest. My point here is that the term “Keynesian” can only have coherence analytically if it refers to something more than “look how much the government spent here.”

Because I feared this kind of simplistic approach to what we mean by “Keynesian,” I spend a fair amount of time in the book explicating the thinking of the foremost adopters and adapters of Keynesian economics in the United States, and particularly the people who would go on to criticize the Eisenhower administration for failing to understand Keynes’s insights. I write about Alvin Hansen and Paul Samuelson, along with Leon Keyserling and Walter Heller (I also spend some time on Ishibashi Tanzan in Japan). These men feared that savings do not automatically become investment, so they worried about “liquidity traps”—“idle funds for which there is no outlet” and which, if left unused, result “in a downward spiral of income and production” (187). They also feared that even a small economic downturn would quickly become a depression as “animal

spirits” drove investors from the market. The solution? They recommended “an easy-money policy (reduced interest rates) . . . large governmental expenditures of all types, and . . . huge public works programs *financed by public borrowing*” (187, italics added). In general, they did not fear inflation. As Paul Samuelson said, the country should not let “concern over price inflation dominate our decisions” (251).

In general, they also agreed that demand created its own supply, which in very simple terms means that that if one just has enough money, eventually someone else will produce what one wants. For the American Keynesians, this often meant that governments could generate ideal levels of production if they just spent enough. The economic historian Brad DeLong nicely summarizes American Keynesians as agreeing that the business cycle represented a fluctuation “below some sustainable level of production,” not a “fluctuation around” it.¹ Thus, throughout the 1950s, people like Keyserling and Samuelson criticized Eisenhower for letting the economy drop below its optimal output, for letting unused resources go to waste.

Cardwell does a nice job of laying out the defense spending figures for the Eisenhower administration as a way of showing that military spending persisted at high levels throughout. He also mentions that we can find this information in his own book. As it turns out, I worked up the same figures in a variety of places in my book (see, for example, “Figure 18: US defense spending, 1950–1961.”) The point, though, is that these figures alone amount to telling us who won a baseball game by revealing only one team’s score, which does not tell us much, even if it’s a high score.

As I tried to clarify repeatedly throughout the book, to understand the policy regime that emerged from the occupations we must consult the financing side of the ledger. What the occupation taught Clay, Dodge, Draper, Eisenhower et al. was to ask these questions: Where did the money come from? From taxation? From borrowing? From

printing money? And why? Did the spending serve to soak up "idle funds?" Did it aim to keep "effective demand" high enough to guarantee maximum output and full employment? Was it devised to counteract "animal spirits?" These are the questions they cared about, and their answers distinguished them from the Keynesian economists, who had very different answers and fears (as I mention above).

Military spending may have positively affected the overall economy in the 1950s. Cardwell thinks so: 1950s growth "would not have happened absent NSC 68," he writes. I am less certain. But in and of itself, military spending is not necessarily Keynesian unless it fits within this broader analysis and unless it aims to "compensate" for some failure in the private economy.

Meanwhile, there is another sentence that needs a little unpacking. Cardwell writes that "the Marshall Plan, first, then military spending under the Mutual Security Program, allowed Germany to avoid deficit spending that otherwise would have been, by Madsen's own admission, necessary." If one follows the analysis of my book, it should be clear that this sentence cannot make sense. Forgive me for spending perhaps too much time on this one sentence, but it gets at an important point that is also often misunderstood. As noted, Keynesian economics is often simplified into the notion that "demand creates its own supply." But there are some catches. First, what if your economy simply does not produce what you need? For example, suppose your country can't grow enough food to feed you. How can you spend on something that simply does not exist in your context? Worse, what if no one in or outside your country trusts your money enough to give you anything of value for it? No matter how much you have, you can't buy anything because no one wants your worthless money.

In general, both of these conditions held true for postwar Germany and Japan, which raises questions about Cardwell's understanding of the economic implications of what he says in the sentence quoted above, particularly where the Marshall Plan is concerned. What could deficit spending hope to accomplish? As long as the West German government tried to spend its own money, who would sell anything to it, regardless of whether it "balanced" its budget? Joseph Dodge made exactly this point in a radio address supporting the Marshall Plan in 1947. "We are not sending so much of our money abroad as we are sending our goods," he explained. "Monetary stimulations within Germany are of no help, for the goods the extra money would buy, no matter how soundly based or tightly controlled the money might be, are simply not available" (111-12).

This was the Achilles heel of the Keynesian analysis in its foreign context. Unless a country has access to goods and has a currency trusted by its own citizens and foreign trade partners, deficit spending (indeed any attempt at spending) will not work. That, in a nutshell, was the central problem of the occupations. Hence, in both Germany and Japan the occupiers insisted on restoring a currency that would generate this trust; they worked to build a global trading system that included both countries while insisting that both focus on growth through exports; and ultimately insisted that the United States fight inflation, because its currency remained the anchor for the financial system they had just insisted Japan and Germany join. True, American aid facilitated these aims. But again, the mere fact that the United States provided goods for other countries does not make that aid "Keynesian," for the reasons stated above. More to the point, the aid had to be American because America had the goods. No amount of German or Japanese deficit spending could change that fact.

Ultimately, Eisenhower saw some deficits. He also spent nearly all his political capital closing those deficits as quickly as he could. But again, that is not entirely the point (or the focus of my book). In making policy, he never talked about guaranteeing full employment through government spending. He never blamed liquidity traps for an economy running below its potential, or feared "animal spirits" among the investor class. He worked hard to limit inflation because he feared what would happen to a global economy tied to the dollar if America's trade partners lost confidence in its currency. "Good Keynesian" is a weird misnomer for Eisenhower.

I have left my response to Aaron O'Connell to the end because I am not sure what to say. He writes that as a "military history" my book "falls far short of the mark." Given the way he reads it, I can't see how it could have been otherwise. If, like him, one dismisses the economic heart of the book as so much fuss over economic "common sense," then reads the institutional aspects of the story exclusively through the lens of military history (ignoring the American-political-development-cum-policy-history lens I try to employ), and then dismisses much of the intellectual history because, well, the source of so many good ideas never served in uniform, how could my book not seem like a failure? As he excludes from the outset much of what the book attempts to accomplish, it is no wonder O'Connell finds himself perusing the footnotes for spelling errors. What else does he have to add?

I suspect that he had hoped that my book would look a lot like his *Underdogs: The Making of the Modern Marine Corps*, a good book to be sure. To speak to his sensibilities, I should have blended organizational analysis with cultural history, which would illuminate the way army leaders developed governing capacities as well as a governing ethos that would ultimately become a coherent set of doctrines, capabilities and ideological commitments within an evolving United States Army. I should have elaborated "what the specific military missions were in the Philippines, Panama, or in the Rhineland" and, in addition, should have considered "how many soldiers served in the occupations, what they did there, how the occupations were organized or led, or whether the missions succeeded or failed according to the goals set by the commanders and the president." At least I think that is what he expected. Obviously, I did not write that book. But I agree with him that such a book is long overdue.

In any event, his is not the review I expected either, which is disappointing since I like his scholarship very much and thought there might be some interesting overlaps in our work. So, in hopes that turnabout is fair play, let me describe what I had hoped to get from him. In my ideal review, O'Connell offers a few thoughts about how we should relate military history to intellectual history, policy history, and/or American political development, and he brings up some connections to his own work. He expands on the way military historians have thought about non-military actors who nevertheless had an impact on the things he cares about: organizational capacity, public relations, and institutional culture. He takes up, for example, that part of my book when Joseph Dodge wanders the halls of Congress, drumming up support for the currency conversion plan. He even considers the clever way Lucius Clay and William Draper take advantage of the "missions" of businessmen to advance the military government's policy aims. In my ideal review, he notes that these episodes dovetail nicely with his own work, which also considers the clever ways military leaders legitimize their organizations with Congress and the American public.

American historiography tends to call "Keynesian" any government activity designed to generate economic growth. Whether it is Marshall aid, or tax cuts, or perhaps even land-grants, as long as the state is doing something to spur the economy, it gets labeled "Keynesian."

Eventually, in my ideal review, O'Connell explains how military historians make sense of people like Clay or Eisenhower after they transition into professional and political life. Finally, he offers some pointed criticisms along with avenues for further thought about the external state (a term he never mentions) and military government. Perhaps he even brings his critique into the present, contemplating specific parts of the occupation in Iraq that were subcontracted to private firms like Haliburton and expressing insights about how military historians make sense of shared duties between private actors and army officers. Here he might even offer some thoughts based on his own military experience, thoughts that speak to the concerns raised by Michael Hogan and Curt Cardwell about exactly how free military governors are to make their own policy.

Of course, these were my expectations and concerns, not his. He is fully entitled in his review of my book to defend his intellectual turf at my expense. But I would have liked a little credit for inviting O'Connell to maybe share a bit of his turf and try strolling around some of mine. In the end, though, his review mostly just said "Get off my lawn!"

Note:

1. J. Bradford DeLong, "America's Only Peacetime Inflation: The 1970s," NBER Historical Working Paper, no. 84 (May 1996), 7.