

# Between Regions: Science, Militarism, and American Geography from World War to Cold War

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Histories of American geographic thought and practice have sketched, but not critically explored, the relationship between war, intellectual change, and the production of spatial knowledge. This article sheds light on a crucial period, the middle decades of the twentieth century, when new modes of understanding and representing geography were being formulated at a variety of sites across the nation-state, from Princeton to the University of Washington. In particular, there emerged an altered conception of *region*, not as a descriptive but as a theoretical unit. This intellectual transformation, driven by an invigorated scientific imperative, was closely wedded to broader geopolitical conditions of war and militarism—to the demands for synthetic regional intelligence and new collectives of research that could adequately address complex technical and social challenges consistent with global influence. Moving from the formative hub of the Office of Strategic Services to the more diffuse but no less powerful structures of Cold War funding, we chart the emergence of a new regional model, inextricably linked and concurrent with the solidification of a world of strategic regions open to the exertion of American power, but also part of a remarkable emergent technoscientific complex at home. *Key Words:* Cold War, region, Second World War, technoscience.

Kirk Stone, who received his offer to join the Office of Strategic Services the day after Pearl Harbor (Dobson et al. 1999, 537), may have been exaggerating when he said: “World War II was the best thing that has happened to geography since the birth of Strabo” (Stone 1979, 89), but clearly the Second World War and the early years of the subsequent Cold War coincided with a significant shift in American geographical thought. The hallmark of the change was an altered idea of science, made manifest in a different conception and treatment of “region,” long a cornerstone of geographical inquiry.

The traditional notion of science held by geographers arrived from natural history, which was field-based, descriptive, rested on scrupulously recorded observations of a lone scholar, and tended toward classification, even the encyclopedic. Regions were portrayed correspondingly. During the Second World War and afterward, however, a different model of science emerged, one produced in the crucible of war, both hot and cold, and forged through interaction among scientists, the military, industry, and the state. This science happened at the lab bench or at the writing desk, involved large sums of money and a team of researchers (“big science”), was theoretically abstract, mathematical, often model- and machine-based, and geared toward meeting specific ends. We argue that this conception of science made its way, albeit haltingly, into postwar American human geography.

Accordingly, it produced a very different idea of region, conceived now as explanatory, theoretical, and instrumental, a tool to achieve functional objectives.

This new conception of science, bound up with mid-twentieth century geopolitical upheaval and pervasive militarism, changed the intellectual trajectories of a number of natural and social sciences in the United States. To use Andrew Pickering’s (1995a) term, the Second World War and subsequent Cold War represented an *epochal change*. The world, including the world of the mind, was ruptured and remade. Paul Forman’s (1989) investigation of the effect of the Cold War on physics was the first of the studies to make this point clear, and since then detailed histories have been written of psychology (Herman 1995), economics (Mirowski 2002), anthropology (Price 1998), molecular biology (Kay 2000), and the philosophy of science (Reisch 2005). The specific consequences and mechanisms of rupture varied according to the discipline, but in each case Second World War and Cold War science left its mark, molding for decades to come a subject’s methods, practices, internal sociology, and objects of investigation. We argue that geography should also be on this list; it represents an especially interesting case study given that it straddles science and the humanities.<sup>1</sup>

Apart from this historical imperative, this article is also impelled by theoretical arguments within science studies, particularly Andy Pickering’s (1995a, 1995b)

work on what he calls the “World War II regime,” and Donna Haraway’s (1997, 15) attempt to understand “contemporary millenarian technoscience,” the roots of which for her lie precisely in World War II and the Cold War (Haraway 1997, chap. 2). Pickering and Haraway are useful because both are concerned to understand the formation, coherence, and influence of the large scientific interdisciplinary assemblage that first emerged in the United States during the Second World War. Pickering’s interest is in the origin of the radically different matrix of science and associated practices that surfaced. For him, it occurred because of what he calls “mangling,” an interactive transformative process mixing and integrating elements in combinations never seen before. Haraway’s concern is with the power of technoscience in effecting large-scale change by “hailing” new subjects and enrolling them into that assemblage. Our argument is that the Second World War and Cold War science assemblage, because of its ability to mix and integrate (“to mangle”) as well as “to hail,” enlisted geography, resulting in a dramatic disciplinary rewriting of the meaning of science and consequently of the very idea of region.

This article is divided into four sections. First, we situate our argument within the larger corpus of work on Second World War and Cold War cultures of science and social science. We begin with some brief theoretical points and then sketch the salient historical background. Second, by way of a benchmark, we discuss briefly the conception of science and the place of region within prewar American geographical thought and, in particular, Richard Hartshorne’s (1939) formulations in *The Nature of Geography*, still viewed as a classic disciplinary statement (but like most classic disciplinary statements, rarely read). Two years after Hartshorne produced his tome, America was at war and Hartshorne himself occupied a key administrative position within the Office of Strategic Services (OSS), the agency established in 1941 to provide military and political intelligence to the President, and the forerunner of the Central Intelligence Agency (CIA). Hartshorne was not the only geographer employed at the OSS, however. At its height, more than a hundred geographers worked there. The article’s third section, then, illustrates how the experiences of carrying out operations at OSS, at least those of a few key individuals who were later to be so important in shaping postwar geographical thought, helped to forge a different conception of science, and thus of region—one that was increasingly instrumental and applied. OSS geographers recognized that achieving the military and political ends mandated for the organization required systematicity, explanatory purchase, and practical focus—all difficult

to realize with the older conception of geography and the methodologies underpinning it. Fourth, we discuss how this new sensibility was subsequently further developed during the Cold War as geography was formalized as a spatial science of society. In turn, this formalization produced and depended on a conception of region as a generalized structural phenomenon, subject to uniform underlying forces that could be identified and instrumentally directed, and given rationale and legitimacy by Cold War science.

## The Conception of Cold War Science

Military power extended into the world of the mind.

—(Kay 2000, 11)

During the Second World War and the early Cold War, the entire Earth became a generalized space of American military strategy. Central to that endeavor was a new conception and practice of science and, increasingly, social science. In his 1941 President’s Report, the Massachusetts Institute of Technology’s (MIT’s) Karl Compton (1941, 369), writing from the campus with the most at stake in wartime scientific research, perceived “the outlines of an educational and research institution based upon the present ideals and objectives but incorporating a greatly magnified capacity for national service.” Compton’s vision was realized: by the end of the war, MIT was America’s largest university defense contractor, and it fought for that position throughout the Cold War. The Institute’s annual reports of the 1940s and 1950s were rife with exhortations to meet the “inescapable demand . . . to serve the national defense and strengthen the free world” (Massachusetts Institute of Technology 1954, 10). Compton’s counterpart at Harvard, James Conant, was similarly convinced about the importance of science. He formed a Committee on the Physical Sciences at Harvard during the war, and joined Compton on Vannevar Bush’s National Defense Research Committee. These commitments continued after the War ended. In a 1947 address, Conant outlined a “special sense in which science is called upon to help out with national problems here in this country” (Conant 1948, 77).

More generally, the Second World War lent legitimacy in the United States to the coupling of scientific and social scientific knowledge to national interest expressed militarily, a relationship that extended into the Cold War. The resulting “military-industrial-academic complex,” as Senator William Fulbright labeled it (Kay 2000, 10–11; see also Leslie 1993), administered by scientific

managers such as Conant, Compton, and Bush, in turn produced a new form of science and social science. By drawing on science studies, we first sketch the emergence and elements of that complex; and second, we identify its specific substantive characteristics.

### The World War II Regime and Technoscience

The sociologist of science, Andrew Pickering (1995a, 5), following Michel Foucault's vocabulary for marking off abrupt discontinuous epochs, applies the term "World War II regime" to understand the form science and social science took during the Second World War and Cold War. It was a regime marked by disciplinary and material transgression, by what Pickering calls "cyborg history." Lubricated by wartime bodies such as the National Defense Research Committee and later the Office of Scientific Research and Development (R&D), the hitherto relatively distinct entities of the military and science were during the Second World War forced to engage one another, resulting in what Pickering calls "mangling."<sup>2</sup> Mangling is the dynamic, mutual transformation of entities as they interact. In this case, science and the military were mangled. Their very practices changed substantially as they worked together, became quite different, and never returned to their respective original forms. Pickering (1995b, 239) maintains that

The intersection of science and the military in World War II can thus be understood as a macromangling that encompassed both an inner transformation of these two macro-actors and an outer transformation in their relationship to one another. The way of doing science changed from small to big science; the military shifted its tactics and basic disciplines; both institutions were topologically transformed in a reciprocal transformation of shape. . . . and all of these transformations were interactively stabilized in relation to transformations in machinic culture (symbolized by developments in radar technology).

The machinic reference is important. Pickering is keen to emphasize that mangling is not restricted to macro-actors, but occurs at every level, involving humans and nonhumans, including machines such as radar and, crucial to geographical research (as we shall see), the computer.

Mangling for Pickering provides a means for understanding the changing form of science and social science during the War. In order to meet military purposes, he argues, both became mission-focused, team-based and interdisciplinary, hierarchically organized, state-funded, machine-oriented, and, owing to reduced interest in pure theory and more interest in application, model-

based. Of course, the military itself was also altered, adapting its tactics and strategies to new technologies and devices, as well as to novel techniques, forms of information, and logistical models. As Pickering (1995a, 18) notes: "What had been largely separate and autonomous institutions before World War II—science and the military—had been profoundly transformed and locked together as a complex, social, material, and conceptual cyborg entity by the end of it."

Donna Haraway is also keen to use the term *cyborg*, and for the same reason as Pickering. It connotes fluidity, and transgression; cyborg entities are not pure and singular, but are frequently blurred, multiple, and changing, assembled from diversity. Science is one such case. It appears pure and solitary, hermetically sealed, but it is heterogeneous and open, with its intellectual and material boundaries continually shifting, as they did during the Second World War. For this reason Haraway prefers the compound word *technoscience*. It "designate[s] dense nodes of human and nonhuman actors that are brought into alliance by the material, social, and semiotic technologies [and] through which what will count as nature and as matters of fact get constituted" (Haraway 1997, 50).

Where Haraway differs from Pickering is in her emphasis on power. Pickering is content to let the mangle roll on, viewing what is fed in and what emerges at the other end as nothing but the result of contingency. In contrast, Haraway identifies larger forces at work: "the world-building alliances of humans and non-humans in technoscience shape subjects and objects, subjectivity and objectivity, action and passion, inside and outside, in ways that enfeeble other ways of speaking about science and technology. In short, technoscience is about worldly, materialized, signifying and significant power" (Haraway 1997, 51). To understand further that power, she draws on Louis Althusser's (1969) notion of "interpellation," which he employed to understand "how ideology constitutes its subjects out of concrete individuals by 'hailing' them" (Haraway 1997, 49–50).<sup>3</sup> For Haraway (1997, 51) technoscience also interpellates and hails, and not only humans but also nonhumans, enlisting them into new "world-building alliances." The Second World War and even more so the Cold War were shaped by ensembles of technoscience that hailed people and things, including geographers, incorporating them into new world-building alliances, and resulting in a changed conception of region.

### The Character of the World War II Regime

Separate from these theoretical writings is a complementary literature describing the general substantive

characteristics of the World War II regime. Belief in the instrumental power of science in achieving national security interests is the first characteristic, found most starkly in the conviction that science, and its embodiment in technology, won the Second World War (a claim most obviously evidenced by the Manhattan Project). Such a belief was reaffirmed and further institutionally embedded in the early Cold War era. Vannevar Bush's 1945 report to the President, *Science: The Endless Frontier*, set the stage, yoking the acquisition of knowledge to American global power. With Europe still shattered by war, Bush argued the United States "had become not just the center but the *sanctuary* of science" (Manziona 2000, 32). Later, other Cold War scientific managers like Conant and Compton suggested that it was less that America had become scientific than that science had become American and was consequently indistinguishable from national geopolitical strategy. Conant, a chemist by training (his research was on poisonous gas), directly transferred military language into the domain of science. His Harvard "Nat Sci 4" course, created in 1947, sought to instruct students in scientific "tactics and strategy." He believed that the future of science, and indeed, modernity, was synchronous with the future of the United States, and that if correctly conducted science would defend against a looming Dark Age (Fuller 2000, 150–78; see also Conant 1947, chap. 4).

Second, it was widely believed that the social sciences would be efficacious only so far as they too were scientific. Daniel Bell (1982, 13) asks in his survey of postwar social science: "If the widespread mobilization of science, and the concentration of some specific objects, could produce scientific and technical breakthroughs, why could not a similar mobilization . . . produce similar results in the social sciences? . . . The social sciences were [to] becom[e] 'hard,' like the natural sciences." This intellectual interest squared with and was reinforced by the potential contribution of the social sciences to a *national* interest. The OSS's Research and Analysis (R&A) Branch was established in 1941 precisely to apply social scientific knowledge and methods to America's strategic needs in a global war. Despite including among its ranks critical sociologists like the German émigré and Frankfurt School member Herbert Marcuse, R&A actively defined itself in terms of scientific objectivity and the realization of a pure and presuppositionless logic (Katz 1989). Best represented by the work of OSS economists, who, for example, developed mathematical optimization models of air bombardment to calculate the maximal destruction for the least cost, objectivity and rigor were expected from everyone and, if not delivered, then authoritatively enforced.

The same mindset was deliberately carried forward into the Cold War period. Reflecting on the development of the human sciences in the period 1940–1960, Carl Schorske notes a "passage . . . from range to rigor, from loose engagement with a multifaceted reality historically perceived to the creation of sharp analytical tools that could promise certainty where description and speculative explication had prevailed before" (Schorske 1997, 295). Although most obvious in the massive postwar mathematization of economics as both theoretical modeling and statistical econometric evaluation, the move to analytical rigor was also found in fields as diverse as philosophy, political science, sociology, and, as we shall argue, human geography. This broader approach was signaled by the formation in 1952 of the Social Science Research Council's Committee on the Mathematical Training of Social Scientists. Scientists such as the engineer and physicist Lloyd Berkner (1960, 1377), a member of numerous Cold War projects, demanded as late as 1960 that social science "find elementary, fundamental, and independent concepts or parameters, whose coefficients can be determined numerically, and which combined in suitable mathematical formulations could predict analytically something about the ultimate capacities of the individual."

Third, the institutional sites where science and social science were carried out ensured multidisciplinary. The Manhattan Project, in place at thirty distinct American sites, most notably the trinity of laboratories at Los Alamos, Hanford, and Oak Ridge, permitted its leaders, General Leslie Groves and Professor Robert Oppenheimer, to recruit the best and the brightest from across the spectrum of the natural sciences, including many recent arrivals from Europe. Similarly, General William Donovan and Professor William Langer "conscript[ed] the leading thinkers in a dozen scholarly disciplines into the Office of Strategic Services" (Katz 1989, xi). Both examples reflect a novel model of research—often dubbed "big science"—in which enormous commitments of money and resources allowed a diverse range of personnel and expertise, along with heterogeneous materials, to work on a common problem, whether it was the construction of an atomic weapon or the provision of military intelligence (see Galison and Hevly 1992).

This model became the template for carrying out research after the war. Government and, increasingly, private funding brought together large teams of varied researchers to work on specific problems most directly connected to national security and military interests. Perhaps the best example is the RAND Corporation, based in Santa Monica, California. When founded in December 1945 within the Douglas Aircraft Company,

RAND reported to the Army Air Force. By 1948, it was a separate nonprofit organization, still tied closely to the newly created United States Air Force, concerned with providing interdisciplinary information and knowledge to the military in a climate of the gathering Cold War (J. A. Smith 1991; Hounshell 1997; Collins 2002). RAND brought together a who's who of American scientists and social scientists to work on a variety of problems turning on the "science of warfare" (Hounshell 1997, 244). Those employed by RAND included the scientific polymath John von Neumann, the economists Kenneth Arrow, and Tjalling Koopmans (both later winners of the Nobel Prize), the mathematician with the beautiful mind John Nash, and (at the Systems Development Corporation, a RAND spin-off) Waldo Tobler, who worked on a project to develop a computer-based early warning system for nuclear attack (SAGE). It was a result of that experience, and classes at the University of Washington with William Garrison (whose contributions are described in more detail in a later section), that Tobler wrote "Automation and Cartography," a key text for analytical cartography and the development of geographic information systems (GIS; Tobler 1959; see also Clarke and Cloud 2000). The wider point is that science and social science were carried out at such sites as teamwork, based on generous funding lines, directed *instrumentally* at a particular problem by drawing on a range of disciplinary proficiencies.

Finally, funding was critical to the types of research assemblages that formed. Initially provided almost exclusively by the U.S. military, it later widened to include other government agencies and philanthropies, and the latter were especially important for the social sciences. During the 1950s, 80 percent of Federal U.S. R&D expenditure came from the Department of Defense and accounted for two-thirds of all national R&D spent in aerospace and electronics during the period (Leslie 1993, 2). Initially, the social sciences were also overwhelmingly funded by the state, with the Office of Naval Research (ONR) particularly important in the early postwar years (in 1949 the ONR funded 40 percent of the pure and academic research in the United States; Mirowski 2002, 200). Later, the Ford and Rockefeller Foundations were increasingly active. After much wrangling, the National Science Foundation set up a subdivision in 1955 called "sociophysical sciences" that included human geography. Finally, in 1958 an Office of Social Sciences was established that admitted the remaining social sciences.<sup>4</sup>

A central question in the history of Cold War science is the effect of military and more generally outside funding in shaping the knowledge produced by research. The classic study is by Forman (1989), who concluded

that the immense amount of military money provided to physicists altered their previous research trajectory, changing the knowledge they created and the problems they studied. Since Forman's work, similar conclusions have been reached with respect to other sciences, including meteorology, oceanography, and geology, all targeted by the R&D Board of the Department of Defense during the early Cold War (Cloud 2003; Dennis 2003; Doel 2003; Oreskes 2003). The Department of Defense was also behind much of the research in remote sensing and development of GIS at places such as Ohio State University (Cloud 2000). John Cloud (2001, 240) speaks of that process of research as a "Shuttered Box" allowing "successful passage of people, money, ideas and technologies and data back and forth between [classified and nonclassified] domains, but without ever providing direct sight or communication between the realms. The Shuttered Box therefore preserves the security of the classified realm. At the same time it transforms or disguises the identities of the elements passing through it." Cloud and other scholars concerned with the genealogy of the geographic information sciences (GISciences) have done much to pry open that box.

Fewer historical studies of the social sciences exist (although Mirowski 2002 examines economics and Solovey 2001 provides an example of where proposed military funding spectacularly failed and undid the anticipated research). But during the early Cold War era, the larger culture of militarism influenced what counted as appropriate social science research. Two Social Science Research Council representatives, for instance, argued in a 1950 booklet on research for the federal services that although a "nation, a community, a family, cannot readily be put in a test tube," this did not mean that the search for "uniformities of behavior" should be discarded. Humans were still "subject to physical laws" (Social Science Research Council 1950, 12, 22, 11).

The knowledge created out of the Second World War's destruction and the Cold War's proxy conflicts and modernization projects was not innocent, but was shaped within a peculiar institutional permutation—the military-industrial-academic complex—that directly or indirectly promoted an American geopolitical agenda. The agenda was realized through the politicization of science and its technological products, the use of scientific methods, including analytical logic and quantitative techniques, within the social sciences, the development of a "big science" model of interdisciplinarity carried out at specific geographical sites to achieve definite ends, and the provision of very large sums of money primarily from military agencies to gen-

erate new, relevant knowledge. In Pickering's terms, there was much mangling as the military, sciences, and the academy, along with pieces of technology, techniques, and vocabularies, came together in hitherto novel combinations and, in that very emergent process, produced yet further novelty (Pickering 1995a). Moreover, those combinations were difficult to resist, representing enormously powerful actors and vast resources. When it hailed, as Haraway suggests, one usually listened, and was interpellated. That was the case with human geography. We argue that in the process its conception of science and associated notions of the region were altered and replaced by quite a different view. We begin with an older core idea and, in particular, statements made about it by Richard Hartshorne.

### Richard Hartshorne, Science, and Regional Geography

1938 was no time for an American geographer to be examining European boundaries with notebooks, maps and camera.

—(James 1972, 418)

Hartshorne called his work *The Nature of Geography* (1939), using the definite article and the singular form of the nouns. Not everyone agreed with the statement, and some violently disagreed, nevertheless many thought the book represented a crossing of the Rubicon. Touted as the most sophisticated geographical methodological statement yet made in English, the text meticulously explicated, rigorously justified, and genealogically fixed the discipline in terms of its relation to science and region like none before.

Initiated in December 1937 after an invitation from Derwent Whittlesey, the editor of the *Annals of the Association of American Geographers*, to write a "statement . . . it can be brief" (quoted by Hartshorne 1979, 63) on John Leighly's (1937) earlier paper published in that journal, Hartshorne's manuscript quickly got out of hand. By April 1938, it was 61 manuscript pages (Hartshorne 1979, 70). In July 1938, on the eve of Hartshorne's academic leave to Vienna where he planned to study boundary issues in the mid-Danube region, it was 194 pages (71). The Anschluss, however, put a stop to plans for fieldwork, and Hartshorne did what many academics would do in such circumstances: he went to the library. Consequently, when the manuscript was finally completed in April 1939 in Meilen, Switzerland, where he had gone for safety fearing a

German war with Poland, it was more than 600 pages (73).

Hartshorne conceived of geography as a science, although it was different from what he variously called the "exact sciences," "natural sciences," or "systematic sciences" (Hartshorne 1939, 115, 144). Geography was a science in the sense that it provided "organized, objective knowledge" (130) and its remit was "all facts of the earth's surface" (372). But clearly those facts needed to be organized. For Hartshorne this organization was to be realized chorologically: "geography will determine which facts it will utilize, not according to their substance, but according to . . . their relation to the areal differentiation of the world" (373).

It is here that region was so important. "The facts of the earth's surface" would be ordered regionally. As Hartshorne wrote, "the ultimate purpose of geography, the study of areal differentiation of the world [chorology], is most clearly expressed in regional geography" (Hartshorne, 1939, 468).<sup>5</sup>

Hartshorne (1939, 275) was well aware that regions were constructs, "entities only in our thoughts, even though [they] . . . provide some sort of intelligent basis for organizing our knowledge of reality." But this did not make regional geography futile. Whatever the precise boundaries of regions, it was still always possible to determine how "particular elements and complexes of elements within regions are related to those in others" (282) and in doing so fulfill the mandate of chorology.

Specifically, for Hartshorne the building blocks of regions, however they were eventually delineated, were complex combinations of hard facts and specific causal relations. Both the facts and relations were capable of objective disclosure (Enrikin 1981). These objective combinations that he sometimes called "element complexes" consisted of the facts of place and their interrelationships. Furthermore, their very combinatorial character produced uniqueness—that is, complexes not found anywhere else. Robert Sack (1974, 441) notes that "the specific region as described by Hartshorne is . . . synthesized from its parts and their interrelationships; [for that reason] it cannot be studied entirely in terms of generic concepts. It must also be 'regarded as unique in [its] *einmalige* combination of interrelated phenomena.'"

That uniqueness meant that traditional scientific explanation based on general laws did not apply. The type of explanation found in the exact, natural, and systematic sciences rested on asserting general (generic) relationships between homogenous classes of phenomena: if class of phenomena A, then class of phenomena B. But under Hartshorne's conception, the synthesis of facts and empirical relations constituting a region was

never the same anywhere else and, indeed, the region itself was not a “real” phenomenon. Scientific explanation in which instances of broader classes of phenomena were related by lawlike statement had no purchase. Consequently, as Hartshorne (1939) notes, “We arrive, therefore, at a conclusion similar to that which Kroeber has stated for history: ‘the uniqueness of all historical phenomena. . . . No laws or near laws are discovered.’ The same conclusion applies to the particular combination of phenomena at a particular place” (446). We cannot, therefore, explain, or predict, or knowingly intervene but only describe: “Regional geography, we conclude, is literally what its title expresses: . . . It is essentially a descriptive science concerned with the description and interpretation of unique cases . . .” (449).

A second feature of Hartshorne’s argument also bore on what was to transpire later, its self-conscious intellectual isolationism. Hartshorne did not engage other emerging social sciences. The reason was found in *The Nature’s* subtitle: “A critical survey of current thought in the light of the past.” Hartshorne’s project was to define geography’s essential nature on the basis of a historical review of the discipline’s origins, primarily German. Works that conformed to that historical development—that is, they adopted Hartshorne’s chorological conception of the region—were geographical, and works that did not were “deviations” and were not geography, whatever their authors might say (Hartshorne 1939, chap. 3).

Several consequences followed. First, regional geography necessarily became insular. The only sources of its definition were the practices of previous geographers. Potential contributions by other disciplines including the social sciences were put to one side. They were not relevant, external to “The nature of geography according to its historical development” (Hartshorne 1939, chap. 2). Second, regional geography was static because concepts of region taken from earlier geographers did not change. Hartshorne’s upholding of these frozen precepts of regional study kept the discipline in a cryological state. Finally, regional geography necessarily became protective of its boundaries. Without vigilance, “deviations from the course of historical development” (Hartshorne 1939, chap. 3) lurked just around the corner, with implications of contamination and defect. But with vigilance, the pure character of regional geography was sustained. The upshot, as Neil Smith (1989, 92) puts it, was that Hartshorne’s *Nature* “committed geography to a museum-like existence. The museum perimeter was jealously fenced by a ring of conceptual distinctions that kept geographers in and effectively discouraged would be intruders.”

This museum conception of region, as if it were preserved under glass, and the view that regions could not be explained by the methodology of natural science were both subject to increasing pressures and strains, and almost from the moment that Hartshorne first articulated them. Those pressures and strains did not originate so much from internal criticism, although there was some of that (Carl Sauer’s irritable response in the form of a December 1940 Presidential address to the Association of American Geographers (AAG) was the most immediate; Sauer 1941),<sup>6</sup> but from seemingly external events that produced a different conception of scientific practice. Before *The Nature* appeared in print, Europe was at war. And just over two years later, Hartshorne himself, along with a large number of other American geographers, began serving the state, as the United States itself entered into global conflict and later extended through other means and other foes into the terrain of the Cold War. American geopolitical maneuvering and strategy brought the greatest pressures and strains to bear on geographical thought and practice, exposing the discipline to a very different conception of science and in doing so shattering the old idea of the region and creating something quite different.

## Research and Analysis at the OSS

. . . half cops-and-robbers, half faculty meeting.

—(Johnson 1964, 3)

In September 1941, Richard Hartshorne was called to Washington, D.C., to form a geography branch within the two-month-old Office of the Co-ordinator of Information (OCI). In June 1942 the name was changed to Office of Strategic Services (Martin 1994, 488), and this was the forerunner of the CIA. That Office reported directly to the President and the Joint Chiefs of Staff. Its founding charter was “to collect and analyze all information and data which may bear upon national security” (quoted in Troy 1981, 423).<sup>7</sup> The subsequent importance of the OSS was immense. According to Andrew Kirby (1994, 306), it “created many of the blue prints for post-war US economic and military hegemony . . . [as well as] presiding over the emergence of essentially new conceptions of academic labor.” There were also smaller, more localized effects, one of which was the beginning of a new conception of region, propelling academic geography along a new intellectual arc.

Headed by a former Wall Street Lawyer, decorated World War I soldier, and friend of Winston Churchill, General William J. Donovan, the operations of the OSS

at 23<sup>rd</sup> Street and East in Washington, D.C., expanded dramatically once the United States entered the war in December 1941. Its staff grew from 2,000 in 1942 to more than 9,000 in 1945. The OSS was the single most important wartime institution employing American geographers, 129 at its height (Kirby 1994, 306; Rössler 1996; Harris 1997; Barnes 2006). Its functions varied from carrying out covert operations to psychological warfare to (almost) conventional academic study. The latter occurred under the umbrella of the R&A branch headed by the Harvard historian William L. Langer.

R&A was the key site within OSS, its “heart and soul” (Winks 1987, 114), responsible for collecting and analyzing data and information on every theater of the conflict. Winks (1987, 63) notes that “R&A controlled the most powerful weapon in the OSS arsenal: the three-by-five index card.” Described as the “Chairborne Division” (Katz 1989, xii), R&A was the branch in which Hartshorne held a key administrative position from November 1942: Chair of the Projects Committee that prioritized, oversaw, and vetted individual assignments carried out by R&A staff.

That staff was extraordinary. It consisted of three main groups: midcareer American professors including Langer and Hartshorne, typically conservative, but vigorously anti-fascist; young scholars, often graduate students, who included Walter Rostow, Carl Schorske, Charles Kindelberger, Arthur Schlesinger Jr., and even the Marxist Paul Sweezy, who collectively would later reshape the postwar humanities and social sciences; and European refugee émigrés including Herbert Marcuse, Franz Neuman, and Paul Baran, theoretical, left-wing, and prodigiously erudite (Katz 1987; Söllner 1990). Among the geographers were three future Presidents of the AAG: Hartshorne, Preston James, and Edward Ackerman. Other geographers included Edward Ullman, Chauncy Harris, Kirk Stone, and Arthur Robinson, director of the Map Division, which boasted the largest single contingent of geographers, thirty-eight (Harris 1997: 246; on the Map division, see Wilson 1949; Robinson 1979).<sup>8</sup> What they generated was a massive amount of textual material and information. With a total staff of always less than a thousand, and a third of those overseas, R&A produced by war’s end more than 3,000 research studies, 700 reports, and 3,000 original maps (B. F. Smith 1983, 371). In addition, through Wilmarth Lewis’s Herculean efforts at the Central Information Division, the OSS could draw upon more than a million  $3 \times 5$  file cards, based on original information sources, that were cross-indexed and included pictorial material (Winks 1987, 110). More generally, a particular form of knowledge was fashioned and used to further American

military and political interests. No nation had made such *systematic* use of the social sciences in the gathering and interpretation of military and strategic intelligence for day-to-day wartime operations. National interests would be pursued, as Donovan said, through “good old fashioned intellectual sweat” (quoted by Ford 1970, 148).

It might have been good old-fashioned sweat, but the intellectual practices produced were brand new or, at least, brand new to geography. The first practice was imposed cooperation with other disciplines, formalized in January 1943 when the discipline-based grid of organization at R&A was abandoned. Instead, “the primary lines of research work in the Branch [were] defined by theater areas.”<sup>9</sup> Within each, interdisciplinary collaboration was expected along the three main axes of R&A research: “economic capabilities,” “topographical intelligence,” and “political, sociological, and psychological characteristics.”<sup>10</sup> As Kirk Stone (1979, 91) recalls, “Commonly a team approach was used . . . [and] improvised for each assignment. Geographers usually found themselves working night and day with economists, historians or political scientists, or when sent elsewhere in Washington or the country to search for data, the contacts were professional military people, biologists, geologists, or climatologists.”

Interdisciplinary relations within and across the regional Divisions were not always harmonious. The geographer Preston James (quoted in Rössler 1996, 78) wrote to the economist Chandler Morse, Head of the R&A outpost in London, in August 1944:

Although there is complete understanding at the higher “echelons” (God bless ‘em) between geographers, economists and other breeds, we still have a certain amount of friction at the so-called working level. . . . The real problem is this: can two groups of people who work from such entirely different angles and for such opposite objectives ever be made to see that each gives only a partial picture, and for the complete picture both are necessary? Can the economist ever be made to stop speaking of the gathering of facts and the plotting of details on maps as a lower order of thought than that required for the building of formulae? And can the geographers ever realize that unless they devise more accurate and objective procedures they can not hope to achieve the results they wish.

In the end, though, there was no choice; it was an order. In Pickering’s terms, this forced interdisciplinarity within the social sciences, and the equally forced connection to the military, was a form of mangling, tangling, and pressing together practices that hitherto had been distinct and separate.

Certainly the projects in which geographers were engaged became increasingly mangled, such as the Joint Army-Navy Intelligence Studies (JANIS), the mandate of which was “to make available in one publication . . . all the necessary detailed information upon which may be based a war plan . . . in a given area.”<sup>11</sup> The result was a set of anonymous and confidential volumes about various strategic regions, each packed with photographs and comprehensive detail on resources, terrain, infrastructure, and other geographic features.<sup>12</sup> Representing both an unprecedented military interdepartmental and academic interdisciplinary cooperation, thirty-four JANIS studies were produced between April 1943 and July 1947 and formed the template for the CIA’s National Intelligence Surveys initiated just months after the Agency opened in September 1947.<sup>13</sup>

Kirk Stone was the first OSS representative on the Joint Intelligence Studies Publishing Board responsible for publishing JANIS reports. But he was frustrated by the lack of competence of Board members, calling them “dead-heads,” and writing to Hartshorne only a few weeks after he was appointed, “Perhaps this Board should be dissolved . . . before . . . it unduly wastes money that could go to the production of bullets rather than second-rate intelligence.”<sup>14</sup> Edward Ullman followed Stone as the OSS Board representative and ultimately became its director. His problem was less with Board membership than with how mangling was to be undertaken. Ullman thought it had best be done by revamping the notion of region. In a December 1944 memo on “topographical intelligence” he complained that too many of the past JANIS reports were overly focused on regional description:

Specialized knowledge of a subject is more important than knowledge of an area. . . . When the Research and Analysis Branch of OSS was first organized, it was set up primarily on a regional basis . . . [but] most of the product was poorly organized, unbalanced and of preliminary value. Later that branch was reorganized and some functional sections were set up . . . the result was a better, more useful product.<sup>15</sup>

This suggests that Ullman, trained in the regional geographical tradition at Chicago and Harvard, was beginning to think of region in non-Hartshornian terms, emphasizing “functional” (generic) relations rather than the strictly chorological approach. His comments reflect the second feature of intellectual practices at R&A, the emphasis on scientific standards and forms of inquiry.

Hartshorne was never opposed to scientific standards and very much believed in the possibility of the objective description of facts and empirical relations. Although regions were constructs, they were not fuzzy, subjective

entities. There was always a “there” there that could be represented in hard-boiled, factual terms. In fact, Hartshorne was chosen as the principal enforcer of objectivity at R&A, when in November 1942 he was made chair of the Projects Committee,<sup>16</sup> partly inaugurated to safeguard scientific standards of language, truth, and logic in the fledgling organization. In a Guide he laid out those standards:

It is of the utmost importance . . . [to] strive for the highest degree of objectivity. We should cultivate what might be called a clinical attitude. . . . The most obvious and yet most common crime against objectivity is the use of hortatory and value words and phrases. Generally speaking, ‘should’ and ‘ought’—not to mention ‘must’ are taboo. . . . Intelligence reports find their merit in terseness and clarity rather than expressive description. . . . Proust, Joyce, or Gertrude Stein would all be equally out of place in R&A.<sup>17</sup>

While there was this imposition of scientific standards of objectivity from the outside, inside the subbranches new scientific methods were applied to the problems of war (albeit not necessarily by geographers). Economics was the paragon case. Through its linkage to Operations Research (OR), a World War II invention combining mathematical theories drawn from physics and engineering to solve diverse military problems (Kevles 1979; Pickering 1995a; Mirowski 2002), economics was drawn into the world of wartime science. The Enemy Objectives Unit, an R&A outpost in London, was charged from September 1942 with identifying the most important German targets for Allied bombing raids (Katz 1989). For the economists, this was to be done rigorously and scientifically through a mathematical “Philosophy of Air Power” (Katz 1989, 117). For Philip Mirowski the work of economists at OSS and similar military agencies during World War II was a turning point. “Only from the 1940s onward has American economics assumed its characteristic modern format and scientific pretensions. . . . The American orthodoxy became more formal, more abstract, more mathematical, and more fascinated with issues of algorithmic rationality and statistical inference” (Mirowski 2002, 157). In Haraway’s terms, economics was hailed, one of the items mangled in the World War II regime along with physics, mathematics, and engineering, turning into something quite different.

A transformation not quite as extreme also occurred in psychology. The OSS’s Psychological Division, led by the University of California professor Robert Tyron, undertook an elaborate program of behavioral testing to weed out unsuitable recruits for espionage missions. Assisted by the anthropologists Clyde Kluckhohn and Alexander Leighton, along with the social psychologist

Kurt Lewin, OSS staff scrutinized the actions of more than 5,000 candidates. The summary report, "Assessment of Men," is dense and heavily mathematical, the product of "months of statistical calculation" aided by IBM. It describes a rigorous schedule of exams, interviews, group tasks, questionnaires, and physical activities, including a "map memory" exercise, all intended to shed light on general variables such as motivation, emotional stability, leadership, and initiative (OSS Assessment Staff 1948, 3–4, 30–31, 124, 467; Capshew 1999, 111–14). Some psychologists who worked on the Assessment project later regretted their actions or were troubled by the project's repeated "validation problems," but others carried selection procedures to the much wider testing group of the public at large, convinced "that they had a valuable contribution to make toward viable human relations" (Herman 1995, 9, 44–46; Capshew 1999, 5).<sup>18</sup> More generally, the concern with rigor, quantification, statistical generalization, and reliance on calculating machines found in the assessment study became key characteristics of postwar American academic psychology (Herman 1995).

At least within the nongeographical social sciences at R&A, strides were thus made toward scientism, mathematization, explanation, and the solution of practical problems. R&A was not about mere description, the construction of typological schemes, and finding uniqueness. Geographers barely contributed to the larger scientific project, although some like Ullman may have wished that they did. With Hartshorne as the final arbiter and author of seemingly the final word on a geographical practice that said geography was not a lawlike science, it was perhaps not surprising.

Critical responses by geographers to the perceived sluggishness and marginalization of their discipline did not surface until after the war. When the complaints came, they typically revolved around "various deficiencies . . . in the[ir] previous training," which "handicapped their ability to carry out their [wartime] work with maximum success" (Committee 1946, 206).

Edward Ackerman (1945) offered the first and the most blunt critique: "Wartime experience has highlighted a number of flaws in theoretical approach and in the past methods of training men for the profession." In particular, Ackerman focuses on two central inadequacies: an "inability to handle foreign language sources, and a lack of competence in topical or systematic subjects" (Ackerman 1945, 122). It is the latter, of course, that directly called out the deficiencies of Hartshorne's *Nature*. In a less-than-veiled dig at his former boss, Ackerman (1945, 122) argues that "The second [problem], lack of systematic speciality among geography

graduates, touches the heart of a well-known problem in our field—interpretation and its methodology. Although the main lines of methodological interpretation are familiar to almost every professional geographer, they might well be re-examined at this time, because of their bearing on future training and research in geography." To make the point even clearer he adds: "If our literature is to be composed of anything more than a series of pleasant cultural essays, . . . we shall do well to consider a more specialized, or less diffuse approach. The demands of a future peacetime are not like to prove any more tolerant of superficiality than the demands of wartime" (Ackerman 1945, 129). Ackerman was being hailed by the emerging military-industrial-academic complex, and in more ways than one, given that he ended up working full-time from 1955 for nonprofits, including Resources for the Future Inc. and later the Carnegie Institution of Washington, institutions that lay, like RAND, in the interstices of commerce, government, and academia (White 1974).<sup>19</sup>

The National Research Council Committee on Training and Standards in the Geographic Profession offered less forthright criticisms, even vacillating among positions (Committee 1946). This is not surprising given that the Committee boasted fifteen members and that Richard Hartshorne was the chair. On the one hand, in a section on "Future opportunities for research in geography," there is a restatement of the Hartshornian regional line: "geographers will be expected . . . to serve primarily as 'hewers of data and drawers of maps.' One is not to be discouraged by that attitude. These are contributions highly valued in applied research. . . . In most cases, what geography can provide can be demonstrated only by accomplishments, not by theoretical argument" (Committee 1946, 203–4). But in the following section, "Lessons learned from the war experience," the grievances begin (Committee 1946, 207, 209), directed exactly at the deficiencies of the old regional approach, including its lack of a sense of a problem (there is "only . . . an elementary detailed description of pattern"), its lack of relevance ("heavily overloaded with unnecessary material"), its lack of precision ("professional geographers . . . will require much more grounding in statistics"), and its lack of knowledge in systematic fields ("The experience in Washington indicates that too many geographers were found to be naïve or superficial in their approach to economic and political problems on which they had to contribute").

Ackerman's piece was explicit, the Committee's implicit. Geographers, apart from cartographers, had ultimately not fared well in wartime intelligence service because they had been previously directed toward a

conception of region that emphasized areal differentiation over systematic approaches, description over explanation, typology over theory, words over numbers, insularity over openness, and broad eclecticism over narrow instrumentalism. But this approach could not continue. The siren calls of the military-industrial-academic complex were getting louder as academics returned to their universities with new contacts and as new organs of the national security state emerged. Indeed, from its wartime experience, geography was already becoming increasingly enmeshed within the complex. Something had to give.

## The Cold War and the Science of Regions

### Closing Geography at Harvard

... the one big time of change, the time of the 1950s.  
—(Kish 1983, 207)

Two signs of something giving were institutional. The first was negative: the closing of geography at Harvard University in 1948, orchestrated by one of the key postwar science managers, James Conant. In N. early 1948, after much internal strife, Conant declared that geography was “not a university subject” (quoted in N. Smith 1987, 159); that is, it was not a science, at least, a science in the way it was becoming defined during the War and early Cold War.

The history of the Harvard closure is instructive. There was never a separate geography department at Harvard; it was a Department of Geology and Geography. What sparked the 1948 crisis was the proposed promotion of the geographer Edward Ackerman to Associate Professor. Ackerman was hired at Harvard in 1940 (White 1974, 299), but was rarely present on campus because of wartime (OSS) and postwar service. When the Department put him forward as an Associate Professor in 1947, it was informed by the administration in February 1948 that there was no such vacancy (N. Smith 1987, 157–60). Geography went on life support.

There was one thin reed of hope: the convening a year later of a Subcommittee on Geography charged with evaluating “the desirability of geography instruction at Harvard” (Glick 1988, 51). Another OSS alumnus, Edward Ullman, who had been hired in 1946 as an Assistant Professor, was the sole geographer appointed to the subcommittee of eight chaired by the French historian Donald McKay, another former OSS member. For the next year, the subcommittee brought to Cambridge senior Anglo-American geographers to justify the dis-

cipline’s validity and to make a case for its existence at Harvard. While the panel recommended a resumption of geography, the recommendation was half-hearted because a majority could conceive of the discipline only in the old regional mold and, stuck within that cast, there was little to distinguish it.<sup>20</sup> McKay lectured Hartshorne, one of the “expert witnesses” called: “When the geographer deals with men, rather than physical facts, it appears he always marries with another discipline. When a geographer gets into another discipline he is obviously doing a less adequate job than the man trained in the other discipline” (quoted in Glick 1988, 53). Ullman was the dissenting voice, arguing that the very idea of regional geography should be rethought.<sup>21</sup> But he wasn’t heard. Before Ullman, in turn, left Harvard in 1951, he “went to bid farewell to President Conant, who remarked that he had had a wonderful primary school teacher who taught him where the rivers and mountains were. ‘That is why you abolished geography,’ Ullman told him, ‘we don’t do rivers and mountains anymore’” (quoted by Glick 1988, 52). Ullman’s response is a wonderful retort, but it needed to be written in the future tense. Geography’s Cold War ensemble had not quite realized itself yet, but it would.

### A Merger and a New Geography

The second development was more positive and began to set in place the institutional apparatus required to make Ullman’s present-tense assertion a future-perfect. It was the merger of the American Society of Professional Geographers (ASPG) with the AAG, also in 1948. Founded in 1904, the AAG restricted membership to approved scholars, permitting only such elected members to present papers at its annual meetings and to publish in its journal, the *Annals of the Association of American Geographers*. But few new AAG members were elected annually. For example, in 1936 only eight new members were elected into an organization of 200 (McCune 1986, 359). Purity of research, including publication in scholarly journals, was one criterion of membership. The effect, though, was to exclude applied geographers working for industry and government, as well as students. Furthermore, the AAG rule that any nominee could be blackballed by 10 percent of the membership vote ensured the party line was toed (McCune 1986, 360).

Out of collective frustration, an informal Young Geographer’s group emerged in 1936. This group later became the alternative ASPG, formally inaugurated in fall 1944 (Miller 1950, 30; McCune 1986). Most important is that the ASPG came into being because of the

relatively large numbers of young geographers who undertook applied work during the Second World War but who were ineligible for AAG membership. There was also a methodological impulse. In one of the earliest ASPG missives, William Van Royen bemoaned the “agonizing detail” of “microchorography,” which “in its search for minor facts . . . has often ignored major problems which are staring at us in the face” (quoted in Miller 1993, 24, 27). Clearly, this was a gibe at the orthodox view of geography and the place of region. Van Royen’s complaint was an implicit demand for more systematic approaches with practical applications. When the AAG and ASPG merged on 29 December 1948, at the annual meeting of the AAG in Madison, it was a signal that American geography was changing and preparing itself institutionally for what was to come. As George Kish (1983, 202) recalled, “After that . . . Wisconsin meeting in 1948 . . . nothing was ever quite the same.”

What was to come emerged hesitantly over the next ten years, culminating in the “quantitative revolution” of the late 1950s—the wide-scale disciplinary adoption of scientific theory and methods resulting in a reconceptualization of geography’s object of study, the region. At first there was much more scientific talk than scientific action. Eugene Van Cleef (1952, 654), for example, claimed in a 1952 issue of *Science* that regional differentiation was “untenable if it is not based upon certain fundamental and established principles, which may be utilized as standards of reference.” By making “a vast number of observations in which similar conditions occur, [geographers] may be able to generalize,” he said, producing a true science of geography (Van Cleef 1952, 654). Or more famously a year later, Fred Schaefer (1953) launched in the *Annals* a searing attack on Hartshorne’s conception of region that Schaefer labeled “exceptionalist,” proposing a methodological alternative based on systematic generic principles. “To explain the phenomena one has described,” Schaefer (1953, 227) wrote, “means always to recognize them as instances of laws.”

Geographers were mobilizing Carl Schorske’s “new rigorism,” or at least the terminology of that new rigorism; they were being hailed and interpellated. The result was a new version of region, fitting into the demands of the Cold War scientific landscape. Two examples make the point.

**Warntz and Social Physics.** The first is the work of William Warntz, a wartime navigator for the U.S. Army Air Force and later a research associate at the American Geographical Society (AGS) from 1956 to 1966. Working under the rubric of social physics (later renamed

macrogeography), Warntz collaborated frequently with the Princeton astronomer John Q. Stewart. Warntz first read Stewart’s (1945) book *Coasts, Waves and Weather for Navigators* while based at Gander, Newfoundland, in 1945. He described it as a work including “an exotic chapter describing potential of population and its sociological importance” (Warntz 1984, 141).

In the late 1940s, Stewart proposed an interdisciplinary project, social physics, which drew on mathematical principles to explain social patterns. At its base was the assertion “that the dimensions of society are analogous to physical dimensions” (Stewart 1950, 245). “There is no longer an excuse,” he wrote, “for anyone to ignore the fact that human beings on the average and at least in certain circumstances, obey mathematical rules resembling in a general way some of the ‘primitive’ laws of physics” (Stewart 1947, 485). At a 1949 conference sponsored by the Rockefeller Foundation, Stewart and like-minded attendees invoked cybernetics, OR, and other products of the Second World War to justify the extension of methods from the study of physical nature to the analysis of human nature.

Writing with Stewart from the mid-1950s onward, Warntz drew primarily on theories in meteorology and physics to reinterpret the region as a unit of analysis. Newton’s gravity and potential models were particularly important and were used to represent and generalize spatial interactions across a terrestrial regional landscape. For any given region  $i$ , it was possible to calculate its relationship with any other region  $j$  measured by potential interaction. For example, “annual gross economic population potential” for U.S. regions was calculated by Warntz for his Ph.D. thesis (later published as *Toward a Geography of Price*, 1959c).

In representing regions in this way, Warntz was clearly distancing himself from chorology and was snuggling up to Cold War science. As he wrote, “the tendency of American geographers [is] to be preoccupied with the unique, the exceptional, the immediate, the microscopic, the demonstrably utilitarian” (Warntz 1959a, 447). Instead, he was interested in “general laws . . . that . . . unify the individual, apparently unique, isolated facts so laboriously collected” (Warntz 1959b, 58).

Hailing Warntz were several elements of the Cold War complex. The first was military money, particularly from the Geography Branch of the ONR. While Warntz was at the AGS, ONR significantly funded his research and, after 1966 when he moved to Harvard as a Professor, it even paid a portion of his salary (Janelle 1997, 725).<sup>22</sup> ONR’s geography program, established in 1948 and later administered by Evelyn Pruitt, favored especially the kind of large-scale collaborative projects that

were technique-oriented, instrumental, and pursued by the new generation of scientific geographers that included Warntz (Pruitt 1979, 107).<sup>23</sup>

Then there were Warntz's interdisciplinary collaborations. Apart from his title of AGS Research Associate, Warntz held the same position in the Department of Astro-Physical Sciences at Princeton and was also a Visiting Professor at the University of Pennsylvania's Department of Regional Science, itself a mongrel discipline. Warntz, perhaps more than any other geographer of his cohort, mangled, pressed together ideas, disciplinary traditions, theories, numerical data, and technology in combinations never witnessed before. He embodied the very intellectual rationale of Cold War science.

Finally, Warntz gravitated ineluctably to the power of the machine par excellence of Cold War calculation, the computer. While his first potential computations were done by hand, on large, thudding Frieden mechanical calculators, his later estimates necessitated a computer. This need even extended to his spare-time project of constructing a  $2 \times 3\frac{1}{4}$  feet, three-dimensional, potential population map of the United States involving his pounding in 3,100 separate nails, the exact height and location of each calculated using an IBM 7090 computer (*The Princeton Packet*, 12 February 1964, 1; Janelle 1997: 724).

During the early Cold War, Warntz was interpellated, enrolled, and enfolded, mangled. He was reconceptualizing region before many realized it needed reconceptualizing. He was not a mere dupe of Cold War science, though, because he contributed to it. His books, maps, ONR proposals, computer calculations, and even woodwork projects were part of the very performance of Cold War science, bringing it into reality. Further, his creative leaps of translation, joining, for example, a seventeenth-century equation with 1940s data on population and income, also broke contexts, setting geography on a different path (Warntz 1959c). That said, without the larger circumstances of the Cold War, this work would be unimaginable, something all the more evident when those circumstances began to change.<sup>24</sup>

**Garrison and the "Space Cadets."** The second example turns on the work at the Department of Geography at the University of Washington in Seattle, perhaps the most important of the early sites of geography's quantitative revolution (Barnes 2004; Johnston and Sidaway 2004, 75–82). The two key faculty members were Edward Ullman, appointed in 1952 following his departure from Harvard a year earlier, and William L. Garrison, who came to the Department as an Assistant

Professor in 1950. We focus on Garrison because although he was the junior of the two he supervised more of the key graduate students at Washington (known as the "space cadets"; see Barnes 2004, 572), and his research and writings are illustrative of some important transformations in the pursuit of Cold War science.

Garrison, like Warntz, served in the Army Air Force, but in the Pacific theater. It was there, also like Warntz, that he was exposed to mathematical theory in "a tough course in dynamic meteorology involving thermodynamics and fluid mechanics" taught by the MIT meteorologist Henry Wexler (Garrison 1998). After the war, however, as a graduate student in geography at Northwestern University, Garrison was mired within the Hartshornian tradition. In fact, so disgruntled was he with his resulting regional-geography-based Ph.D. dissertation involving just "a lot of walking around, . . . classification and description" (Garrison 2002, 103), that he now says it was "the most fortunate thing that Northwestern University library lost it!" (Garrison 1998). He was hired in 1950 at the University of Washington, then spent the academic year of 1952–1953 at the University of Pennsylvania's Wharton School, where he was first exposed to Cold War money and science. Assigned to Project "Big Ben," a classified Department of Defense venture, he began to be mangled and hailed. His interaction there with economists, statisticians, and biologists whetted his appetite for "analytic work" (Garrison 2002, 105).

This analytic work began shortly after he returned to Seattle. The first of the space cadets, Duane Marble, entered the graduate program the year Garrison returned. Two years later another contingent arrived including Brian Berry, Ronald Boyce, Richard Morrill, and John Nystuen. That same year, 1955, Garrison gave the first-ever statistics course in an American geography department (Geog 426: Quantitative Methods in Geography—a "baptism of fire"; Morrill, 1984, 60), as well as two courses in location theory. It was also the first year that a computer, an IBM 604, was installed on campus.

The conception of region that emerged out of the University of Washington was abstract and instrumental, bearing on "significant, theoretical, policy and/or practical questions" (Garrison 1959a, 232). The theoretical conception, found particularly in the first of Garrison's important three-part review essay written for the *Annals* (Garrison 1959a, 1959b, 1960), drew on German location theory and its contemporary elaborations. That theory conceived of regional arrangements as a consequence of an underlying rational economic process that could be represented and analyzed mathematically. The

region was a geometrical expression of that process and was illustrated by a now-familiar set of nested hexagons of market areas following the work of August Lösch (1954), the theorist to whom Garrison gave the greatest attention in his review. As with Warntz's conception, this was everything that Hartshorne's region was not and everything that Cold War social science aspired to be: generalizable, mathematical, explainable, abstract, and rigorous. As Richard Morrill (1984, 59), who attended both of Garrison's first courses in statistics and location theory, explained: "Richard Hartshorne whose work we studied in detail was what we struggled against." In conceiving regions as Löschian hexagons, or as any of the other iconic geometrical patterns of location theory, Garrison and his space cadets brilliantly succeeded.

This theoretical conception of region, the Washington geographers also believed, would provide the possibility of instrumental intervention. Second World War and Cold War OR had already indicated that joining scientific theory with newly invented analytical techniques and empirical data could solve pressing practical military problems within a centralized command and control setting. By the late 1950s it was increasingly believed that those same techniques could be used to solve practical problems within civil society, such as those that faced by America's burgeoning postwar urban centers and regions, problems increasingly overseen and managed by a centralized state (see Light 2003). It was here that Garrison and his students positioned their new conception of region, allied with new analytical techniques drawn from OR, for *strategic* geographical interventions.<sup>25</sup>

The Washington cohort was presented with two exceptional opportunities to use both their theories and their techniques instrumentally. The first concerned Cold War civil defense. William Garrison's work was featured in a high-profile 1954 evacuation test dubbed Operation Rideout, a dispersal of Bremerton, Washington's population by car and ferry. In a memo to a National Research Council Disaster Studies Committee member, Garrison mentioned that his "observation team" had recommended the use of aerial photography and a "Time-Space Car Tracking Plan" to Washington civil defense leaders. His suggestions were obviously impressive, because he and a team of graduate students were soon enlisted to conduct research for the state's 1956 *Survival Plan* (Washington State 1956).<sup>26</sup> It is not surprising that the new conception of the region was so compatible with civil defense. Urban evacuation's lines of flight and the systemic grids of emergency response represented a rational ideal, but also the refined *unreality* of abstraction.

The second opportunity arrived in 1955, when the Washington State Highway Commission contacted Robert Hennes and Edgar Horwood in Civil Engineering at the University of Washington to examine potential "user and nonuser benefits" resulting from construction of a highway system around Seattle (Garrison et al. 1959, v). The project gained greater impetus when the Highway Revenue Act was passed the following year guaranteeing continued money. Financed for the first year at \$30,000, Hennes and Horwood invited Garrison onto the project, along with other disciplinary representatives across campus. It was the perfect assignment for Garrison and the space cadets, allowing them to hone their analytical and theoretical skills, to bootstrap-learn from other disciplines, to make a larger disciplinary methodological point, to show that geographers could make practical contributions without embarrassing themselves, and to be on the receiving end of Federal largesse. Garrison and his students produced what Richard Morrill (1984, 61), one of the coauthors, called a "revolutionary book," *Studies of Highway Development and Geographic Change* (Garrison et al. 1959). It remains a remarkable volume, crammed with calculations, data matrices, statistical techniques, costs curves and demand schedules, and conventional maps overlaid with numbers, arrows, starburst lines, and balancing equations. But the real revolution was the changed conception of the region as a theoretical object, a consequence of a radically changed view of science, Cold War science. As the Washington students (and Garrison himself) left Seattle for new jobs, they took with them these new conceptions, which permeated and later irrevocably altered American geographical thought.

## Conclusions

While William Warntz was busy on the computer calculating regional potentials, and William Garrison and his students were analyzing regional highway systems, the events of the Cold War piled up: the Berlin Wall, the Alger Hiss trial, the nuclear-arms race, HUAC and McCarthyism, the Rosenberg executions, Hungary, Sputnik, and numerous third-world revolutions backed or opposed by one superpower or the other. On the surface the two sets of events—those in academic geography and those of the Cold War—may appear to have little to do with one another, but we have argued the reverse. It is not that these specific Cold War events match up one-to-one with particular changes in the discipline, but in total over the long run they helped to reinforce and solidify a particular intellectual and institutional culture, "the World War II regime," that inex-

orably drew in (“hailed”) geography. Slowly, and not without contestation, geography entered the mangle and came out on the other side quite differently. In particular, we focused in this article on the mangling that occurred to the idea of region. The old conception became inappropriate, requiring significant alteration. This was the significance of Warntz and Garrison, and before them Ackerman and Ullman: they were the first to reconfigure the region, to bring it into alignment with the times, to mangle it, and to make it part of Cold War science and associated geopolitical imperatives.

Percival Pointsman, one of the characters in Thomas Pynchon’s *Gravity’s Rainbow* (1975, 49), a satirical novel about wartime military research and intelligence, says, “Suppose we considered the war itself as a laboratory?” This is no supposition. It happened. The Second World War and the Cold War, whether intentionally or not (and Pynchon implies intentionality), served as and produced a series of laboratories, experiments for producing new kinds of knowledge that spiraled out to refashion the world. These new kinds of knowledge were produced at now-famous sites such as Los Alamos, the RadLab, the Lincoln Lab, and RAND. A similar process occurred with respect to the geographic region. In the imaginative and material lab spaces of the Second World War and Cold War, the region was remade and reanimated, becoming a new kind of knowledge. As in all successful laboratories, this task required marshaling considerable resources and personnel. And again, as in all successful laboratories, what was produced inside changed what was outside. Louis Pasteur famously said, “Give me a laboratory and I will raise a world.” We have argued that sites like the Washington, D.C., building at 23<sup>rd</sup> Street and East housing the OSS’s R&A Branch, the Social Physics Lab at Princeton, and Smith Hall, home to the University of Washington’s Department of Geography, were the laboratories where a new regional conception for geography was raised, altering the discipline and the world, and given purpose as part of a broader landscape of militarism and war.

That a novel conception of region was the result for geographical thought and practice is crucial. The shifting language and practice of regional geography, from catchall description to an instrumental science, provide a guide through the thickets of Cold War scholarship, and suggest a means to locate the work of a Hartshorne or an Ullman in the realm of encounter that Andrew Pickering calls “the mangle”—in this case the vast and complex one set messily in the 1940s and 1950s. Yet it is this mangle, or more accurately geography’s place in the mangle, that is ultimately our concern here. This is not a matter of credit (though the accomplishments of geog-

raphers were significant), but it is a crucial matter of locating the history of a discipline beyond internal artificial barriers and recognizing the power of the mangle. In demonstrating that the discipline was very much caught up in the military-industrial-academic complex, the cyborg regime of technoscience, or whatever provocative descriptors are appropriate to the era in question, we point to the external, to the world outside where geography and life itself reside.

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## Notes

1. Two bodies of geographical literature bear on our project. The first is on the effects of war and the military in producing geographical knowledge (Heffernan 2000; Gregory 2004). That case is now well made for the period around the formal emergence of the discipline in the late nineteenth century in Western Europe, and into the period around World War I. Indeed, World War I gave rise to versions of the OSS, organizations in which academics, including geographers, were pulled into politico-military planning. The Comité d’Etudes, established in Paris in February 1917, and in which Vidal de la Blache played a prominent role, is one such case, and the Inquiry founded in April 1917 in New York, and in which Isaiah Bowman was key, is another (Heffernan 2000; N. Smith 2003, chaps. 4, 5). In neither case, though, were members of the Comité or the Inquiry involved in day-to-day military operations, a hallmark of activities at the OSS. More generally, like its popular manifestations (Schulten 2001), the formation and shape of academic geography were materially and intellectually harnessed to the armed forces, conflict, colonial exploration, and empire (Driver 2001). Our purpose in this paper is to extend this connection made for an earlier period to mid-twentieth-century America. The second body of literature is on the spatial character of science (Livingstone 2003 is exemplary). This literature

- rarely ventures into the twentieth century, however, and does not recover recent geographies of “big science” or the military-industrial complex (the work of Scott Kirsch is an important exception; see, for example, Kirsch 1998, 2000). Neil Smith (2003) has recently provided an impressive account of the “American Century” and the practice of science through a biography of the geographer, university administrator, and political advisor Isaiah Bowman. Bowman, however, died in 1950, just when the United States, often led by its armed forces, was beginning to seize, explore, and define a set of strategic environments, and when American geographical study was entering the significant new disciplinary phase that we will explore.
2. Just as geography has long been tied to imperial or state power, scientific and military imperatives have always been linked. But equally clear is the evidence that World War II represented an unprecedented coupling, one that was continued, and in certain senses expanded, during the “peacetime” of the Cold War.
  3. While Haraway’s use of Althusser’s term *interpellation* may raise eyebrows because of its apparent determinism, Althusser championed “overdetermination,” the idea that everything determines everything else. The “lonely hour” of the famous “last instance” never arrives (Althusser 1969, 113). More important for our purposes is the resonance between Althusser’s idea of “hailing” that Haraway invokes, and the attempts by others in science studies to capture how people, as well as things, come together to form common projects, and in the process are fundamentally transformed. For example, Bruno Latour (1987), from the perspective of actor-network theory, speaks about the process of translation in which the interests of people and things, including machines like the computer and radar, are made congruent, allowing their enrollment within a collective network.
  4. Available at <http://www.nsf.gov/pubs/stis1994/nsf8816/nsf8816.txt> (last accessed 20 April 2006).
  5. In part, this idea derived from the German tradition in which he immersed himself in the libraries at the University of Vienna, and in particular the writings of Alfred Hettner (alive for two more years after the completion of Hartshorne’s manuscript, although the two never met; Elkins 1989; Butzer 1989).
  6. Sauer (1941, 2) explicitly names Hartshorne as one of those responsible for “the Great Retreat” that surely refers in part to the increasing insularity of the discipline, and shunning of history, as it does to the movement away from environmental determinism, the proximate reference. It is hard not to think also that when Sauer (1941, 1) writes “we can hardly claim to be getting our chief intellectual stimulus from one another,” he had in mind Hartshorne’s just-published volume. The dislike was mutual: Hartshorne said to Glen Trewartha in April 1937 at the academic meeting that sparked the birth of *The Nature*, “It is time to question this great god west of the Sierras” (quoted in Martin 1994, 484).
  7. The July 11 Executive Order that created the OCI and the “Military Order of June 13, 1942” that transformed the OCI into the OSS are reprinted as appendixes in Troy (1981).
  8. An obvious social characteristic of all of these “defense intellectuals,” one that held well into the Cold War, is that they were overwhelmingly men. Katz (1989, 25) began to try to make visible “the women’s army corps of typists, secretaries, and filing clerks whom William Donovan tactfully called the ‘invisible apron strings of an organization which touched very theater of war.’” However, there is more to be considered than massive inequity in the gender division of labor. The work of defense intellectuals, and especially the language they use, can be stamped as masculinist. In her provocative examination, Carol Cohn (1987, 717) finds that masculinism everywhere and overt, from the metaphors used to the jokes that are told to “the drive toward competency and mastery, [and] the pleasures of membership in an elite and privileged group.” In an interesting wrinkle, however, Pickering (1995a, 9–10, fn. 11) argues that scientists, at least during the Second World War, were coded as female and the military as male, in what became a classical “seduction narrative. . . . The military went out to fight . . . while the scientists stayed at home. . . . At the same time the military often regarded the scientists as stereotypically feminine: wild, unpredictable, dangerous, unreliable, mad—out of control.”
  9. The four theaters were Europe-Africa, USSR, Far East (South, South East, and East Asia, Australia and Pacific Islands), and Latin America.
  10. Research and Analysis Administrative Regulation, 21 January 1943, RG 226 (Records of the Office of Strategic Services), Entry 1, Box 1, Folder 2, page 4, National Archives and Records Administration, College Park, MD (hereafter NARA).
  11. Memo: War and Navy Departments and OSS, 1 July 1943, RG 226, Entry 1, Box 1, Folder 2, NARA.
  12. For a representative example, available in the New York Public Library’s Humanities and Social Sciences Branch, see JANIS 150 (1945), *Joint Army-Navy Intelligence Study of Borneo: Resources and Trade*, chap. IX. Washington, DC: Joint Intelligence Study Publishing Board, May.
  13. Reviewing the state of American military geography in 1954, Russell (491) wrote, “Perhaps the finest example of wartime area reports were the Joint Army & Navy Intelligence Studies (JANIS). . . . [T]he range of topics covered, the variety of sources tapped, and the high quality of the writing in the cartographic work placed them among the major geographic achievements of recent decades.” The JANIS reports were the closest equivalent to the Naval Intelligence Handbooks produced at Oxford and Cambridge by British geographers during the Second World War (Clout and Gosme 2003).
  14. Kirk Stone to Richard Hartshorne, 13 July 1943, RG 226, Box 1, Folder 20, NARA.
  15. “Notes on organization of topographical intelligence,” 13 December 1944, RG 226, Entry 1, Box 1, Folder 18, NARA.
  16. The Projects Committee was the executive arm of the Board of Analysts that served as an intermediary between Langer and R&A’s functional intelligence divisions. The Projects Committee was initiated in July 1942, but Hartshorne did not become Chair until 17 November of that year. Projects Committee Minutes of Meetings, 18 November 1942, RG 226, Entry 59, Box 1, NARA.
  17. “Draft of proposed guide to preparation of political reports,” n.d., RG 226, Entry 37, Box 5, Folder 3, pp. 7–10, NARA.

18. Like other systemic methodologies, the “assessment method” later became a fixture in the management world.
19. Although Ackerman stayed in academia for a relatively short period after the Second World War, he continued to push geography into the Cold War era as part of mainstream social science. His short monograph, *Geography as a Fundamental Research Discipline*, published in 1958 three years after he left the University of Chicago, was influential, and was followed in 1963 by his important AAG Presidential Address, “Where is a research frontier?” Ackerman’s interdisciplinary experience at R&A was formative to the impulses represented in these publications. After he resigned from R&A to return to Harvard because of health issues, he wrote to Langer in early 1944 saying, “I returned here with a new confidence and a much clearer professional mind than when I left. I had an opportunity to think over the merits and shortcomings of my field, to drop some of the intellectual baggage I had accumulated in previous years, and to organize my intellectual skills in more useful tools” (Edward Ackerman to William L. Langer, 3 January 1943 [sic], RG 226, Entry 1, Box 4, Folder 11, NARA).
20. N. Smith (1987, 169) says that the “committee saw the field as hopelessly amorphous.”
21. This is indicated by the paragraph Ullman drafted for the subcommittee report detailing specific examples of an alternative regional geography (para. 8 reproduced in Glick 1988: 58–59).
22. Box 6 of the William Warntz papers (1955–1985), Kroch Library of Rare Books and Manuscript Collections, Cornell University, Ithaca, NY, contains much of the ONR correspondence and research proposals.
23. A March 1953 *Annals of the Association of American Geographers* advertisement for ONR research contracts in geography stressed research that “show[s] promise of developing important new techniques” and that “appear to be practicable for solution of . . . problem[s]” (*Geography Branch* 1953, 2).
24. Those changing circumstances coincide with Warntz moving in 1966 to the Laboratory for Graphic Design at Harvard, where he was even involved in yet one more (failed) attempt to revivify geography (Warntz 1984, 146). But time was running out on his version of Cold War studies. Skepticism was surfacing about the beneficence of science; social science itself was becoming more social, less scientific; Warntz’s colleagues at the Graduate School of Design were not so keen on his kind of interdisciplinarity; and even old reliable, the Geography program at ONR, reined back its grants to human geographers from the late 1960s. And so, like Ackerman and Ullman before him, Warntz left Harvard in 1971 (Janelle 1997). This did not imply geography at Harvard was finally and irrevocably dead, however. On 20 October 2005, the Harvard University Center for Geographic Analysis was inaugurated and based on GIS, the very technique that Warntz was helping to pioneer at Harvard. The current Center’s Director is no spatial scientist, Warntz double though. He is Peter Bol, Professor of East Asian Languages and Civilizations (Richardson 2005, 2).
25. One of the key techniques was linear programming, or activity analysis, a jewel in the crown of OR. It showed mathematically how a specific end could be best achieved given a set of constraints. As a technique, Garrison (1959b) thought it so important he devoted the whole of Part II of his *Annals* essay to it, demonstrating that linear programming was capable of providing solutions to geographical problems couched as location theory.
26. Memo, William Garrison et al. to Harry B. Williams (n.d.), Folder “Anthropology and Psychology: Disaster Research Group—Studies: Civil Defense Evacuation Test—Bremerton: Final Report, 1955,” National Research Council-National Academy of Sciences Archives, Washington, DC.

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