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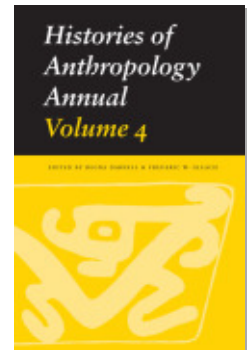
## On the Locations of Korean War and Cold War Anthropology

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## *On the Locations of Korean War and Cold War Anthropology*

*Robert Oppenheim*

The most famous instance of direct American anthropological involvement in the Korean War (1950–53) is barely recognized as anthropology at all. In December 1950, after the Inch'ŏn landing had driven back the initial North Korean advance and before the second capture of Seoul, three university-employed social scientists and a PhD-holding CIA and Air Force–affiliated psychological warfare specialist were hastily assembled and dispatched to the peninsula to study the North Korean occupation of the South and, by proxy, the Northern system itself.<sup>1</sup> A month later they just as quickly withdrew, but over the months that followed produced a series of classified and unclassified reports on “Sovietization” and the “impact of Communism” for their governmental sponsors, as well as scholarly articles and a popular book on the occupation of Seoul, *The Reds Take a City*, that would be distributed worldwide by the State Department as a staple anti-communist text. Of the members of the team, the most prominent then and since has been Wilbur Schramm, founder and head of the Institute of Communications Research at the University of Illinois. In both celebratory and critical scholarship, Schramm has been regarded as a central figure in the post-1945 development of communications as an academic discipline, and as a result the Korean War study has often been portrayed as a foundational moment in the birth of a field specially geared to the understanding of media-saturated mass society, to the extension of Cold War power and knowledge through technologies of consent, or both (Chaffee and Rogers 1997; Simpson 1994; Robin 2001). Yet the team of 1950–51 was multidisciplinary in both personnel and methodology: its other academic members were John Riley, at the time chair of the Rutgers department of sociology and himself subsequently a participant in the growth of communications, and the Harvard anthropologist John C. Pelzel, whose contribution, a study of two villages south of the 38th parallel that had undergone North Korean occupation, would set a model for similar wartime research by other United Nations and U.S. researchers.

Certainly one goal of this article, relative to other writings on the study that have focused on its place in the making of a characteristic post-1945 new discipline, is to bring to view its specifically anthropological address amidst institutional and intellectual developments that followed the end of World War II. This is not, in itself, easy to do. No single archive gives access to the events in question; such documents that do exist are scattered across a number of collections—with Pelzel, the anthropologist, probably the least well represented of the three academics. A step more broadly, this essay might be regarded as a case study of the Cold War liberalism that must be grasped in its centrality if we are to understand this formative period of postwar American anthropology. In our own post-1960s disciplinary moment, we tend to register a rather Whiggish shock at the prior entanglement of anthropology with military and intelligence projects of the Cold War state, or at finding anthropologists working within a problem definition that failed to regard the Korean conflict, for example, as anything but a transparency for global Soviet expansionism. Shock may have its ethical virtues, but it should not impede our attempt to gain a historical understanding of intellectual subject positions, neither McCarthy/MacArthurite nor anti-anti-communist, that complexly stretched through such overt and covert involvements rather than simply orienting themselves in full acceptance or opposition.<sup>2</sup>

Related to this is the still larger metaconceptual aim of this paper, which is to use the Schramm-Riley-Pelzel “Sovietization” study as a focus towards a consideration of how Cold War social science,<sup>3</sup> including Cold War anthropology, has been located in studies of the topic and how it might be relocated in future work. In his scathing critique of the foundation of the field of communications, Christopher Simpson (1994) situates the Sovietization study episode within a basically singular dynamic of the cooptation of American academics by military and intelligence-related funding and interests. The “science of coercion,” in his view, was inescapably linked to fundamentally anti-democratic ends of opinion manipulation through propaganda, psychological warfare, and the like. Ron Robin (2001:5) responds to such “conventional analys[es] of a one-way conduit of influence” by highlighting a greater diversity of academic engagement and response, noting that the “militarization [of academia], like industrialization, was complex and multifaceted: individuals and interests could grasp one aspect of it and resist another” (quoting Sherry 1995:499). Yet Robin (2001:13–15, 75–93) locates the Sovietization study within a different sort of encompassing knitted-togetherness, as a chapter in the “normal science” of a behavioralist

“paradigm,” terminology he borrows, of course, from Thomas S. Kuhn (1996). Thus both authors set the Korean War project within a larger unity, one of macrosociological military interest mostly taken as self-evident in its aims and expansiveness and one of intellectual orientation—it might be recalled that, while in the full flowering of Kuhn’s definition(s) a “paradigm” is not simply a conceptual lens, Kuhn drew crucially upon the perspectivist holism and strong notion of incommensurability of Gestalt psychology (1996:63). And both authors locate the defining dynamics they trace firmly within the United States as Cold War metropole. As a result, there is something of a layered irony: even as the general historiography of the Korean War and its background has moved away from superpower-centered Cold War understandings of that conflict as a proxy war in favor of a multi-sited examination of interacting domestic and international sociopolitical processes (Cumings 1981, 1990; Em 1993; Goncharov, Lewis, and Xue 1993; Armstrong 2003), the social science histories that aim to unpack the making and lacunae of Cold War constructs like “Sovietization” risk reproducing the metropolitan bias and logical internalism of these very constructs.

My concern is that histories of Cold War anthropology may be threatened by the same historiographical trap. My solution, discussed further in the conclusion of this essay, is to suggest that our own social science history might productively draw upon a different sociology of science than has usually been implicit, specifically in its spatial-ontological imagination. As a result, I do not ask in what the Sovietization study was *centered*, to find an answer akin to Robin’s behaviorist conceptual framework or Simpson’s fundamental logic of interest. Rather, I foreground its very *distributedness*: its co-location in varied projects, the translations of forms and concerns that constituted its aspects, and the articulatory “connections without collapse” that resulted.<sup>4</sup> In other words, I highlight not what it stably was, but the potential and practice of what and where it could be.

After outlining the origins and conduct of the study, the body of this essay considers three such distributions. First, especially if one is interested in the anthropological component of the Sovietization study in wartime Korea, it cannot simply be explained or contextualized with reference to the United States-centered practice of Cold War social science. The anthropology of the Supreme Command for Allied Powers (SCAP) American occupation of Japan provided an additional formative nexus of personal networks, practical habits, and intellectual engagements with the discipline. Japan, in other words, was more than a conduit for metropolitan Cold War anthropology; it imposed transformations. Second,

if one may certainly speak of a “militarization” of the academy as entrepreneurial social scientists forwarded military interests out of the habits of the World War and because of the funding and prestige on offer, military interests themselves were hardly singular in an early–Cold War era dominated by inter-service rivalries, uncertainty over the potential role of civilian science and social science, and debates about the nature and interrelations of conventional, nuclear, and psychological strategies. There was thus also room for some amount of intra-military entrepreneurship with which the Sovietization study was connected. Finally, as a study of “Soviet” (i.e., North Korean) practices of psychological manipulation, the project clearly sought to orient American psychological warfare, and among its products the popularized *The Reds Take a City* (Riley and Schramm 1951) was, from the title on through, itself quite deliberately an exercise in persuasion. Yet across its extension the Sovietization study also intersected with the non-identical ideological production of the nascent South Korean (ROK) state. This is to say that the morphing forms the study took were a result not simply of one metropolitan set of Cold War interests and ideological processes, however internally diverse, but minimally of two and of the tensions of their overlap.

### Peeking Behind the Curtain

The Sovietization study came together as a conjunction of multiple organizational efforts in the first months of the Korean War. Its direct sponsor was the Human Resources Research Institute (HRRI), an Air Force research organ attached to the Air University at Maxwell Air Force Base in Montgomery, Alabama—one of several social and behavioral science research agencies linked to the various services and the less-famous cousin of the Air Force–affiliated RAND Corporation. Interest in the mission was generated at the very highest levels. On October 16, 1950, the commandant of Maxwell, General George Kenney, wrote to Air Force Chief of Staff Hoyt Vandenberg proposing an Air University–HRRI team of approximately ten people to be sent to the Korean theater “to help extract some of its lessons.” In his subsequent October 25 letter to George Stratemeyer, commander of the Far East Air Forces (FEAF), Kenney was both pithier and more detailed in what he hoped might be accomplished. He reached for sporting metaphors to underscore the prevalent underlying assumption of the Korean fight as a proxy war: “We know who coached the North Koreans. The same coach is training other teams. In case we ever have to take on some of these as opponents, it would be a good thing to study the recent operations pretty thoroughly while there is yet time.” Kenney promised

Stratemeyer operationally-relevant results related to “human factor problems” of two basic kinds, to be addressed by two different research teams. One would visit bases in Korea, Japan, and Okinawa to assess issues related to the morale of combat air crews: whether numbers of missions or total combat hours were more relevant to combat fatigue, the effects of the presence of military dependents in the operational theater, the effect on morale of the “certainty of death or torture or both in the event of capture by the enemy,” and “the attitude of our combat personnel toward shooting up women and children engaged in carrying supplies to forward enemy positions”—as well as whether it mattered if such women and children were perceived as North Koreans or South Koreans operating under duress.<sup>5</sup> Meanwhile, a second group would consider questions related to both Air Force and enemy conduct of psychological warfare. “Korea,” Kenney wrote, “is the most available laboratory for the study of Communist military control and the effectiveness of our own psychological warfare campaign. We have dropped millions of leaflets. We should know how effective they were.” This proposed second team would give birth to the Sovietization study. In overall charge of the mission, and to coordinate between the needs of the research team and Air Force commands in Japan and Korea, Kenney proposed to send HRRI’s senior military official, Colonel George W. (“Pete”) Croker (ICR B5 “Air Force—Korean Mission Business 1950–51”: Kenney to Vandenberg, October 16, 1950; Kenney to Stratemeyer October 25, 1950, November 16, 1950; for Stratemeyer’s agreement AFHRA microfilms reel 4362 Iris A2573; Stratemeyer to Kenney, November 2, 1950).

Yet the genesis, form, and personnel of the study would also illustrate in microcosm broader connections between anthropology, larger post–World War II military-social scientific relations, and specific patterns of research tied to the reason of the American administration of defeated Japan. On October 17, the day after Kenney wrote his letter to Vandenberg, the anthropologist John W. Bennett wrote from Tokyo to Clyde Kluckhohn at Harvard with what he described as “a shot in the dark—or perhaps a shot *at* the dark.” Bennett, then director of the Public Opinion and Sociological Research (PO&SR) Division of the Supreme Command for Allied Powers (SCAP) American occupation of Japan, suggested that his own organization might take advantage of the “opportunity for the first-hand study of communist communities and their aftermath” presented by the impending American takeover of North Korea.<sup>6</sup> He assumed that Kluckhohn, at the center of social scientific relations with government in his capacities as director of Harvard’s

Russian Research Center and of the Strategic Planning and Intelligence committee of the national Research and Development Board, might have similar plans, and expressed the possibility that in whatever resulted “‘your people’ [could] also be our people,” whether through direct collaboration or a more informal supportive role (CKP B4 “Bennett, John”: Bennett to Kluckhohn, October 17, 1950). Kluckhohn wrote back on November 3 to indicate that, indeed, plans were underfoot and that he had alerted “various Washington agencies” to the existence of PO&SR (CKP B4 “Bennett, John”: Kluckhohn to Bennett, November 3, 1950). Meanwhile, he seems to have included Bennett’s letter in his own communication with Raymond Bowers, the civilian sociologist who was the director of HRRI—the same institution that was then sponsoring the Russian Research Center’s larger project on the Soviet social system (cf. O’Connell 1990). The “packaging” of PO&SR research interests into the HRRI project on Sovietization in Korea via the mediation of Kluckhohn, Harvard as an interdisciplinary center, and the interest in ostensible “satellites” of the USSR generated by the Russian Research Center’s investigation of Soviet strategic vulnerabilities was nowhere more evident than in the selection of Pelzel to be part of the team. Pelzel had preceded Bennett as the first head of the PO&SR Division in Japan before the job changed hands in mid-1949, had completed a dissertation with Kluckhohn as one of his supervisors, and had taken up an assistant professorship at Harvard on Kluckhohn’s strong recommendation. On meeting him, John Riley would be highly impressed, and would clearly regard Pelzel as the area expert in the bunch, describing him as “a scholar and man of the world” who “speaks some eight languages fluently including both Japanese and Chinese and has spent long periods in the Far East” (cf. CKP B20 “Pelzel, John”: Kluckhohn to Buck, January 25, 1950; JRP: Riley to Riley, n.d. [“Saturday”]).

The study team was assembled on short notice in mid-November, and asked to arrive at HRRI headquarters in Alabama by November 25. John Riley and Pelzel were the first to be added. Pelzel, described by Riley in a letter home as “a friend of Talcott’s and Helen [Parsons]’s,” was said to have been “practically ordered by the higher-ups in Harvard to come on this expedition”; Kluckhohn helped prevail upon the Harvard Corporation to allow Pelzel the necessary leave. Notwithstanding the centrality that has been filled by his role in subsequent writing on the study, Schramm was the last to be contacted, perhaps as late as November 20. His place had originally been assigned to the sociologist John Useem, who had accepted the assignment but then cancelled at the last minute (JRP: Williams to Riley, November 16, 1950, Riley to Riley, n.d.

["Saturday"]; ICR B5 "Air Force—Korea Mission Business 1950–51": Schramm to Stoddard, November 20, 1950). After a few days with "Ray Bowers and his crew," the trio traveled together to California's Travis Air Force Base, from which they were scheduled to depart for Tokyo on Friday, December 1. Delayed by winds over the Pacific, they apparently left early the next week; Riley wrote his first letter from Tokyo on Thursday, December 7, after a forty-hour flight. Throughout the planning stages, there was much anticipation that the war situation would not actually permit research in Korea, but after receiving go-aheads from both Seoul and Alabama, the three made their way to Korea on December 9 "in Gen. MacArthur's plane with no less than three generals" (CKP B20 "Pelzel, John": Pelzel to Kluckhohn, December 17, 1950; JRP: Riley to Riley, n.d. ["Saturday"], n.d. ["Thursday"], December 8, 1950, December 12, 1950).

Pelzel, Riley, and Schramm seem to have gotten on well with each other throughout the study period, but at least the first two were much less fond of the fourth central member of the research team, attached from HRRI itself. Frederick W. Williams was an assistant director of HRRI, in charge of its Psychological Warfare Directorate and thus the senior research official on the ground in Japan and Korea. After receiving a PhD in Social Philosophy and Social Science Methodology from New York University in 1940, Williams held a position as a research associate at Princeton University before becoming Research Chief of the U.S. Office of Military Government in 1945, joining the Central Intelligence Agency in 1949, and being assigned to HRRI in April 1950 (ICR B5 "Air Force Correspondence 1950–51": HRRI 1951 annual report:42). His role with respect to the Sovietization study was partly logistical, and he traveled ahead of the group at each stage—to Tokyo and then to Seoul—in order to make arrangements. At the heart of Williams's tension with others in the group was political ideology. John Riley wrote home,

I'm afraid that the three of us (Bill [Schramm], John [Pelzel], and I) are destined to suffer considerable frustration at Fred's hands (as you predicted), perhaps not so much on the professional as on the personal level. He is extraordinarily difficult to live with, being a man with many values that are almost vicious, and heavily oriented to war and the use of force. He is, furthermore, aggressive about his ideas and you can imagine it's hard to take as a steady diet. The three of us really don't dare to fight with him for fear of creating a perfectly impossible working relationship. But, thank goodness, this is of a fixed duration. (JRP: Riley to Riley, December 22, 1950)



Pelzel, writing to Kluckhohn before he, Schramm, and Riley had left the United States, initially described the three as “all a little leery of Fred Williams” (CKP B20 “Pelzel, John”: Pelzel to Kluckhohn, n.d. [“Friday AM”]). After arriving in Seoul, he told of “one big blow-up” that the two had already had. “Fred is, among other things, a fanatic,” he wrote, “and you know me” (CKP B20 “Pelzel, John”: Pelzel to Kluckhohn, December 17, 1950). “Fanaticism” and “militarism” were characteristics of Williams that could be contrasted with the liberal anti-communism that Riley, Pelzel, and Kluckhohn presumptively all shared.

By December 1950 Seoul had already been occupied and retaken once in the early stages of the war, and was again threatened from the north by the advancing Chinese and North Korean forces. The research team’s presence there was provisional from the first, with an eye ever cast towards the possibility of “any significant deterioration farther north” (JRP: Riley to Riley, December 12, 1950). Pelzel described a city “packed” with refugees, with “nights black with people indoors and noisy with jumpy guards; days with emergence outside, pretending to go about their business, worried but under tight personal discipline.” Though he could not be the “fanatic” Williams was, Pelzel added, “I admit the terror became very clear to me” (CKP B20 “Pelzel, John”: Pelzel to Kluckhohn, December 17, 1950). The team was, however, ensconced in comfort at the Chosun Hotel—Seoul’s best—where senior U.S. officers were also housed. Williams had spent the week prior to the arrival of the other three “lin[ing] up most of the known social scientists in Korea whom we are trying to turn into interviewers,” most of whom spoke English. They also met, interviewed, and were entertained by a series of ministerial and cabinet-level officials of the Syngman Rhee government (JRP: Riley to Riley, December 12, 1950, December 14, 1950).

With refugees also again flowing south, on December 15 the team moved to the outskirts of Pusan. Schramm and Riley, and initially Pelzel, set up their research operation in the squad room of an Air Force base—“the south is so crowded,” Pelzel wrote, “we can’t do any better” (CKP B20 “Pelzel, John”: Pelzel to Kluckhohn, December 17, 1950). They were meanwhile housed in an old church mission building. Riley wrote,

The conditions here are unbelievable. Their stand [*sic*] of living, of course, has been traditionally near the minimum and the cruel trick of fate which has added war to their burdens is hard to accept. I don’t suppose I’ve ever seen real poverty before. The books which describe a poor land with teeming millions simply can’t tell the story. The pitiful attempts at washing in open muddy streams—old men with enormous loads piled

high on a fantastic kind of pack board<sup>7</sup>—small children with only slightly smaller brothers or sisters on their backs—whole families on the move, where only the more fortunate ones have an ox-drawn cart—thousands of native troops, ill-equipped but on the march—all such observations create a continuous picture of stark tragedy. (JRP: Riley to Riley, December 16, 1950)

While noting that their research had already contributed to the understanding of some “tactical problems,” Riley was generally pessimistic about its larger prospects, but concluded it was “better than nothing.” Pelzel, he wrote, “calls it ‘pooping and snooping’ and I can’t think of a better description despite the fact that we have lined up the cream of Korean social scientists to work for us” (JRP: Riley to Riley, December 16, 1950). The social scientists, though, had something else to worry about, insofar as their own families “had been by necessity left in a small town considerably farther north,” nearer to the line of battle, with few possessions or funds (JRP: Riley to Riley, December 25, 1950).<sup>8</sup>

Schramm, Riley, and for a time Pelzel as well spent the research period reviewing transcripts of interviews with refugees and interrogations of North Korean prisoners of war, sometimes making their own trips to refugee camps in order to locate subjects of a specific socioeconomic or other sort of status (JRP: Riley to Riley, January 1, 1951). One refugee survey, for example, asked subjects their primary reason for fleeing and what they would have done had they been unable to do so (JRP: “Codes and Sorting Instructions for Refugee Study,” January 14, 1951). In his December 17 letter to Kluckhohn Pelzel also explained that, in his role as anthropologist of the group, he “want[ed]—if guerillas let us—to spend a week or more in a village outside the old [Pusan] perimeter” (CKP B20 “Pelzel, John”: Pelzel to Kluckhohn, December 17, 1950). It is unclear exactly when he left Pusan, or whether any of the Korean staff accompanied him, but he was joined in his rural research by an Air Force major, and later historian of Korea, Clarence N. Weems. Weems was of missionary background; Cumings (1981:510) describes him as having been associated with the Korean Provisional Government and Kwangbok Army in Chungking as an OSS officer during World War II. The extent of his Korean language ability can be judged by the role he played during the U.S. occupation in 1947—listening in on Korean parliamentary debates and reporting on what was said to the military government (e.g., NA RG554/E1370/B2/F 2/25/47-4/15/47: “KILA daily summary 25 March 1947”). Pelzel, Weems, and whatever Korean members of the team there were spent time in two villages, Kūmnam-myōn and Kach’ang-ni,<sup>9</sup> both northwest of Taejōn and thus uncomfortably close to Seoul—especially

after the new year brought a renewed offensive by Chinese and North Korean forces and a recapture of the southern capital. On the eve of the return of the “rural team” to Pusan on January 8, Riley remarked, “we’ll be glad to have them back because things don’t look any too bright” (JRP: Riley to Riley, January 7, 1951).

The researchers spent their last week in Korea primarily on preliminary data analysis. Notwithstanding the general poverty and desperation of circumstances, they were provided with punch-card fed “IBM machines,” with their own power supplies, for the numerical tabulation that Schramm and Riley focused on—Riley remarked on the irony of lacking running water and light bulbs yet having access to a sociologist’s fantasy of “more . . . equipment than I have ever seen in one place.” Schramm, Riley, Pelzel, and Williams left Korea on January 15, pessimistic that they were abandoning Korean colleagues to their fates. After presenting oral reports to General Stratemeyer in Tokyo and General Kenney at Maxwell, they returned to their respective homes (JRP: Riley to Riley, January 12, 1951, January 14, 1951).

Through correspondence and several meetings at HRRI in Alabama, Schramm, Riley, and Pelzel collaborated to write up the results of the study through the spring and summer of 1951. The main product of their work was an unclassified report submitted to HRRI in May and designated its “Psychological Warfare Research Report No. 1,” entitled “A Preliminary Study of the Impact of Communism Upon Korea” (Schramm, Pelzel, and Riley 1951). Schramm had main responsibility for three of its five chapters, including a general summary, a chapter on the “Sovietization of Seoul” covering its months of occupation, and a chapter on North Korea that drew on refugee interviews. Pelzel’s chapter prepared with the help of Weems, “The Sovietization of Two South Korean Rural Communities,” was the longest, while Riley wrote a final chapter on the “Flight from Sovietization” drawing on survey research with Southern refugees in Pusan. A second report submitted simultaneously, originally classified Secret, extended the main study to provide operational suggestions for psychological warfare targeting the vulnerabilities of ostensibly Sovietized states like North Korea (Human Resources Research Institute 1951, in AFHRA reel 33584 Iris 1028945). Meanwhile, Riley and Schramm also began work on a popular book that mixed findings from the portions of the main report relating to Seoul with first hand accounts of its occupation, which would be released widely in October with an introduction by Williams and the title *The Reds Take a City* (Riley and Schramm 1951). It was dramatic by design, telling in Riley’s words “a general story that the public needs to know”;

as the book was in final preparation Schramm wrote Williams with the news that it was “going to be so anti-Communitistic that Riley, Williams, and Schramm will be elevated to high priority in the Moscow hierarchy of future business” (ICR B5: “Air Force Correspondence 1950–51” Riley to Schramm, April 30, 1951; Schramm to Williams, August 1, 1951).

### **Distribution 1: Japan, PO&SR Anthropology, and the Pragmatics of “Institutional Interventionism”**

Writings on the Korean Sovietization study have tended to emphasize its connections with the postwar “communications paradigm,” and thus its place in the careers of Schramm and (to a much lesser extent) Riley. To locate the anthropological address it also had we must trace Pelzel, Kluckhohn, John Bennett, and others back to the interdisciplinary practice of anthropology in the SCAP occupation of Japan, centering on the Public Opinion and Sociological Research (PO&SR) Division that both Pelzel and Bennett led. In a narrow sense, the network of scholars that fed into the Korean study was significantly formed in Japan. More importantly, however, its assumptions and disciplinary *habitus* were as well. While this brand of anthropology certainly took its place within the metropolitan U.S. field of disciplinary orientations defined by departmental and theoretical tensions—by a Chicago emphasis on social anthropology and Harvard Social Relations versus Columbia’s cultural focus, or by “patternism” and its critics—the specific pragmatic demands of research under occupation conditions were also critical. The result was a theoretical-practical orientation that I am going to call “institutional interventionism.” This was, in effect, an attention to institutional formations, blockages, and pressure points that might accelerate or retard change. The point is not that institutional interventionism was inherently tied either to the reform of Japanese society in the name of democracy or to psychological warfare, but rather that it abetted a mobility between them.

A full history of PO&SR is beyond the scope of this paper; John Bennett, in particular, was dedicated to having its story told from the 1950s right up to his death in 2005 and thus several sources exist.<sup>10</sup> What is relevant, first, is that PO&SR brought together many of the actors who, having dispersed into different organizations, would be central to the Sovietization study in 1950. The research body originated in 1946 as a unit of the Civil Information and Education (CI&E) section of SCAP Japan, headed by Herbert Passin, who together with Bennett had been a graduate student in anthropology at the University of Chicago and, in the early 1940s, had conducted attitude surveys un-

der the Program Surveys Division of the Department of Agriculture and the Office of War Information. Passin's background in anthropology had nearly led to his assignment to the Arts & Monuments Section of CI&E until he discovered its new unit and requested a transfer based upon his expertise (NA RG331/UD1697/B5780/F7: Passin to Stout, March 27, 1946). PO&SR's expansion and elevation to divisional status within CI&E was then advocated by several of the "visiting experts" who circulated back and forth to Japan in the late 1940s. The decisive argument was made by the psychiatrist Florence Powdermaker in 1948, but a prior interdisciplinary team consisting of Kluckhohn, sociologist and later HRRI director Raymond Bowers, and the psychologist Herbert Hyman, had made the case over the winter of 1946–47—with Kluckhohn seeking to work the issue gradually through military channels while Bowers "tried to meet . . . [the] situation by a frontal attack: he demanded interviews with MacArthur, General Muller, etc. and, in [Kluckhohn's] opinion, prematurely" (JBP B1 F15: Kluckhohn to Bennett, April 25, 1957; Bennett 1952:21–22).<sup>11</sup> As a formal element, the "interdisciplinary team" itself thus circulated among invited consultants, the explicit design of PO&SR research, and the eventual HRRI study in Korea—the disciplinary range of Kluckhohn, Bowers, and Hyman was closely reproduced in the trio of Schramm, Riley, and Pelzel. The status elevation of PO&SR was thought to require, in its chief, higher academic standing than Passin could provide, and as a result Pelzel, already working for SCAP on issues of Japanese language reform,<sup>12</sup> was chosen for the position in October 1948. When Pelzel took up an assistant professorship at Harvard less than a year later, John Bennett, whom his old friend Passin had long tried to coax to Japan, assumed the top slot (JBP B23 F211: Passin to Bennett, December 23, 1947, April 5, 1948; Bennett 1952:22). Bennett listed Passin, Robert Redfield, and Kluckhohn—with whom he had corresponded at least since the early 1940s—as references on his Form 57 Application for Federal Employment (JBP B23 F208).

PO&SR largely functioned as an in-house contract research agency for other SCAP units and divisions, employing Japanese social scientists under the supervision of its American staff. As its name suggested, it undertook both public opinion polling and more in-depth sociological and anthropological research. Among examples of the former were surveys, requested by SCAP Legal Affairs and its Economic and Scientific Section, that sought to determine "what percentage of the Japanese people is in favor of an economic system involving freedom of enterprise" and "what percentage of the various groups and classes of the Japanese people is in favor of the program eliminating the Zaibatsu concentrations

of economic power.” Communism was also a concern of public opinion researchers—the Economic and Scientific Section also wanted to know its degree of success among Japanese workers (JBP B1 F4: “Summary of Major Research Problems of the Public Opinion and Sociological Research Division, CIE”:1, 2, 6). Meanwhile, the more anthropological and sociological sort of PO&SR studies took institutional arrangements and templates rather than statistical populations as their object. One major cross-cutting focus was on *oyabun-kobun* (paternalistic or patron-client) relations—it flowed, for example, from SCAP reformers’ desire to know “the relationship of the legal system to gangs and guilds with respect to political corruption and the assumption of pseudo-governmental functions” (JBP B1 F4: “Summary of Major Research Problems of the Public Opinion and Sociological Research Division, CIE”:5).<sup>13</sup> Institutional analysis also grounded a study of “forestry social economy” that “had an explicit theoretical base consisting of concepts adapted from the Weber-Parsons scheme of analysis for institutional economics” (Bennett 1951:3). In any case, in light of its advisory capacity to SCAP policymakers and reformers, PO&SR’s research had a dominant emphasis on understanding and effecting concrete sociopolitical change that mandated a refusal of some larger frames of analysis. Bennett would later write,

It is worthy of note that the research using this latter concept of change avoided two familiar reference points found in other research on Japan. First, the interpretation of certain aspects of social life as traceable to either “feudal” or “modern,” or a fusion of the two; and second, as traceable to either “native Japanese” or “Western.” It is not implied that these familiar formulations are wrong, only that PO&SR researchers felt that an approach which utilized neutral, universal concepts and types might bear more fruit in the long run than pre-commitments to specific historical origins. Although final results are not yet at hand, it is strongly indicated that much of what has been familiarly regarded as “feudal” in origin is better seen as stemming from basic tendencies in Japanese socio-economic structure which can be viewed as adaptations to a large population and limited resources. . . . Similarly, the use of the Japanese-Western contrast often tends to obscure institutional systems and relationships; tendencies toward centralized economy and “capitalist” practices were well under way previous to the opening of Japan in the 19th century. Such developments were carried out in a social framework containing community, familistic, and

traditionalistic emphases which have persisted in large measure. This is not to deny that there are other practices and attitudes definitely traceable to the technologization of Japan following contact with the West.<sup>14</sup>

“Large block” (Law 2002:52) categories of “feudal” and “modern” had many ancestors, and had found their place in various currents of East Asian historiography and social science influenced, often silently, by Marxist evolutionism. The denial of “Japanese” and “Western” as a productive polarity, meanwhile, pointed more directly back towards currents in American thought, including the legacies of Boasian historical particularism.

The pragmatic orientation of PO&SR’s anthropological research towards institutional analysis and reform thus distanced it from other recently ascendant modes of anthropology in the service of government: working within an occupation setting demanded a different scale of attention than interpreting “culture at a distance.” PO&SR anthropology defined itself against wartime and postwar culture and personality research, which in Japan, of course, meant Ruth Benedict’s (1946) *The Chrysanthemum and the Sword*. One of Bennett’s subsequent histories reflected the initial circumspection of its members negotiating their relationship with a work that was already assuming the status of a monument. “The stimulating hypotheses offered by Ruth Benedict . . . were of special interest to Division researchers,” he noted, but then revising himself continued, “at least they formed a significant point of departure” (JBP B1 F12: “Social and Attitudinal Research in Japan: The Work of SCAP’s Public Opinion and Sociological Research Division”:11). By 1950, however, the two-sidedness had vanished.

One day we will have to sit down and rewrite Benedict on the basis of what we’ve accomplished. Most certainly we will be able to provide a very sharp set of comments on her work, and that of the other Japanese experts who wrote so much during the war. (JBP B24 F214: Bennett to Linton, May 10, 1950)

Subsequently, Bennett would sublimate the political optic of interventionist occupation anthropology into a general critique of the culture concept as applied to national wholes. Together with Michio Nagai, he was first instrumental in introducing American anthropology to the Japanese academic reaction to *Chrysanthemum*. Bennett and Nagai (1953:410) summarized the decidedly mixed judgment, but then in conclusion offered the more forceful and thus somewhat disjointed claim that “collectively, these Japanese appraisals of Benedict make up *the most*



*thorough and exhaustive critique of a particular specimen of the whole-culture-patternist approach ever made, or at least printed.*"<sup>15</sup> Next, in his own 1954 *American Anthropologist* article "Interdisciplinary Research and the Concept of Culture," Bennett expanded the point to argue that approaching social groups with an eye towards an integrative approach to problems rather than simply a "federation" of different approaches—in effect, an interdisciplinarity that was more than multidisciplinary—demanded of anthropologists a "*cultivated ignorance of 'culture'*" (Bennett 1954:174).

In the first place, he cannot afford to see all social scientific problems as problems of culture because he discovers that a whole range of problems require finer discriminations. If he studies social relationships in modern society and its institutions, as he is likely to do currently, he soon discovers that he cannot assume that his subjects are simple bearers of culture who are learning and interacting in the face-to-face group atmosphere. (Bennett 1954:173)

The specific issue with "culture," in other words, was that it was too big to be useful. While Bennett oriented the article towards the conditions of possibility for interdisciplinary explanation and set his position in theoretical dialogue with canonical anthropological writings, it was clear also that the consequences for interventionist practice of the "extreme phase" of "the 'national culture' studies of the past war," in which "anthropologists actually attempted to isolate the ethos—the holistic face-to-face tribal culture—of great modern nations" were not far from his mind (Bennett 1954:173). In a long footnote, he claimed,

All of these wartime studies were designed with practical ends: by knowing the enemy's culture one is better able to devise weapons of psychological warfare. Thus Gorer . . . recommended that the Japanese "way of life" and the Emperor not be attacked in propaganda, this recommendation being "based on an understanding of the *whole* Japanese culture." Gorer really means the particular expressive-symbolic aspects of Japanese culture that he studied. If he had literally studied the "whole" culture, he would have discovered the existence of liberal, universalistic, democratic elements of considerable historical depth which were crying for a change in the whole Japanese system. Politically, it might have been more expedient to strengthen the hand of these elements; because by maintaining the policy of "revere the Emperor" and the Japanese "way of life" the Allies



preserved the value orientations and social relationship system which in Japan and elsewhere tends to be defeative of democratic change. (Bennett 1954:178 n.7)<sup>16</sup>

PO&SR, indeed, had been about enabling American occupation authorities aiming to transform Japanese society in making the necessary “finer discriminations.” For Bennett, the practice of occupation revealed the limits of cultural analysis.

Pelzel’s voice in these theoretical debates was not nearly as loud; his own contribution to *American Anthropologist* stemming from his PO&SR experience was simply an overview of Japanese anthropology (Pelzel 1948). But the village studies he presented as part of the HRRRI “Preliminary Study” report on another occupation—the North in southern Korea—undertook the same sort of fine-grained, individuated analysis of local institutional politics that was PO&SR’s stock in trade. Pelzel’s section began, it is true, with a summary of the North Korean (i.e., “Soviet”) “ideological line” as presented to villagers during the occupation. However, beyond these first pages, which functioned to bring the chapter into at least superficial harmony with the emphases of Schramm and Riley on communicative practice in the rest of the report, neither ideological messages nor the propensities engendered by “Korean culture” were major foci. Rather, Pelzel examined how, in one part of Kūmnam-myōn, political and party allegiances before and after the arrival of North Korean forces had been structured by a rivalry between two lineage segments of the dominant resident clan, while in Kach’ang-ni, informal village leadership of the sort that had mediated the exercise of local control by centralized states during the Japanese colonial period and preceding Chosōn dynasty had managed to retain influence over village affairs and blunt North Korean extractive efforts even as the formal leaders of the village were completely replaced. In looking also at the occupiers, he devoted attention to civil government, mobilizing mass organizations, and the police as parallel institutions through which invading forces had sought to procure support and compliance, with mixed results (Schramm, Pelzel, and Riley 1951:103–187).<sup>17</sup>

PO&SR itself was also quite quickly called to respond to the Korean War—by August 1, 1950, Bennett described the Division as “doing nothing but research on the Korean situation” (JBP B24 F214: Bennett to “Perry” [Denune], August 1[, 1950]). The outbreak interrupted work on a Japanese rural village survey but initiated or expanded several opinion polling projects designed to gauge Japanese reactions to international affairs (NA RG331/UD1700/B5870/F55: Weekly Report July 7, 1950). A

survey of Kobe residents already underway on June 30 sought to determine the persuasiveness of opposed accounts of the beginnings of the war: who fired first (NA RG331/UD1700/B5873/F3: Report July 22, 1950). After Bennett had suggested that further work on Japanese attitudes turn to the problem of morale, noting that “the Japanese people are at present actual passive allies and to a degree active allies” of the United States and the United Nations, a much larger poll on “Japanese reactions to the international situation” conducted in concert with the *Asahi* newspaper focused more broadly on feelings about the future and the likelihood of success (NA RG331/UD1700/B5872/F49: Report November 20, 1950; F50: Bennett to Chief CIE, September 2, 1950). Still other projects brought PO&SR close to the eventual focus of the HRRI study. On July 17, 1950, the organization submitted a staff report on a potential “reorientation program for Korean POWs,” which was meant to contribute to the design of a wartime prison camp system. Drawing on their knowledge of the literature and in some cases their own experience, the anthropologists and other social scientists advanced the Japanese-American relocation camps of the last war as an especially relevant touchstone, and argued that camp morale would be critical for political conversion through education is to occur (NA RG331/UD1700/B5872/F23: Memo/Report July 17, 1950).

PO&SR’s expertise also attracted other military bodies with their own wartime research needs. In his weekly report dated October 13, 1950, Bennett recorded the visit of an officer attached to the Tokyo-based Far East Air Force (FEAF), which had become concerned with the broad effects of bombing in Korea. It was none other than Clarence Weems, who would later accompany Pelzel to the villages.

The USAF is currently planning a survey of the physical, social, attitudinal, psychological, and political effects of bombing (and indirectly, of warfare) on the Korean population. FEAF has been assigned major responsibility. Major Weems, of the office of Strategic Intelligence, Directorate of Evaluation, FEAF, informally visited this office twice during the week to ask for advice on the conduct of this survey. Informal and general discussions were held; conference reports are in preparation. (NA RG331/UD1700/B5870/F55: Weekly Report October 13, 1950)

Less than a week later, towards the broader research design of what would become the Sovietization study, Bennett was writing Kluckhohn to suggest that “your people could also be our people.” In several senses, of course, they already were.

## Distribution 2: HRRI, Social Science, and Psychological Warfare

If PO&SR research in the Japanese occupation constituted one articulation of the Sovietization study in Korea, and if “institutional interventionism” was one antecedent of its organization and intellectual practice, the study represented also a crucial moment and move in the development of its domestic military sponsoring organ, the Human Resources Research Institute. As Robin (2001:5) suggests, it was not only the case that academics could engage military reason (and funding) in different ways—accepting or rejecting, acting opportunistically or with more strategic commitment, using and being used. Rather, the military appropriation of social and behavioral science was not itself unitary; its purpose and potential efficacy was debated within military institutions by persons in and out of uniform. The distribution of intellectual product to come out of the 1950–51 Korean study, particularly the conceptual range between a more instrumental analysis of “vulnerabilities” and a broader commitment to understanding “Sovietization” in depth, reflected a similar range of military conceptions of the possible uses of academic social scientific knowledge. Within HRRI, this distribution was manifest in the contrasting career trajectories of Raymond Bowers and Frederick Williams.

The National Security Act of 1947 established the Research and Development Board (RDB) as the national body with overall responsibility for military research; the various armed services were each assigned their own subordinate research missions. HRRI was established at the Air University to take up the Air Force’s responsibility for research in a variety of areas, including morale, management, officer leadership and education, psychological warfare, and intelligence methods and techniques. It was advised by a civilian board that strongly resembled the RDB and other national committees in its makeup: Charles Dollard of the Carnegie Corporation, Pendleton Herring of the SSRC, Associate Dean Philip Hauser of the Division of Social Sciences at the University of Chicago, and Carroll Shartle of Ohio State were among its members. An Air University staff study of 1949 made in preparation for HRRI echoed such advisors in emphasizing the necessity of civilian direction and a scholarly feel:

The organization must be such that it attracts competent scientists and promotes effective scientific research . . . civilian consultants have stressed the importance of scientific direction and atmosphere as a factor in the recruitment of high-level social scientists. . . . The development of an integral staff with a selected

kind of unifying research focus, will make an integrated research program possible. The head of this research organization should be a recognized social scientist in order to attract qualified civilian personnel. This man should have the confidence of civilian scientists and have the backing of Air Force officers responsible for the implementation of the program. (AFHRA microfilm reel 4362, Iris A2573 "HRRI—'Regulations, Staff Study and Basic Letters Establishing the Human Resources Research Institute' 17 Jun 49–10 Aug 51," "Air University Staff Study")

Organizers did not look very far: Raymond Bowers, in mid-1949, was serving as the Executive Director of the RDB's own Committee on Human Resources.

While many who have written on the history of anthropology or other social sciences have regarded their Cold War history of involvement in military programs as inherently an instrumentalization of knowledge, Air Force officials drew their own distinction between the sort of results they hoped from HRRI and research more directly tied to immediate military needs, and it was this distinction that the selection of Bowers materialized. Offering him the position of HRRI director, Air University commander General Kenney cautioned,

As the university of the Air Force we want to stress the substantial long-range advances rather than superficial short range improvements. Hence, we want a well coordinated long range program rather than a series of poorly coordinated crash programs, and we want to establish [a] professional atmosphere . . . and the various types of support necessary to accomplish such a program. (AFHRA microfilm reel 4362, Iris A2573 "Human Resources Research Institute": Kenney to Bowers, June 17, 1949; "HRRI—'Regulations, Staff Study and Basic Letters Establishing the Human Resources Research Institute' 17 Jun 49–10 Aug 51": Bowers to Kenney, July 5, 1949)

Bowers accepted on July 5, with a letter that expressed his hope that HRRI could achieve maximum usefulness for the Air Force within five years but also reinforced the long view Kenney had articulated. "Your emphasis on a well coordinated long-range program is the key to the institute's success in my opinion," Bowers wrote, adding "I hope that we shall be firm in resisting all efforts to postpone or modify this objective" (AFHRA microfilm reel 4362, Iris A2573 "Human Resources Research Institute").

War in Korea came at a key moment in HRRI's development—in July

1950, the organization was just moving into its buildings at Maxwell Air Force Base—and it certainly contributed to the Institute's rapid expansion (cf. O'Connell 1990:349). What was unavoidably a "crash" research program would retrospectively be integrated into a coherent institutional research biography alongside the Harvard Russian Research Center's project on Soviet psychological and sociological vulnerabilities, which was itself sponsored by HRRI and was at a crucial juncture even as the Korean team was being dispatched.<sup>18</sup> In HRRI's 1952 report, within its strategic intelligence program the Soviet project was juxtaposed with a complementary and deepening project on Soviet satellites, which centered on the Korean research (AFHRA microfilm reel 4362, Iris A2573 "Human Resources Research Institute"). Yet war also shifted the balance of power between the "long-range" vision to which both Kenney and Bowers had expressed "firm" commitment and its implied antithesis of a research program more focused on immediately-relevant results—or, which was somewhat the same thing, between the degree of control exercised by civilian social scientists versus Air Force officers. In February 1951, writing Wilbur Schramm after having offered him a permanent position at HRRI, Bowers stated that "as for the military-civilian business I have had no more a problem here than one would have being a newcomer in any kind of a going organization" (ICR, B5, "Air Force Correspondence 1950-51": Bowers to Schramm, February 20, 1951). By May, however, the situation had changed. After a visit to HRRI, Schramm sent separate letters on May 8 to its commanding officer Colonel Croker, to John Riley, and to Bowers. To the first, he noted that he was "disturbed by what I heard and saw of the situation at HRRI" (ICR, B5, "Air Force Correspondence 1950-51": Schramm to Croker, May 8, 1951). To Riley, in a letter marked "confidential," he explained that

Everybody is unhappy. Fred [Williams] is trying to transfer to the Pentagon, hoping to take a number of the personnel with him. There is some expectation that HRRI may be moved from Air University command, and some feeling that the proposed departures may be the wave that washes Ray [Bowers] out of his red chair. Ray wasn't there last week, but nobody had a really good word for him. It was a rather ominous clinical situation. (ICR, B5, "Air Force Correspondence 1950-51": Schramm to Riley, May 8, 1951)

To Bowers, Schramm sent a letter that was cordial but tellingly silent on all but official business (ICR, B5, "Air Force Correspondence 1950-51": Schramm to Bowers May 8, 1951).

The situation came to a head at the end of the year, with Schramm hearing from various sides. Bowers wrote on December 13 to explain that he was on leave and thus could not answer a December 10 letter from Schramm officially. In a postscript handwritten onto the typed letter—likely to escape the typist’s attention—he explained, “The fact is that I am on forced leave. Croker & Williams have been out to get me & apparently have succeeded. Please keep this to yourself as the process is still underway.” Croker’s own letter to Schramm implied that Bowers, at the recommendation of the HRRI Advisory Council, was being forced out for cause, and that should he not resign an inquiry might result and “action could be taken.” “Naturally,” Croker added, “everyone (perhaps not Fred [Williams]) is quite sorry for him—even to the point of losing his objectivity. For a while I felt the same way” (ICR, B5, file “Bi 1950–51”: Bowers to Schramm, December 31, 1951; file “Cr 1951”: Croker to Schramm, December 16, 1951). Abbott L. Ferriss, a researcher employed in another branch of HRRI at the time, while knowing little of Williams, suggested to me that Croker resented Bowers’s civilian control over the organization and maneuvered to have him dismissed and transferred, as he officially was in March 1952 (personal communication, December 10, 2006).<sup>19</sup>

In 1953, Bowers underwent an FBI investigation in connection with a pending appointment to the Voice of America. Though some small amount of hay was made of his affiliation with the American Association of University Professors and other professional bodies less than enthusiastic about the rage for loyalty oaths that had swept the nation, the personal comments on Bowers from former neighbors and associates almost uniformly presented him in a positive light—Bowers had, after all, already been cleared through Top Secret in connection with his position at the RDB and perhaps before. The most significant exceptions came from the FBI’s Mobile, Alabama, field office as a result of its canvassing of HRRI, from informants who were clearly Croker and Williams, and these comments suggest additional dimensions of the conflict within the organization. A “Colonel [BLANK]” considered Bowers’s loyalty unquestionable, but remarked that he had a “marked inability to get along with his associates,” and with two-sided praise noted that “BOWERS is a most capable social scientist and if he could learn to accept instructions and suggestions he would make an excellent employee in this field.” [BLANK] of HRRI’s Psychological Warfare Division meanwhile

advised that he has an extreme dislike personally for Dr. BOWERS and that it is possible he has misinterpreted certain remarks BOWERS has made in his presence as a result of his

prejudiced attitude against BOWERS. He stated, however, that prior to the outbreak of the Korean War, BOWERS left the impression with him that Russia was more anxious for world-wide peace than was the United States and that the United States should go all out to cooperate with and get along with Russia . . . BOWERS had this opinion prior to the outbreak of the Korean War. He stated that, after the Korean War began, Dr. BOWERS was considerably quieter and apparently more favorably impressed with the United States' peace efforts than he had been before and that he indicated more loyalty to the United States subsequent to that time. [BLANK] advised that he did not desire to execute a signed statement. (FBI Mobile, Alabama, field office file MO 123-434, March 27, 1953:2-3, released to me by the Federal Bureau of Investigation December 3, 2005, pursuant to Freedom of Information Act [FOIA] request, with redactions noted)

Qualifications and self-vindicating narrative of Bowers being shut up by world events aside, these were weighty words to be giving an FBI investigation, signed statement or no.<sup>20</sup> At any rate, the picture Bowers's HRRI adversaries provided of an academic unable sufficiently to take direction and a vaguely internationalist liberal mixed up on who to blame suggest that disagreement over HRRI's research direction was redoubled by more visceral feelings about civilian and military authority and the sort of political tension between visions of anti-communist commitment that had had Pelzel, Riley, and Kluckhohn laughing across the Pacific behind Williams's back.

Bowers's ouster provided the opportunity to resolve such issues in favor of more directly actionable rather than "long-range" research, to consolidate military control, and thus to establish retrospectively which goals and aspects of the Korean War Sovietization project might serve as precedents for other work. In the aftermath, a draft memo (unclear in its authorship) entitled "Organization of HRRI" suggested that with the departure "we should make the change from civilian to military head at this time."

There are two principal reasons for this recommended change. Each project has to be carefully weighed to determine whether the results will have a real military value; and the work of each contractor on these projects has to be constantly watched for the same reason. While so far it is not true to an excessive degree, we have gained a definite impression that some of the research

is broadening out into areas which have less and less application to our military needs. A military man can judge the military application of the research better than the civilian scientist. (AFHRA microfilm reel 4362, Iris A2573, "HRRI 1950-51": "Draft, Subject: Organization of HRRI," n.d.)

There would still be a civilian academic to help oversee contractors' work—Carroll Shartle succeeded Bowers in that role—but the position would be reduced to deputy status, although the author of the document also suggested that for the sake of appearances the demotion could be finessed by creating a new title, perhaps "Commandant and Director of Research." Finally, the author stated, it might be best to reintegrate HRRI more closely with the rest of the Air Force's research and development mission by moving it from the Air University to Washington or another R&D command, such as Wright-Patterson Air Force Base in Ohio. It is hard to read this memo, almost certainly by an Air Force officer, as anything but an effort to further reign in HRRI's excessive independence (AFHRA microfilm reel 4362, Iris A2573, "HRRI 1950-51": "Draft, Subject: Organization of HRRI," n.d.).

Yet if HRRI in the Korean War played out a sociological split that correlated with "long-range" versus operational emphases with respect to the most productive contribution of social science research to military ends, the organization and the Sovietization study could also be translated as harbingers of potential instruments of war that might accompany a more fundamental redefinition of military interest itself. Schramm's entrepreneurialism in linking the growth of the field of communications to the new requirements of the nuclearized national security state has been well documented (Simpson 1994)—even in the late 1940s, he was couching project proposals in terms of the need to understand "Communications and Inter-Continental Warfare" and citing the Manhattan Project in calling for a "psychological Oak Ridge" (ICR B3: "Inter-Continental Warfare 1947"; B1: "Be 1948-50" "First Suggestions Concerning Our Psychological Oak Ridge"). Whatever his feelings about the HRRI turmoil or his own personal politics (likely closer to the Bowers end of the spectrum), he kept up correspondence with all of HRRI's principals, and in this writing Frederick Williams in particular emerges as an entrepreneurial alter ego inside the military-security complex. It is hard to more than glimpse at Williams's career,<sup>21</sup> but his personal and intellectual ambitions are evident in a document he wrote and sent to Schramm in the summer of 1951 stemming from his participation in Project Vista.



Project Vista, as described by W. Patrick McCray (2004), was an artifact of an early 1950s fashion for “summer studies” to bring together academics and military personnel for mutual discussions. Convened at Caltech, its participants ranged from J. Robert Oppenheimer to Frank Capra, and perhaps unsurprisingly like other events of its ilk it was not ultimately judged as being very productive by many who attended. Yet as clearly hoped by its organizers Vista did provide a new vision for American war policy, one that advocated the development of tactical nuclear weapons in order to make possible the restriction of a nuclear exchange to the battlefield, and this very suggestion was sufficiently offensive to Strategic Air Command leader General Curtis LeMay and others wedded to a doctrine of massive (counter-)assault upon enemy cities to result in the Vista report being classified out of view for the next thirty years (cf. Elliot 1986). As usual in the era, psychological warfare, so called, was treated as a decidedly second tier concern at the Vista meetings relative to considerations of nuclear weapons technology and operations.<sup>22</sup> Yet as evident, for example, in the rapid application of game theory to its questions, nuclear strategy was also simultaneously foregrounding essentially psychological considerations (however reductively these were then rendered). It was in this context that Williams offered his own integrative move in a short paper written the night before his July session—a response to formal presentations, probably less than thirty minutes in length—called “A Report on the HRRI Psychological Warfare Research Program” (ICR B5 “Air Force Correspondence 1950–51”).<sup>23</sup>

Notwithstanding the anodyne title, Williams presented a quite basic reordering of the intellectual grounding of warfare. His own tactics were those of conceptual re-hierarchization, encompassment, and branding, centering on a neologism that at the Vista proceedings he wrote on a blackboard set off with horizontal curly brackets and that he subsequently told Schramm was the whole point of the paper: “Comops,” a blending of “communication” and “operations.” Williams began by explaining HRRI’s place—and leading role, he claimed—among other military research institutions concerned with psychological warfare, which he insisted must be construed broadly to include “considerations relevant to the fields of psychology, sociology, anthropology, government, political science, economics, military science, etc.” He emphasized the close connection between HRRI as a research unit and Air Force psychological operations. He explained that this connection had led him, in hiring staff and recruiting research contractors, to prefer those with both PhD-level training in a relevant field and psychological warfare experience in World War II. The staffing of the Korean Sovietization study exemplified

this, Williams stated, and revealed “something about my thinking about our work”—he reviewed Schramm’s service with the Office of War Information, Riley’s connection with the psychological warfare division of the SHAEF European command, Pelzel’s work in Marine intelligence, and Weems’s status as both an Air Force officer and old Korea hand. Having thus traced the liaison that Comops might represent through questions of institutional linkage and personnel, Williams then arrived at its conceptual heart. He was concerned to avoid the “splitting off” of psychological warfare as a special weapon or set of techniques. Comops was war itself, if war be defined as “operations involved in affecting target populations” (ICR B5 “Air Force Correspondence 1950–51”: “A Report on the HRRI Psychological Warfare Research Program”).

I submit that here is the core of Comops: controlled operations with built in communication devices. For weapons talk—they scream and whine, they rumble, they stink, they bite. Weapons of all descriptions, in their use, arouse anticipations. Some weapons not only convey a message but their use tells a story, becomes an object-lesson, for unaffected peoples. Yet many weapons tell only part of a story. These weapons demand additional operational effort in order that the use may clothe the use [*sic*] with meaning and explanation and interpretation. That which stirs people may best be used to stir people in those directions, release those potentialities in a target, which are desired by our forces. For it is true that the enemy will always be ready to seize upon our default and clothe our weapons with his meanings. (ICR B5 “Air Force Correspondence 1950–51”: “A Report on the HRRI Psychological Warfare Research Program”)

Williams was hardly alone in this sort of formulation—Schramm and other academic thinkers on psychological warfare at the time were fond of quoting RAND’s social science director, Hans Speier, on creating effects through the manipulation of events. But Comops provided a nice packaging, and a promising *raison d’être* for HRRI as a vanguard of the new order. It was not enough, said Williams, to think of words as weapons—a conventional understanding of propaganda and psychological warfare. Rather, weapons were words, and war a communicative act—even or especially in an age when unprecedented destructive force was available. His was a scientized, neo-Clausewitzian “by other means” for the twentieth century. Comops in the Korean War, according to Williams, was only potential and not actuality, but the future would hold a need for many more equivalents of the Sovietization study

team to articulate its re-integrative aim via their own biographies and perspectives. Williams seems to have participated in the HRRI palace coup against Bowers: certainly, in his vision, social scientific knowledge must indeed be tightly and operationally linked to military purposes. But what, he simultaneously asked, are military purposes, and what might they be?

### **Distribution 3: “Opinion Leaders” and the Ideological Production of the ROK State**

Counterfactuals and comparisons across time and space are always dangerous. But had there been such a thing as social science in the late eighteenth century, had one of its practitioners made his or her way to American shores, and had he or she then employed, let us say, James Madison as a native research assistant, historians today might well be driven to take note. This is fantasy, but with a purpose: something like this seems to have happened in Korea in 1950–51. Understandings of the process and product of the Sovietization research as solely resident within the dynamics of post-1945 American social science are based upon the implicit assumption that neither the team of elite Korean social scientists (itself partially a misnomer) nor other Korean mediations were anything more than instrumental to its conduct. But what if this were not the case? Even as the Sovietization study extended one U.S.-centered project of knowledge and ideology, it also extended another such project driven instead by the ideological self-production of the early, Southern, Republic of Korea state.

The HRRI study was motivated by Air Force concerns and conducted using social scientific techniques of sampling, survey, and participant observation, but the orientation of its lead researchers in Korea was also framed, as noted above, by dialogues with English-speaking educated Koreans and government elites. Between December 12 and 14, while still in Seoul, the Americans had audiences with Korean ministerial officials and the social scientists who would subsequently interview for them. “They all have dramatic stories to tell,” Riley remarked, of their own experience of the first North Korean occupation of Seoul or narrow escape southwards (JRP: Riley to Riley, December 12, 1950). His notes, which partially survive, record conversations with a “Dr. Kim,” “Mrs. Song,” and “Mr. Lee” of the Ministry of Commerce and Industry as well as, from the Ministry of Social Affairs, its liaison officer W. S. Leigh and Vice-Minister Choi [Chang Soon]. They variously informed Riley, for example, that “the Red pattern re business & industry is to try to keep the technical personnel, [and] put in new administrations,” that Seoul saw

“piles of bodies—women & children: the Reds went crazy,” and that there had been (only) a “10% hard communistic core in [the] south” who responded actively and positively to the occupation. Several told Riley, rather incredibly, that Seoul’s citizens were completely supportive of American air strikes against the occupiers, regardless of bombing errors—“even when a school was hit killing many children,” Mrs. Song said, “there was rejoicing” (JRP: December 13 and 14 [1950]).

The “cream of Korean social scientists” recruited to work for the study were “some of the most prominent men in Korea”: “the director of the national museum, two deans and seven professors from the University of Seoul, a leading authority on internal medicine, a prominent publisher, a successful banker, the outstanding authority on Korean agricultural problems, etc.” (JRP: Riley to Riley, December 16, 1950, December 25, 1950). Two of this group would contribute appendices to the unclassified version of the Sovietization study report. Kim Che-wŏn, the museum director—“a man with European education . . . whose main anxiety is for the national art treasures which he has secreted far up in the hills”—wrote comparing North and South Korean organizations for writers and artists (JRP: Riley to Riley, December 20, 1950). The longer and more centrally salient contribution on “Political Re-Orientation Campaigns in North and South Korea” came from Yu Chin-o. A comparative legal scholar, Yu was both a professor at Korea University and, as the main author of the first constitution of the Republic of Korea established with its inauguration in 1948, the James Madison of my analogy.

What this roster is meant to suggest is the degree to which the Sovietization study, far from being simply an interventionist application of paradigmatic American social scientific assumptions and techniques to various populations within wartime Korea, was also filtered through and enmeshed with statist South Korean elites’ own projects of national self-production, with consequences for its conduct, conclusions, and forms. Briefly, as Chong-Myong Im (2004) explains, after 1945 the establishment of a separate ROK state in the south was pushed by a relatively narrow political group against leftists, real and imagined, but also against many who desired to avoid national division at all costs. Right up to the eve of the Korean War, its legitimacy was highly contested and its rule secured only through a not inconsiderable amount of coercion and actual state terror. Simultaneously, supportive intellectuals were faced with several interlocking conceptual double binds, on which hinged the possibility of the ROK state claiming to be the authentic representative of the Korean nation and people and thus the prospect of political hegemony. Ethnicity had been a sufficient basis for grounding

anti-colonial nationalism opposed to Japanese rule, but in the division context ethnic nationalism alone threatened to be too inclusive—it made Koreans of “communists,” “rebels,” “traitors,” and others hostile to the Southern state. Intellectuals thus forwarded an intertwined “nationalism of ideas,” making ideology a second criterion *for the very claim to be Korean*; the need for a “nation-building ideology” (*kŏn’guk inyŏm*), preferably one with roots in the Korean past, was an explicit concern of many texts in this period. Thus South Korean anti-communist state nationalism had a distinctively positive attitude towards the constitutive character of ideology as such, opposing its own to an Othered Marxism, in comparison with a Cold War American anti-communism for which the salient opposition was more usually between a naturalized, supposedly non-ideological “freedom” (under capitalist liberal democracy) and Communism that, as ideologically-driven, was perforce unnatural. Meanwhile, a second consequence of the conceptual crises of the ROK state was an acceptance and theoretical articulation of the role of state elites in promoting national will in a situation in which the people, in their concrete multiplicity, could not be trusted. Yu Chin-o’s 1948 constitution bathed in a rhetoric of democracy, a desideratum internationally hegemonic in the postwar period (North Korea would also call itself a “*Democratic* People’s Republic”), but legalists like Yu and South Korean followers of German “state science” (*Staatswissenschaft*) alike also came to forward corporatist notions of proper state conduct. For Yu and others, a distinctive necessary aspect of the “contemporary state,” relative to the fractious practice of earlier democracies, would be an organic sublation and coordination of separate legislative, judicial, and administrative functions, and the reified national will itself, within a strong executive.

An emphasis on the leading ideological role of elites was thus independently built into the South Korean statecraft of 1950. Ron Robin (2001:83), placing the main Sovietization report within the conceptual development of the field of communications, contends that it rather uncritically expressed and reinforced contemporary “American-derived models of opinion and personal persuasion” that, against earlier theories of the unmediated operation of propaganda upon a mass object, highlighted the intervening role of “opinion leaders” who might then influence others. What the considerable intercourse of the American researchers with Korean state officials and intellectuals instead suggests is that the elitism of the Sovietization study was itself a hybrid construction. Yu Chin-o himself may have done a good deal to educate the American educators about persuasion. His own contribution to the report appen-

dix stated that because of the “cultural juniority of the Korean people,” who had not known democracy under Japanese rule or (a telling admission) Syngman Rhee’s ROK government, mere criticism of communism would not be enough to win them over, that additionally “we must give to them a definite, planned-in-advance, and decided guidance.” With respect to North Korea, he advocated targeting the “intelligentsia” (Yu 1951:314, 316). Shortly after the HRRI team’s return to the United States, in late January or February 1951, Schramm circulated to his colleagues an informal working paper entitled “Thoughts on Psychological Warfare against a Sovietized State like Korea” that contained, in blueprint, many of the eventual conclusions of the Sovietization reports (ICR B5 “Air Force Correspondence 1950–51”: Schramm memo to Pelzel, Riley, and Williams “Subject: Current Business,” and “Thoughts on Psychological Warfare against a Sovietized State like Korea”). He quoted Yu’s “penetrating comments,” newly arrived in the mail, extensively. In approaching the majority of the North Korean populace, for example, he noted that

They don’t have the freedom to debate. They don’t have access to a wealth of information from which to decide. They want, in a sense, to be led. Intelligent leaders like Yu say this themselves: “It is difficult to expect free creative activity from Korean people who have been oppressed under the Communists. Guidance must be given them.” (ICR B5 “Air Force Correspondence 1950–51”: “Thoughts on Psychological Warfare against a Sovietized State like Korea”)

The pinnacle of intertextuality was Riley and Schramm’s eventual popular book on the occupation of Seoul, *The Reds Take a City*. *Reds* interspersed findings from the HRRI survey research with “eyewitness accounts” of the occupation by professional-class Koreans; Robin (2001:88) describes them as “overtly Western Korean informants” and notes that “representatives from the laboring classes and other Koreans of unfamiliar backgrounds [to an educated American readership] were conspicuously absent.” Yet it is inaccurate to state that these were “selected from the original HRRI report”—even in *Reds* itself, it is noted that the accounts were drawn and translated from two Korean-language compendia of occupation narratives, *This is the Way I Survived* (Na nŭn irŏk’e saratta [Ŭryu Munhwasa 1950]) and *Ninety Days of Ordeal* (Konan ŭi 90-il [Yu et al. 1950]). Riley’s notes record the moment of his assisted discovery of the first book. He wrote down its publication date of December 1, listed the professions represented by the twelve first-person accounts within, and underlined that they were “all well known

people” (JRP: “I Escaped the Communists in Seoul,” n.d.). Amidst his conversations with ROK ministerial officials on their own experiences, it must have seemed the mirror of what he was already hearing. *Ninety Days*, meanwhile, carried a brief Korean introduction that set out its purpose in alignment with the South Korean project of state nationalism, with its positive conception of national ideology. It dedicated itself to the “improvement of national culture,” “the political enlightenment of the masses,” and a “‘destroy communism’ holy war.” To the American and international readers of *Reds*, the accounts within might have seemed generic and universal, with class familiarity serving to overcome potential cultural distance, but the editors of *Ninety Days*, towards a Korean audience they themselves conceived as needing to be led, had deliberately chosen “famous people” (Yu et al. 1950:3–4). Perhaps unsurprisingly, an account by Yu Chin-o himself led off both *Ninety Days* and *Reds*.

Sources do not permit any definitive statement as to how South Korean officials and intellectuals saw the Sovietization study or why they cooperated in the undertaking. It might be remarked, however, that later in the course of the war the independent exercise of psychological warfare by the U.S. military was a source of much anxiety for Syngman Rhee and ROK officials. In 1953, for example, angry letters flew between Rhee’s office and a commanding American general over the activities of a U.S. psychological warfare unit operating in the South to train partisans for Northern operations; Rhee objected to the lack of access afforded his own representatives and worried that those being trained might not be loyal to the ROK government following the war’s conclusion (SRP B58). Of course, some of this anxiety must be attributed to the high degree of tension between Rhee and his ostensible U.S. allies going back to the occupation years.<sup>24</sup> But some may have stemmed as well from a broader concern with the more governmental (in Foucault’s [1991] sense) generation of knowledge and application of persuasion, promising direct access to a target population, that both psychological warfare and its underlying social scientific survey and diffusion technologies seemed to represent—practices his own state science was ill prepared to emulate. What is revealing in this light is how others sought to assuage Rhee. In Rhee’s papers there is a modest proposal from an American operative who, sincerely or not, seemed bent on telling him what he wanted to hear. The American began by echoing Rhee’s position that unification of the peninsula under ROK control must be the only goal of the war, and then proposed essentially a large-scale joint propaganda effort to push for that end targeting both North and South Koreans. The author was meticulous, however, in proposing as well that the campaign should be



headed officially by an executive committee of senior Koreans—“persons of public standing, unimpeachable character, unquestioned integrity and patriotism” (SRP B58: Douglass to Rhee, n.d.). Persons, in other words, whom Rhee might trust. If this is any guide, the “council of elites” had another significance beyond its multiply-theorized relationship to the general Korean populace, serving also as an emergent mediation between scientized American practice and a distrustful ROK state.

### Conclusion: On Korean War Anthropology and Korean Anthropology

One goal of this paper, certainly, has been to return anthropology to the discussion of a central social scientific episode of the Korean War. I have also hoped to contribute to a reciprocal movement. World War II anthropology and the question of colonial forms of knowledge have become relatively central foci of the discipline’s self-reflection (Asad 1973; Yans-McLaughlin 1986; Stocking 1991), staples of many graduate core surveys, whereas notwithstanding a salutary growing volume of discussion and interest (e.g., Price 2004; Lewis 2005; Stocking 2006), Cold War anthropology and the specific optic of “decolonial” (cf. Kelly and Kaplan 2004) U.S. occupation government have not. With the metaphor of “location,” I have first sought to turn inward to the realm of political subjectivities, for the sake of more nuanced historical narratives. Pelzel, Riley, Bowers, Bennett, and probably Schramm understood themselves as liberals in relation to Frederick Williams’s more “vicious” anti-communist perspective, with Kluckhohn seemingly in on the joke. Bennett put himself on the line for PO&SR Japanese employees accused of being communists (NA RG331/UD1700/B5870/F1: Bennett to Acting Chief, CIE 12/16/49), Schramm took a visible role in opposition to the imposition of loyalty oaths at the University of California (ICR B2 “C 1947–50”: Tolman and Brodeur to Schramm, May 3, 1950), and Riley wrote home unsparingly about the violence of ROK policemen who were alienating their own populace (JRP: Riley to Rutgers Sociology Department, January 14, 1951). Even before the crisis of the Korean War mobilized them all to action, however, Bennett would write to his old radical friend Herb Passin about the evident “inadequacy of all the historical liberalisms and radicalisms,” given that “the S[oviet] U[nion] is an objective threat to the United States in economic, political, and physical ways.” His alternative position was “a modified, critical American nationalism” (JBP B23 F211: Bennett to Passin, March 23[, 1947?]). That we are less likely now to see the Soviet Union as having been a singular, malevolent actor, in the Korean War or elsewhere,



does not make the extent of this perception or this movement any less historically significant.

In focusing on the locations of anthropology, I have also sought to use the Sovietization study as a foil to shift discussion outwards away from prior assumptions of the primacy or explanatory sufficiency of metropolitan theoretical “paradigms” or political interests. To understand the networks and practical-theoretical orientation behind Pelzel’s participation in the study, we must look beyond American debates to the needs and demands of SCAP administrators in Japan. Military interests were certainly relevant to this episode and Cold War social science more generally, but passing through HRRI shows that they were themselves debated and not necessarily coherent. Guiding assumptions and ideological products such as *The Reds Take a City* were framed not only by American social scientists but also by what Im (2004) calls the “political ethnography” of the incipient ROK state.

In foregrounding as a consequence the metaphor of “distribution,” I have furthermore aimed to suggest ways in which the history of anthropology might approach a historical anthropology of anthropology, in tune not (only) with models of intellectual history but (also) with old disciplinary concerns with the circulation of things as conrescences of value and mediators of agency (e.g., Mauss 1967; Appadurai 1986; Gell 1998). There is a newer touchstone as well: if I began this paper with some historiographic ironies of the perspective of historians of Cold War social science on the Korean War moment, it is ironic also that our history of our own social science has as yet rarely participated in the expansion of conceptual possibilities emergent from the burgeoning anthropology of science and technology, largely through its dialogue with contemporary, post-Kuhnian science studies. Of signal importance to this literature is the tracking of emergent, stabilizing, hybrid objects (Callon 1986; Law 1986, 2004; Latour 1988, 2005). Such forms and entities as the interdisciplinary team, “institutional interventionism,” John Pelzel (*qua* “your people” and “our people” at the very same time), the social scientific face of HRRI, Williams’s Comops, Yu Chin-o variously in the midst of affairs, occupation narratives in intertextual movement, “opinion leaders” and the related “council of elites” were all “boundary objects” in Susan Leigh Star and James Griesemer’s (1989) sense—they were common objects conjoining different communities of practice while retaining different meanings, or uses, across those communities. They connected diverse sites in an overall ecology while preserving distance in the act of connection, and in doing so, could serve as vehicles for translations. For example, PO&SR anthropology, what I have called

“institutional interventionism,” was formed as one aspect of the intellectual praxis of the reformist U.S. occupation of Japan. In the realm of pure theory, it could underwrite a position that was anti-“patternist” and eventually anti-“culturalist”—having first been anti-Benedict after finding *Chrysanthemum* unhelpful on the ground. But it could be moved another way to respond to wartime conditions and military desires. With the shutdown of the PO&SR Division pending in early 1951, John Bennett went looking for an institution to house together at least some of its researchers and files so that its works in progress might be completed. One option was HRRI itself. Trying to sell the idea to Frederick Williams, Bennett began by outlining PO&SR’s uncompleted sociological, anthropological, and survey projects and explaining the loss to social science and “Far Eastern studies” their extinction would entail. But he then tracked closer to Williams’s own interests in an integrated military practice at once all-encompassing and newly finely tuned in its scale of strategic and tactical application: “the data collected in Japan in the past five years,” the fruit of examining institutional nodes and not just culture, Bennett claimed, “bear directly on the general problem of psychological warfare: how to get people to do the things you want them to do” (JBP B30 F253: Bennett to Williams, January 30, 1951).

Let me end, however, by opening another issue of distribution. Why has the American anthropology of Korea not been more like the American anthropology of Japan? The question risks seeming naïve or ill formed—Korea has certainly been a minor area of concern for American anthropologists, a “weak tradition” repeatedly recast through the importation of new outside perspectives, and perhaps there is simply no there there. Nonetheless, comparing lists of major works one is left with the distinct impression that the legacy of culture and personality and national character studies, manifest in a faith in the overarching explanatory power of internal cultural concepts and distinctions, remains strong for the Japanese case in a way it does not for the Korean (Ryang 2004). And one could imagine a different result, given the proximity and historical (notably colonial) connections of the two, the tendency of foreign scholarly and institutional careers to arrive in Korea via Japan, and the existence in Korea, then and now, of discourses on national selfhood that might play a reinforcing role similar to Japanese *Nihonjinron* (the theorization of Japanese uniqueness). The partial answer I have given points to the effect of founding moments. PO&SR anthropology, built for the intellectual craftwork of the SCAP occupation in Japan, came to tilt at Benedict but could not, of course, fully displace the influence of *Chrysanthemum* in Japanese studies. In Korea, however, its propo-

nents found something much closer to uncontested ground—and other foundational ethnographies of Korea, unconnected with PO&SR, would nonetheless also arise from the occupation context (Osgood 1951; Knez 1997; EKP). Inverting the usual formula, we might say that “distance” solidified “culture” for Japan, while in Korean studies, a social anthropology of villages with an institutional bent, dominant from the late 1940s until roughly the 1970s, found its early confirmation in habits of the ruling hand.

## Notes

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In citing works in the text the following abbreviations have been used:

AFHRA	Air Force Historical Research Agency
CKP	Clyde Kluckhohn Papers
EKP	Eugene Knez Papers
ICR	Institute of Communications Research Papers
JBP	John W. Bennett Papers
JRP	John Riley Papers
NA	National Archives and Records Administration
RRCC	Russian Research Center Correspondence
SRP	Unam (Syngman Rhee) Papers

1. I alternate below between the common “North Korea” and the official “Democratic People’s Republic of Korea” (or DPRK), and between “South Korea” and the “Republic of Korea” (or ROK).

2. Bruce Cumings (1990:24–31) reminds us that anti-communist containment, as a consensus or least common denominator U.S. Cold War policy posture from roughly the decline of MacArthur to the rise of Reagan, was a characteristic *liberal* (to moderate) position in the senses accorded those terms within the American political spectrum.

3. For brevity’s sake, I shall henceforth refer to this as “the Sovietization study” or, for reasons made clear below, “the HRRI study.” Scare quotes should be taken as implied.

4. Much recent anthropology can be cited here—for starters, one might see Pigg 2001; Hatfield 2002; and Choy 2005.

5. The report that resulted from this study, entitled “Human Relations Factors Affecting the Air War Effort: A Brief Summary of FEAF Personnel at a Critical Period in the Korean War: December 1950,” designated as Report 1 of the Military Management Research Directorate of HRRI and dated December 1951, is unclassified and available on AFHRA microfilm reel 4362, Iris A2573. With respect to the most incendiary issue, on the effects of being ordered to strafe columns of women and children refugees perceived to be transmitting supplies or harboring enemy forces, the report quoted a range of reactions on the part of air crews, from one individual who reported little hesitation (“I’m not too squeau-

mish about it”) to a “Lt. F.” who after one such attack began drinking heavily and bitterly described his daily missions as “peasant-hunting.” Overall, the authors concluded, around 60 percent of air crews were found to accept such missions as regrettable but necessary, while approximately 10 percent felt such attacks should be absolutely avoided; scenarios in which the refugees were comprised of South rather than North Koreans and previous air experience in World War II both correlated with somewhat higher (but still not high) sentiments for avoidance. In the late 1990s, considerable media attention and an international investigation surrounded an alleged attack upon refugees by U.S. ground and air forces in July 1950 near the village of No Gun Ri (Nogul-li), amidst a chaotic environment in which refugee groups were similarly understood commonly to harbor infiltrators. A historical investigation by the U.S. Army Inspector General’s office concluded that no such deliberate attack had been ordered (United States Army 2001). Whatever interpretation or judgment might be applied to the findings of the HRRI report or its underlying events, the very call for a social scientific survey of morale effects from the highest echelon of Air Force officials minimally indicates that aerial strafing of refugee groups was a known common practice towards the end of 1950, and suggests the misleading specificity and act of forgetting with which the No Gun Ri incident, subject and potentially dismissible according to historico-legal evidentiary standards of proof, has come to be taken to stand for the overall conduct of the war.

6. The prospect of studying in the North itself suffered reversal with the Chinese entry into the war, leading to the second-best option of studying the formerly occupied South.

7. A *chige*, the Korean A-shaped carrying frame.

8. The families were only about 50 miles from Seoul. With the Northern offensive at the beginning of January, they were evacuated south by train to join the men in Pusan. Amidst hundreds of thousands of refugees moving in the same direction, the families spent five or six days in a sealed unheated freight car, arriving on January 6. At least one infant died en route (JRP: Riley to Riley, January 7, 1951).

9. Kūmnam-myōn is properly speaking a larger administrative unit within which Pelzel found one village for his research. Kach’ang-ni is given as Kachiang-ni (a Sinified pronunciation) in the text.

10. See, for example, A History of the Public Opinion and Social Research Division, SCAP, part of a web site completed by Bennett and others in 2003 and maintained at The Ohio State University, along with Bennett 1951 and 1952 and Bennett and Ishino 1963:3–24. Ryang (2004:73–100) offers a different perspective on anthropology in the Japanese occupation. Recent general histories of the SCAP occupation as a whole include Dower 1999 and Takemae 2002.

11. Bowers and Hyman substituted for Harold Lasswell and Robert Redfield, who had originally been scheduled to go (JBP B1 F15: Klous to Kluckhohn, October 24, 1946).

12. See for example NA RG331/UD1673/B5355/F12: “Study of Textbook Vocabulary and Style,” October 16, 1947, in which Pelzel acts as CI&E representative to a highly technical discussion about the commensurability of Japanese and English word counts.

13. The topic remained a major focus of PO&SR anthropologists in later years (see Ishino 1953; Bennett and Ishino 1963; Ryang 2004: 87–91).

14. This passage is taken from an apparent draft version of Bennett’s 1951 article “Community Research in the Japan Occupation” (JBP B1 F13:13). The equivalent passage, modified in only minor ways, can be found in Bennett 1951:4.

15. The perspectives were aired in a 1949 edition of the ethnological journal *Minzokugaku Kenkyū* (cf. Ryang 2002) and a twelve-session 1951 symposium at Tokyo University attended by Passin, Iwao Ishino, and fifteen Japanese scholars.

16. Gorer replied by letter, countering that Bennett's position contained "a number of hidden assumptions about politics and personality, of which the chief is that 'democracy' is a universally valid prescription for all societies, whatever their previous traditions, values or shared characteristics" (JBP B19 F191: Gorer to Bennett, May 3, 1954).

17. Looking especially at Pelzel's section, Robin (2001:78) contends that the real point of the HRR1 report was "to disprove the relevance of economic class and ideology for understanding the internal rifts in Korean society." He suggests that Pelzel generalized from his analysis of the factional split between lineage segments in Kūnnam-myōn to present a broad picture of "Korean peasant society" as politically defined by "primordial traditions." Yet Pelzel clearly presented that one situation as a single boundary case, and in also discussing, for example, prewar South Korean government campaigns to imprison, convert, and sometimes execute alleged "Communists" he also laid ample ground for considering non-"traditional" reasons why some villagers may have responded positively to the North Korean alternative (Schramm, Pelzel, and Riley 1951:129). Still considering the HRR1 project, Robin (2001:79) goes on to quote Bruce Cumings on the American tendency to see the Korean War as an "East-West" (that is, bipolar Cold War) conflict rather than a "North-South" (that is, postcolonial leading to civil) conflict. Of course this is true of the "Sovietization" frame itself—but the Cumings passage Robin cites actually refers most directly to the perceptions of Dean Acheson, and for what it is worth when Cumings elsewhere surveys the results presented by Pelzel in his section of the HRR1 report ("this valuable study") he finds the complex pattern of the multiple structuration of village political responses that Pelzel emphasized to accord with other historical evidence (Cumings 1990:628, 683–685).

18. Bowers wrote Kluckhohn on December 8, 1950, to discuss the possible expansion of the Russian Research Center contract and Kluckhohn's upcoming trip to Germany to check on the progress of project-related interviews with Soviet refugees there. In passing, Bowers noted that early reports from the Far East Research Group were "encouraging" and that the research team was receiving "very good cooperation" (RRCC B10 "Raymond V. Bowers": Bowers to Kluckhohn, December 8, 1950).

19. I am grateful to Dr. Ferriss for his assistance and corrections.

20. David Price (2004) comments on the promise, perils, and partiality of using FOIA-released documents for research on Cold War anthropology.

21. CIA careers are, of course, especially difficult to track. At the National Archives, on declassified documents recording conferences on intelligence and psychological warfare matters from the Korean War, it is common to find names of participants from other military and civilian intelligence-related agencies clearly listed while CIA participants' names remain expurgated.

22. Writers on RAND, for instance, have described its social science division as subordinate in prestige to researchers offering the (apparent) precision of quantifiable results with the tools of mathematics, econometrics, and the hard sciences (see, classically, Kaplan 1983).

23. The Vista report itself can be found in the Caltech archives, Historical Files, Boxes Y1–Y4. I have not seen it myself, but from archivist Kevin C. Knox (personal communications, July 13, 2006, July 31, 2006), I understand that Williams's participation is nowhere recorded. However, I was also informed that the Vista document is confusing in its organization (names are separated from papers, for example), and does not aspire to be a record of events. I believe that Williams's absence in this record can be explained by his status as a respondent and not a primary presenter.

24. Famously, Rhee opposed the U.S. decision to seek an armistice, preferring to fight on for victory. What is less well known is that Rhee in general was right to worry: U.S. officials *had* formulated plans for a coup—bloodless, it was hoped, but one never knew—to remove him from power should he prove too obstinate.

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