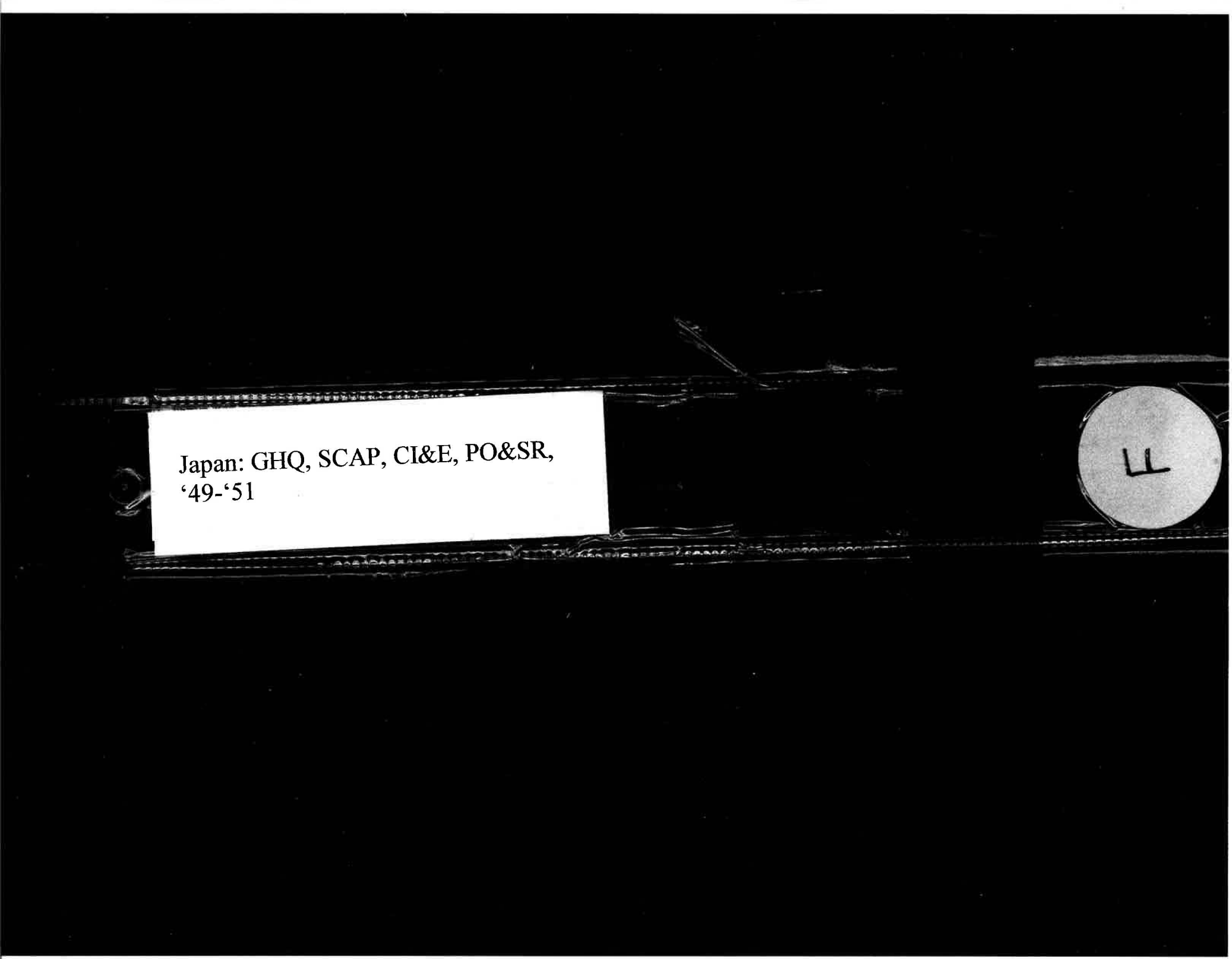
Japan: GHQ, SCAP, CI&E, PO&SR,
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LL

# Life in U.S. Army Military Intelligence Service (1949-1952)

*Following is an excerpt from George Taniguchi's memoirs, "Life in U.S. Army Intelligence Service (1949-1962)." He was stationed in Tokyo, Japan as an Army interrogator and translator after World War II.*

By GEORGE TANIGUCHI

**With the U.S. Army Occupation Force in Japan Enlistment and Basic Training**

On 11 March 1949, I departed Kyoto on the night train to Tokyo. After arriving at Tokyo on the morning of the 12th, I caught an Army shuttle bus to Camp Zama and reported in to the recruiting office at the 4th Replacement Depot where I was sworn in by a major for three years service in the Regular Army. (See Note 1)

On 26 March, I reported in to HQ 1st Cavalry Division, Camp Drake, Asaka, Saitama Prefecture (north of Tokyo). About a week later, I met Joe Okamoto who enlisted after me. .

On 8 April I shipped out to 5th Cavalry Regiment at Camp McGill, Takeyama (near Kamakura) for basic training, and was assigned to Headquarters Company, 5th Cavalry Regiment, and three days later, to a demolition platoon of Hq & Hq Company, 1st Battalion. Eventually, Joe Okamoto plus Daniel Watanabe, Hide Kohara, and Benjamin Wakabayashi who all signed up under the same GHQ directive, joined me in the Platoon.

On 2 September the entire regiment moved to Camp McNair at the foot of Mt. Fuji for maneuvers. Basic training and the exercises at the maneuver grounds were pretty easy for me. I got along fine with the non-commissioned officers (NCO) and everybody else. After about 20 days, I received my reassignment orders to General Headquarters. Far East Command (General MacArthur's Headquarters) with the rest of the first group of Nisei's who were with me at Camp McNair. They were Frank Katayama, Ikkai Nakazawa, Joe Okamoto, Daniel Watanabe, Benjamin Wakabayashi, Hide Kohara, Yoshiki Oshima and Henry Maki.

Courtesy of George Taniguchi

**A portrait of George Taniguchi taken in 1952.**

**Assignment At Allied Translator And Interpreter Service**

On 24 September 1949 we arrived at Tokyo RR Station and all walked with our duffel bags to the Nihon Yusen Kaisha (NYK) building, a short distance where the Allied Translators and Interpreters Service (ATIS) was located. Our quarters were on the fourth floor of this building. The mess hall was on the third floor. We were assigned to Section A, Linguist Training & Control where we went through a 2 to 3-week course on translation and interpreting techniques. For most of us who had a good grasp of the Japanese language, it was like being on vacation. We were free after classes ended for the day in mid-afternoon. Subsequently I was assigned to the ATIS Central Interrogation Center (Captain Fusco, team leader) on the first floor of the NYK Building. Capt. Ben Hazard, 1st

Lt. Reynold Muranaka, and Lt. Kimura were also in the Center at the time.

ATIS supported Major General Charles Willoughby, General MacArthur's intelligence officer (G-2). My guess is that ATIS consisted of possibly over three or four hundred military personnel, with a large number of Japanese American enlisted men and officers, many of who were with the 442nd Regimental Combat Team and the 100th Infantry Battalion that fought in Europe.

After the Japanese surrender in 1945, Stalin decided to use the Japanese POWs to develop Siberia (According to a recent Japanese source, this was in retaliation against President Truman for rejecting a Soviet request to station their troops in Hokkaido). As a result, of the 640,000 POWs taken by the Soviets, 546,000 of them were sent to over 130 Soviet labor camps all over Siberia from Nakhodka (In Maritime Province, a port city facing the Japan Sea), past Lake Baikal and as far west as Tashkent, a city in the Kirgiz SSR and Uzbekstan where 20,000 POWs were sent. Other major cities where labor camps were located were at Komsomolsk, Khabarovsk, Yakutsk, Irkutsk (southwestern side of Lake Baikal), Nobosibirsk, and Alma Atar. The number of POWs who died in labor camps, as estimated in the 1950s by the Government of Japan was 62,000. The Japanese POWs perished in the extreme cold during the harsh winters due to insufficient food, lack of medical care and hard labor. A study done in the 1990s by Victor Karpov, a Russian investigator, in his report "Stalin's Prisoners of Wars — As Revealed From Classified Soviet Documents" claims that 92,000 prisoners had died. A large number of deaths occurred during their first winter where the weak and the sick died off.

Around 1947 the Soviets started to repatriate the POWs back to Japan from Nakhodka to Maizuru Port on the Japan Sea side of the main island of Japan. MacArthur's General Headquarters determined that these repatriated POWs would be an excellent source for Siberian military, industrial, and economic information. It took about four years to repatriate all the POWs back to Japan. Approximately ten to 12,000 POWs were returned to Maizuru by ship per month. ATIS had over one hundred mostly Japanese American GIs at Maizuru who boarded the ships coming into port and conducted shipboard registr

the ship docking. The terrogation documents were forwarded to ATIS from Maizuru and screened for individuals with potentially useful information. After about two or three months those selected for further interrogations were requested to report to ATIS in Tokyo, and were provided transportation, meals and lodging and per diem.

Our daily routine at ATIS started with reveille and roll call in the hallways at 6 in the morning. After breakfast, at 8 a.m. we would report to our office for duty. Our mission in the Central Interrogation Center was to interview the repatriated ex-POWs individually and the interrogators would document all pertinent information. Siberia was divided into six areas, and each interrogator was assigned to an area with which he could maintain his familiarity and talk to the repatriate from that area. On average it took about four to eight hours per individual to complete an interview. Each one of us would be assigned a repatriate who had been through a shipboard interrogation at the port of Maizuru where they arrived by Japanese repatriation ships from Nahodka. Those deemed to have potential for more useful information were earmarked for further interrogation at our Center. We would spend a half to a whole day or more talking to the repatriate. Many of them would tell us about their extreme hardships suffered at the labor camps, with inadequate clothing, food, and hard labor, cutting down timber, working in coal mines, etc. They would describe to us the details of the factories that they were forced to work in, the type of machineries and layout, etc. One of the objectives of the Soviets were to indoctrinate the ex-POWs in the principles of communism, so there were a number of them who were left-leaning, but we were instructed not to discuss politics, but just concentrate on obtaining information. We provided them with packs of cigarettes and hot coffee during the interviews to put them at ease. All of the repatriates that I talked to were basically very cooperative.

After the interrogation, we would take the individuals to the Town Plan Section where there were about 20 or so drafting boards with large sketched maps of towns and cities, drawn from scratch based on observations made by the repatriates. The repatriates provided corrections and additions to the map of the location they were from. After running thousands of repatriates by these maps, we were able to come up with what appeared to be fairly accurate maps. Of course, this is how we had to do business back then, when spy satellites were not available. I knew of one ex-Japanese intelligence officer who spoke fluent Russian and who spent over six months debriefing us. Our reports were reviewed, consolidated and submitted to higher headquarters. So this is how we kept General MacArthur's G-2 apprised of what had been going on in Siberia.

Our daily routine was fairly easy and when we completed an interrogation of an individual in the afternoon, we were free for the rest of the day. Since we carried our own Class A passes, we were free to go out whenever we pleased, as long as we were back by midnight for bed check. We had occasional guard duty assignments and on Saturday mornings, we had close order drills conducted by 2nd Lt. Kitagawa (from Hawaii) at the Palace ground.

On two or three occasions, the Center took the interrogators on one-day field trips which I found very interesting and educational, to factories like the Hitachi Steel Works and Canon Camera in the Tokyo – Yokohama area so that we could learn and familiarize ourselves with similar facilities that the ex-POWs had to work in.

*(Note 1): In late 1948, General Headquarters, Far East Command issued a directive stating that any American citizen residing in Japan would be authorized enlistment in the Regular Army, provided they qualified as Japanese linguists. I immediately submitted my application through Col. Russell T. Boyle (my military boss when I was working for I Corps in Kyoto.) and took my language and IQ tests and physical examination. I was the first to enlist under this authorization according to the recruiting officer at the 4th Replacement Depot at Camp Zama. This was a big day for me. My ambition for the past couple of years was to find a way to join the U.S. Army and get back to the United States and go to college. After being sworn in, I was really happy with the thought that after two unsuccessful tries in the past to enlist, I had finally accomplished the first step toward my goal. Prior to my enlistment, after graduating in March 1946 from a Japanese high school in Wakayama, I worked as an interpreter-translator at the 221st Military Police Company in Kyoto and later on with the Judge Advocate Office of US I Corps, also in Kyoto.*

(To Be Continued....)

September 19, 2002

Subject: List of Japanese scholars who were members of the PO&SR staff.

Bai, Koichi
Go, Minoru
Ikuta, Masaaki
Ishida, Eiichiro
Iwai, Hiroaki
Kitano, Seiichi
Koyama, Takashi
Yu, Mikami
Mizuno, Hiroshi
Sakurada, Katsunori
Seki, Keigo
Shima, Shinichi
Sugi, Masataka
Suzuki, Eitaro
Takeuchi, Toshimi
Yoneyama, Keizo
Odaka, (wife of Kunio Odaka)

Others who were affiliated with PO&SR

Iwai, Hiroaki
Kawashima, Takeyoshi
Koyama, Eizo
Matsushima, Shizuo
Murayama, Tsunneo
Odaka, Kunio
Oka, Masao
Okada, Yuzuru

**To:** ishinoi@msu.edu
**Cc:** Catherine Ishino <cishino@d.umn.edu>
**From:** Catherine Ishino <cishino@d.umn.edu> [Trust] [Block]
**Date:** 10 Aug 2006, 09:06:01 PM
**Subject:** more on po&sr, cishino

HTML content follows

dad, click on line below, to open. xxo, ci

http://www.lib.ohio-state.edu/sites/rarebooks/japan/1b_intro.html

## II. ACKNOWLEDGMENTS

My research colleagues in the Public Opinion and Sociological Research Division (PO&SR), Supreme Commander Allied Powers (SCAP, the official name of the Japan Occupation), made all this possible, especially the following: Masako Inugai, Eiichiro Ishida, Iwao Ishino, Takeyoshi Kawashima, Eizo Koyama, Kazuo Matsumiya, Cynthia Mazo, Hiroshi Mizuno, Kunio Odaka, George Saito, Shinichi Shiina, Keigo Seki, Toshimi Takeuchi, Tamie Tsuchiyama, Keizo Yoneyama, James T. Thayer, Herbert Passin (who was my most important stimulus for coming to Japan), David L. Sills, and John Pelzel *, chief from the very beginning.

The Ohio State University, from 1952 to about 1955, provided the facilities needed for continuing our Japan research and write-ups. This project, entitled "Research in Japan Social Relations," with a staff consisting of Iwao Ishino, Michio Nagai, and myself, produced a number of monographs and at least one book in the course of working up the data acquired during the research of the PO&SR division in the Japan Occuptation. Bibliographic references to some of this material will be found at the end of this book. The Office of Naval Research provided grants for this purpose. All negatives and slides are now the property of the Ohio State University Libraries.

The Washington University Department of Anthropology and the Washington University Libraries provided some facilities for the compilation and work-up of the materials for the manuscript. Victoria Whitte, former director of information services at the Library during the inception and early phases of the project was an indispensable friend and supporter of our work. Glenn Stone, my colleague in the Department of Anthropology, Washington University, encouraged me to collect and compile the photographs, which form the illustrations for this manuscript.

Curators Richard Mitchell, of the University of Missouri at St. Louis, and his wife, Yoshiko Mitchell, read and corrected the introductions of each portfolio of photographs and the explanations accompanying the photographs. The entire manuscript was also read and corrected by Maureen Donovan, Japanese Librarian at The Ohio State University Libraries, and by my son, John M. Bennett, Curator of the Avant Writing Collection at the same library.

## Special acknowledgements.

My wife, Kathryn, carefully preserved the writings and scribblings, which, in the form of letters

home, provided most of the selections called "Journal Extracts" in the manuscript.

Herbert Passin was more responsible for my presence in Japan than anyone else. As the reader will note later, Herb and I jointly participated in a number of research related activities before the episode in Japan. It is Herb who insisted that I accompany him to Japan to participate in Occupation research, which eventually I did. When I arrived he was running the show in PO&SR, and laying the groundwork for the research, of which I then began to take charge. Herb's insights and knowledge were absolutely indispensable in planning and conducting research assigned to the Division. Without him there would have been no PO&SR division nor would it have been possible for me to function as Chief. During the planning and compilation of this book Herb and I had numerous telephone conversations, which provided indispensable information and memories. Herb also provided some notes that are included as footnotes, before his death in early 2003.

*footnote:*

* John Pelzel was the first head of the Public Opinion Unit. Aside from a secretary or two, I don't believe he had a staff. I was called to take over the Unit in about May 1946; John shortly thereafter left to return to Harvard. His return to Japan was about two years later, when he came to carry out a private project--his language study. It was not a CIE project, and he was not a member of our organization. But since he was a friend of Nugent (he had been a Major in the Marine Corps), Nugent arranged space for him and the project within CIE facilities. (By that time, the Public Opinion Unit was no longer a Unit but the Public Opinion and Sociological Research Division, with a sizeable staff.) -- comment by Herbert Passin.



September 19, 2002

Subject: Reconstruction of the PO&SR office.

Lt. Col. Donald R. Nugent, chief of the Civil Informaation and education Section.

John Pelzel, Harvard
Herbert Passin, Columbia

Consultants (informal)

Theodore Cohen, Dudley Davis, Driggs Collette and Ben Mazo of Labor division in the Economic and Scientific Section of the Occupation.

David Sills, now of the Bureau of Applied Social Research at Columbia.

CORPS OF ENGINEERS, U. S. ARMY
OFFICE OF THE DISTRICT ENGINEER
LOS ANGELES DISTRICT
751 SOUTH FIGUEROA STREET
LOS ANGELES 14, CALIFORNIA

ADDRESS REPLY TO:
DISTRICT ENGINEER
LOS ANGELES DISTRICT
CORPS OF ENGINEERS
P. O. BOX 5180 - METRO. STATION
LOS ANGELES 55, CALIFORNIA

REFER TO FILE NO.

21 September 1949

SUBJECT: Travel Order N-Tokyo 774-C

TO: The Commanding General
Military Air Transport Service
Seattle Port of Embarkation
The Chi

1. Under authority CPR T.37 dated 20 May 1948, Mr. Iwao Ishino, Research Analyst, P-5, $6235.20 per annum, is directed to proceed from Cambridge, Massachusetts, to Fairfield-Suisun AFB, Fairfield, California, reporting no later than morning hours 4 October 1949 to the Flight Officer for further movement by military aircraft to Tokyo, Japan, reporting upon arrival to the civilian personnel officer for assignment to permanent duty in accordance with requisition No. 2386. NW-USR-3D-6835-WD-10.

2. Travel by military aircraft, commercial aircraft (AR 55-120 Par. 3), rail and water is directed as necessary in the military service for the accomplishment of an urgent mission and is chargeable to 1-6 P417-02, 03, 2102700 S99-999.

3. In accordance with current Department of the Army instructions, personnel may complete the prescribed immunizations at the port of embarkation, en route to overseas duty station or after arrival overseas.

4. A privately owned automobile may be shipped to the station overseas on a space available basis, at no expense to the Government.

5. a. Baggage to accompany the individual by air will be marked with the owner's full name, will be limited to sixty-five pounds and will accompany the individual to the port of aerial embarkation.

b. Baggage to be shipped by rail and water must not exceed three hundred thirty-five pounds and must be shipped to the port of embarkation marked with the owner's full name and additionally, as follows:

TO: PORT TRANS O (PRIBAG)
SEPoE
SEATTLE, WASH.
FOR: N-Tokyo 774-C

Under no conditions will personal baggage be crated or boxed for shipment to the port. Transportation officers may express at Government

expense the baggage authorized above only when transportation is utilized which does not permit that amount of free checkable baggage and a more economical means will not permit its arrival by date specified in port call.

5. Uniform clothing is not required.

6. Immediately prior to departure for port of embarkation, personnel will
to show name, APO 500, c/o Postmaster, San Francisco, California. Publishers will be requested to withhold mailings of publications until informed of the complete overseas APO address. Immediately upon arrival overseas and determination of the APO address to which they will be assigned for mail service, personnel will inform their correspondents and publishers as to the APO address to which their mail should be sent.

7. In lieu of subsistence, a flat per diem of $6.00 while within and $7.00 while outside the continental limits of the United States is authorized in accordance with existing law and regulations while traveling and absent from permanent station. No per diem is authorized while traveling on board vessels where the cost of passage includes meals.

8. The commanding officer of the port of embarkation will issue a certificate of identification to personnel named in this order with designation cited therein. Upon return to the United States, the certificate of identification will be surrendered to the commanding general of the port of entry.

9. The Chief of Transportation and the Commander, Military Air Transport Service, will each furnish the transportation for which he is responsible and coordinate with all concerned.

10. This person may be contacted through the following: The District Engineer, 751 South Figueroa Street, Los Angeles, California, ATTN: Civilian Personnel Officer.

11. In the event that the person named in this order cannot report as directed, the contact will inform the port of embarkation, via teletype or other expeditious means, of the name of such person, his travel order shipment identifier, statement that such person will not report pursuant to port call or as directed in orders, and when such person will be available.

FOR THE DISTRICT ENGINEER:

M. E. ROVIN
Chief Administrative Assistant

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201 files of theater & CRB (1) ea.
SFPoE (12)

W. D. Form No. 50
(Revised 11-1-44)

# WAR DEPARTMENT
## NOTIFICATION OF PERSONNEL ACTION
(FIELD)

**Corps of Engineers**
**Office of the District Engineer, Los Angeles District**
**751 South Figueroa Street, Los Angeles 14, California**

1. Date: **September 27, 1949**

2. TO: **Iwao** (First Name) **(NMI)** (Middle Initial) **Ishino** (Last Name) 3. S. S. NO. _____

4. THROUGH: **C. G., APO 500, c/o P.M., San Francisco, California**
Office in which Employed or to be Employed

This is to notify you of the following action concerning your employment. This action is subject to the provisions on the reverse hereof:

| 5. NATURE OF ACTION (Use standard terminology) | 6. EFFECTIVE DATE |
|---|---|
| **Excepted Appointment** | **October 3, 1949** |

| | (FROM) | (TO) |
|---|---|---|
| 7. POSITION TITLE | | **Research Analyst** |
| 8. SERVICE GRADE AND SALARY | | **P-5, $6235.20 per annum 40 hour week** |
| 9. FORCE AND SERVICE OR COMMAND | | **FECOM** |
| 10. INSTALLATION & LOCATION | | |
| 11. ORGANIZATION UNIT | | |
| 12. DUTY STATION & LOCATION | | |

13. REMARKS: **"Plus allowances authorized under CPR T6 (or T7, whichever is applicable). The employee has been advised thereof.**

**Requisition No. 2386.**
**Standard Form 61 - APPOINTMENT AFFIDAVITS - administered 27 September 1949.**
**Employment subject to written policies and regulations in effect in the overseas command.**

For the Commanding Officer:

R D Cameron
(Signature)

**R. D. CAMERON**
**Chief, Personnel Branch**
(Rank and/or Title)

EMPLOYEE

INTERLEAVED-PATD.-GILMAN FANFOLD CORP., NIAGARA FALLS, N. Y.

This form notifies you of the action taken with regard to your employment. If you have any questions concerning this action you may take them up with the Civilian Personnel Office.

Appointments to positions are made for such period of time as the work is required and funds are available.

All new appointees are subject to satisfactory character investigation.

If you are entering on duty by one of the following actions, it is subject to satisfactory medical examination.

War Service Indefinite Appointment
War Service Indefinite Reappointment
Reemployment List Reappointment
Reemployment of Annuitant (Special Qualifications)

The first year of a War Service Indefinite Appointment or War Service Indefinite Reappointment constitutes a trial period, satisfactory entrance examination.

This action will neither rescind nor initiate a classified competitive Civil Service status.

STANDARD FORM 50 (4-PART)
UNITED STATES
CIVIL SERVICE COMMISSION
OCTOBER 1946

FORM APPROVED.
BUDGET BUREAU NO. 50-R064.

# ~~WAR DEPARTMENT~~
# NOTIFICATION OF PERSONNEL ACTION

| 1. NAME (MR.—MISS—MRS.—FIRST—MIDDLE INITIAL—LAST) | 2. DATE OF BIRTH | 3. JOURNAL OR ACTION No. | 4. DATE |
|---|---|---|---|
| Mr. Iwao NMI Ishino | 10 Mar 21 | 49-7431 | 10-18-49 |

*This is to notify you of the following action affecting your employment:*

| 5. NATURE OF ACTION (USE STANDARD TERMINOLOGY) | 6. EFFECTIVE DATE | 7. CIVIL SERVICE OR OTHER LEGAL AUTHORITY |
|---|---|---|
| Excepted (Indefinite) Appointment-Corr | 10-3-49 | CSR 6.101 (h) Sch A |

| FROM | | TO |
|---|---|---|
| | 8. POSITION TITLE | Research Analyst (Sociology) |
| | 9. SERVICE, GRADE, SALARY | P-180-5, S-1, $6235.20 p/a<br>40 hour week |
| | 10. ORGANIZATIONAL DESIGNATIONS | Far East Command, GHQ, SCAP<br>Civil Information & Education |
| | 11. HEADQUARTERS | Tokyo, Japan |
| ☐ FIELD ☐ DEPARTMENTAL | 12. FIELD OR DEPT'L | ☐ FIELD ☐ DEPARTMENTAL |

13. REMARKS

Plus allowances authorized under CPR T7. Employee has been advised thereof.

Excepted appointment of indefinite duration. No previous service under same authority during preceding twelve months.

Employee is subject to applicable rules and regulations governing Department of Army civilian personnel in this command. This appointment does not confer a competitive (classified) Civil Service status and is subject to results of security and loyalty investigation. Affidavit striking against the Federal Government secured. Provisions of two year employment agreement become effective upon date of arrival in Japan. Departed POE: 10-7-49 Arrived Japan: 10-13-49

Entrance Efficiency Rating - Good

This corrects items 4, 5, 7, 8, 9, 10, 11, 12, 13, 16, 19 & 23 on WD Form 50 dated 27 September 1949.

FOR THE COMMANDER IN CHIEF:

Issued By:
Civilian Personnel Section, GHQ, FEC
APO 500, San Francisco, Calif.

R. L. MATTESON, CAPT., INF.
ACTG. ASST. ADJ. GEN.

14. SIGNATURE OR OTHER AUTHENTICATION

| 15. VETERAN PREFERENCE | | | | | | | | 16. POSITION CLASSIFICATION ACTION | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| NONE | 5 PT. | 10 POINT | | | WWII | WWI | OTHER | NEW | VICE | I. A. | REAL | P-176<br>DH-1575 |
| | X | Disab. | Wife | Widow | X | | | | | | | |

| 17. SEX | 18. RACE | 19. APPROPRIATION | 20. SUBJECT TO C. S. RETIREMENT ACT (YES—NO) | 21. DATE OF OATH (ACCESSIONS ONLY) | 22. LEGAL RESIDENCE |
|---|---|---|---|---|---|
| M | O | FROM:<br>TO: | yes | 9-27-49 | |

1. EMPLOYEE COPY

★ U. S. GOVERNMENT PRINTING OFFICE: 1946 - 722768

# Reports



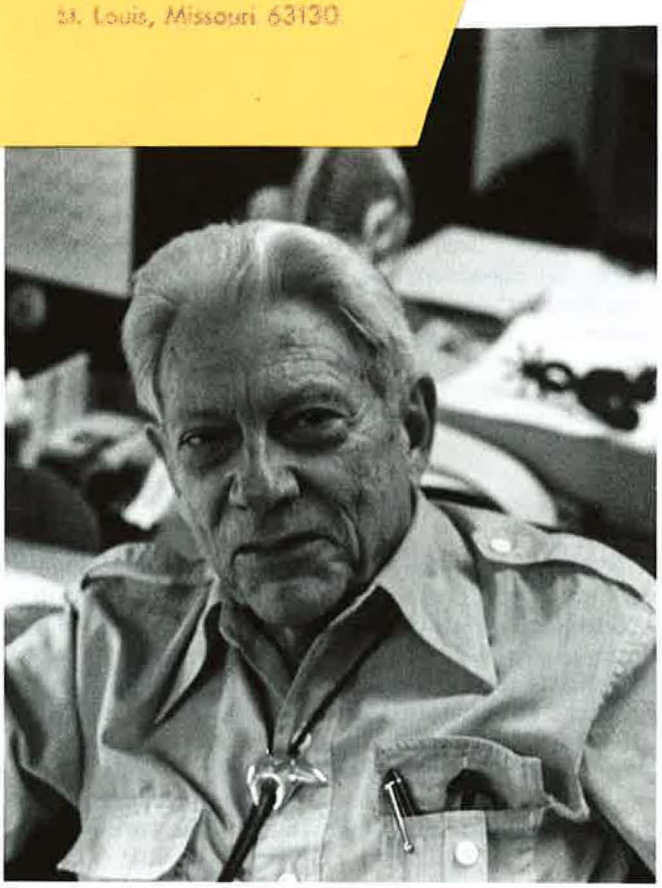
## An Interview with John W. Bennett[1]

LEO A. DESPRES
*St. Louis, Mo., U.S.A.* 22–23 IV 94

LAD: Perhaps we can begin with some biographical details that may have affected your intellectual development. Tell us about your parents, your early years of growing up in Milwaukee, the neighborhood in which you lived, and the character of your primary and secondary education. What, in those early years, did you think you wanted to do with your life?

JWB: At the time of my birth in 1915, my father was the registrar at the University of Wisconsin/Milwaukee Extension Division (later UW-Milwaukee). He took a wartime job in intelligence and never went back to the university. He eventually wound up as an investment banker and took a beating in the 1929 market collapse. My mother was trained as a bookkeeper but left this work after her marriage. The family lived for a time with my maternal grandmother, who had a large old house on the east side of Milwaukee. Eventually, the family moved to a Dutch-colonial-style house in what was then Wauwatosa, a suburb that was subsequently annexed to Milwaukee. I had a conventional middle-class upbringing in a typical middle-class neighborhood which looks much the same today as it did when I lived there.

I attended the Milwaukee public schools, and in the 1920s they were schools in which one could get solid academic training. I owe a lot to the education I received in this system. I went to West Division High, which marks the beginning of my interest in anthropology. There I had an extraordinary science teacher, a man with a high celluloid collar, a luxuriant mustache, and a passion for natural history and archaeology. It was he who steered me toward Beloit. Literary training at West Division was also excellent: we really *studied* the classics. By my third year I was pretty well convinced that I would be some kind of anthropologist, and I was definitely leaning toward the archaeological side of the discipline. Another strong influence came from popular books on exotic locations. I still have the term paper I wrote for the science teacher's class on Robert J. Casey's *Last Home of Mystery*, a book dealing with Angkor Wat as representing the mysterious Orient, vanished civilizations, and so on. I even copied with pen and ink some of the pictures of the ruins.



*John W. Bennett.*

Recently Sol Tax and I were swapping reminiscences of our childhoods in Milwaukee (he was from the east side, I from the west). We discovered a number of mutual friends, one in particular being Frank Zeidler, later a mayor of Milwaukee. Frank was a senior at West Division while I was a bumptious sophomore. We exchanged critical writings in the school magazine. And Frank was Sol's vice presidential candidate when they ran for offices in the Newsboys' Republic—a kind of juvenile labor union movement invented by Sol (who manifested his own bumptious talents for organization early on).

LAD: You mentioned that it was your high school science teacher who stimulated your interest in archaeology and steered you toward Beloit. Why Beloit?

JWB: What attracted me to Beloit was the chance to do actual fieldwork while an undergrad—something Beloit

still offers but rare at the time. Also, Beloit was close to home, and I was pretty much of a home boy. I started college in the fall of 1933. We were still in the Great Depression, and my father was not affluent, but he was able to support my living expenses. I received a full-tuition scholarship my first two years, and that helped. Tuition at Beloit in those days was $350 a semester—sounds ridiculous, but these were uninflated dollars.

I had my first anthropology course in the first semester of my freshman year. It was taught by Paul Nesbitt, the director of the Logan Museum and the only anthropologist on the faculty at that time. The Logan Museum was a relic of a building, built in the 1870s as a Civil War Memorial Hall. The "Hall" part became the display museum; the basement, a sort of cave, became the storage lab area. Three rooms on the second floor provided office and classroom space. As texts, I recall Nesbitt used Kroeber's *Anthropology* (1923) and the Kroeber and Waterman (1924) reader. I must be one of the last anthropologists still functioning professionally who was taught from these books—and others like them, the whole run of classic texts by Kroeber, Lowie, Wissler, and so on.

*As supervisor of the Kincaid Mound Plaza test pit project, 1940.*

LAD: And did you do fieldwork while at Beloit?

JWB: Beloit gave me three summers of archaeology in the Southwest. Two of these involved the excavation of a Mogollon site in southwestern New Mexico under the auspices of the museum. In the third summer I was a member of the Monument Valley–Rainbow Bridge Expedition; my membership was arranged by Madeline Kneberg, an archeologist who was teaching at the Logan Museum for the year while Nesbitt was away. As I recall, the events leading to my participation in this expedition went something like this: In the spring of my senior year at Beloit, as a prospective graduate student, I attended some conferences and visited several seminars at the University of Chicago. One of these seminars was conducted by Radcliffe-Brown during his final semester at the university. I graduated from Beloit in June 1937. That summer, as a guaranteed prospective grad student, I was eligible to join the MVRB Expedition.

This "expedition" was an extraordinary, flamboyant, privately sponsored multidisciplinary project to research the Four Corners region, at that time a wilderness—another "last home of mystery." With considerable fanfare the whole crew on the expedition departed for Arizona from in front of the Social Science Building at the University of Chicago in 15 or 20 woody station wagons provided by the Ford Motor Company. The site where I worked was located in the Navajo Reservation and accessible only by foot or horseback. For six weeks I was stuck up a branch of the great Tsegi Canyon, digging under the direction of Ralph Beals, then a young instructor at UCLA (its first anthropologist). Another member of the crew was Scotty MacNeish, who arrived in Chicago for our departure wearing chaps and other cowboy paraphernalia. The MVRB was a major adventure—sleeping in a bag under a tarp for six weeks near a juniper tree in the red-and-orange sandstone canyon, with a Navaho and his donkey bringing in our food once a week to be cooked by a certified cowboy ranch cook. Talks with Ralph Beals about anthropology and ethnology broadened my perspective within the discipline. This further amplified the influence of the seminars I attended at Chicago. These experiences confirmed my developing interest in sociocultural anthropology. So I enrolled in Chicago as a full-time graduate student in the fall. I started my graduate work knowing pretty well that I was not going to specialize in archaeology. However, at Chicago I had to take courses in archaeology, and I also dug for two summers at Kincaid. I did my M.A. thesis on the artifacts from the Kincaid site.

LAD: Because of Radcliffe-Brown's influence, was Chicago at that time a department in transition? What was the department like, and how did it affect your training as an anthropologist, your proclivity for an interdisciplinary outlook?

JWB: Fay-Cooper Cole ran the place as a traditional four- or five-field anthropology department. I believe you refer to the fact that the department was undergoing expansion: changing from a narrow, archaeology-dominated program to a broader one. Harry Hoijer, a linguist, and Bill Krogman, a physical anthropologist, were both hired about the time I enrolled; after earning his Ph.D., Fred Eggan was put on the staff—not so much "transition" as "buildup." Anyway, in and out of the social sciences, the department and the university were extremely stimulating. If my interdisciplinary outlook came from any one source, it was the general university community, with influence from the Hutchins-Adler tradition, along with Alfred Korzybski and Charles Morris, the latter two being pioneers in semantics and linguistic philoso-

phy (Korzybski was not a faculty member: he held court in an apartment in the neighborhood). The dominant philosophical tone of the university was epistemological, the "unified science" approach. As anthropology grad students we were expected to take courses in philosophy, sociology, and related fields. Thanks to Lloyd Warner in anthropology and Louis Wirth in sociology, this was also a time of the interpenetration of sociology and anthropology and also about the time when "applied anthropology" began to emerge.

In any event, what you call my "interdisciplinary outlook" definitely goes back to Chicago—not to high school or Beloit. At Beloit I was committed to romantic, adventurous, explorational anthropology. (A note to add in this regard is that Beloit was the home town of Roy Chapman Andrews, the adventurer-scientist extraordinary of the 1920s and 1930s. His house was across the street from the Logan Museum.) Another important influence at Chicago was fellow students, particularly Donovan Senter, an offbeat archaeologist-philosopher who more or less vanished from the profession, and Herbert Passin. Passin and I struck up a collaborative relationship designed to feature the anthropological study of contemporary society. There were many others. Chicago in those days had a great deal of fellow-student education; it was somewhat like a medieval university.

LAD: You mentioned that when you went to Chicago you knew that you were not going to specialize in archaeology. Still, during your student years there you were very much involved in archaeological research. You not only dug at Kincaid and wrote your M.A. thesis on materials from this site but also published *Archaeological Explorations in Jo Daviess County, Illinois* and several articles concerning the prehistory of the middle and northern Mississippi Valley [see, e.g., Bennett 1941, 1943*a*, 1944*a*, *b*, 1947]. While being involved in all this, you, Herb Passin, and Mel Tumin carried out a great deal of cultural anthropological research resulting in publications concerning food and social status, dietary changes, agricultural magic, and even personality formation in Southern Illinois [see Bennett 1943*b*, 1943*c*; Bennett, Passin, and Smith 1942; Passin and Bennett 1943; Bennett and Passin 1943]. Could you tell us how your involvement and interest in these very different projects came about and how they relate to your formation as an anthropologist?

JWB: It is not necessary to describe the projects in detail; the publications speak for themselves. The meaning of such diverse activity relates very much to the intellectual climate of the university and the department. The monograph relating to Jo Daviess County resulted from my first assignment as student director of the archaeological lab. The data came from the very first Chicago dig in 1921–22, and Cole wanted me to work them up. As to food habits and rural communities, these interests were part of our "contemporary" orientation but also were influenced by the approaching World War II, and we can discuss them later.

The interdisciplinary orientation of the university and the four-field emphasis of the department impelled me and some other grad students of my "class" or "generation" to bring other fields to bear on anthropological problems. One of my heroes (at long distance) was Clyde Kluckhohn, who had the same perspective: work in all fields and see anthropology as but one of several social sciences. In later years I got called on this perspective by Kroeber. I published a paper [1954] in the *American Anthropologist* on interdisciplinary approaches and the concept of culture. This motivated Kroeber to write a letter in which he stated that he viewed anthropology as a single, independent historical-theoretical discipline and not, as he believed I saw it, as one little endeavor in a larger field.

LAD: Tell us more about your graduate experiences at Chicago. What was your relationship with Redfield? With Eggan? With Tax? Who at Chicago most influenced your thinking?

JWB: As I recall, no one in anthropology tried to constrain or control the instruction of graduate students to a narrow disciplinary course. As to specific individuals, I think Robert Redfield was most influential. As you know, my earliest papers in sociocultural anthropology, those relating to the Southern Illinois food-habits study [1943*b*, *c*], were informed by Redfield's folk society ideas. Redfield's ideas were also larger than anthropology, and they diverged from the main line in that they engaged European social philosophy. Redfield encouraged me to read and do a long essay on Sir Henry Maine and the problem of law and legal institutions generally. This was divergent from the standard classical anthropology of the period. Warner was also important. His famous course "Comparative Institutions" brought us face to face with "complex society" phenomena as differentiated from those of "primitive society." In other words, we came to feel that anthropology had no business generalizing about humanity from tribals alone. This is a lesson still not fully learned by the discipline.

LAD: During the war years, between 1941 and 1945, you were engaged in research activities that were anything but theoretical or academic. For two years you carried out field research in the Bureau of Agricultural Economics of the U.S. Department of Agriculture. Following this, between 1943 and 1945, you served as a field director in the Midwest Domestic Intelligence Branch of the Office of War Information. Can you tell us about your work in these applied arenas and how it might have affected your views concerning anthropology?

JWB: With the war in Europe we all knew that the United States would eventually join the fight, and we enthusiastically began to sign up for research ventures and studies which would serve the coming effort. I became increasingly engaged in "interdisciplinary" or, in Kluckhohn's phrase, "problem-oriented" professional activities. From M. L Wilson of the Department of Agri-

culture to Margaret Mead of the National Research Council to Lloyd Warner and thence to Bennett, Passin, and two or three others, a mixed bag of disciplines came together to focus on problems related to the war effort, and the food-habits research project in Southern Illinois was the prime example.

The food-habits project is an excellent example of the intertwining of academic, professional, and public policy issues characterizing most of the social science of the World War II period. The project really began in an archeological test pit. As graduate students at Chicago we were required to experience a dig at the big Kincaid Middle Mississippi site in the Ohio River Bottoms. In the summer of 1940 we were testing the huge mound plaza by running dozens of test pits across the expanse, each with its own WPA digger. By the middle of the summer the food-habits community-study project was getting under way, and Lloyd Warner, the nominal director, encouraged us to interview the diggers, mostly farm men from the surrounding countryside trying to make a living in the Depression, during our routine visits to their holes, which we did, gathering information on their families and the community. After the Kincaid dig closed, the community research continued into fall and again in the summer of 1941 with a full-dress food-habits study. Throughout the work, each of us pursued a sideline: studies of the culture and social organization of these Ohio River Bottoms communities.

In other words, archeology, community study, food-habits research, attitude research preparing for food rationing in the coming war, Department of Agriculture rural life research, and several other themes all coalesced in this undertaking. Although the United States Department of Agriculture furnished most of the funds, Margaret Mead and her Committee on Food Habits of the National Research Council was the spiritual patron. Since she defined the scope and problems of the project in conferences in Washington, she insisted we work up our data for chapters in her two major report volumes on the work of her committee. Then, and in later years in American Association for the Advancement of Science affairs, I was much impressed by her energy and insistence that anthropology serve the public; I believe this to have been her real career and certainly one deserving of full-length biographical treatment.

Now, as to the wartime "intelligence" work: this really was a kind of offshoot of the food-habits study in the sense that Agriculture had been the source of the funds, which brought us into contact with Rensis Likert's Program Surveys Division in the Bureau of Agricultural Economics. This agency switched from farmer surveys to war-related issues, and Herb Passin and I were hired as interviewers. Later on, the division was switched over to the Office of War Information, and we became a Midwestern office of survey research for the OWI. As the war went on, public opinion studies concerning various war-related efforts—waste collection, propaganda and morale writing, etc.—engaged my attention and employment. Finally, this led to my Japan Occupation assignment: doing research on the social and agrarian reforms.

LAD: In 1946, you joined the faculty at The Ohio State University. What led to this appointment, and how did it come to be interrupted so soon after you arrived there by your appointment as Chief of the Public Opinion and Sociological Research [PO & SR] Division of the Japan Occupation?

JWB: The Ohio State University episode in my career began in a rather complex way. My wife and I and our first child lived in Chicago from the fall of 1939 to 1947. Apart from graduate work, I was engaged in various wartime research and writing a doctoral thesis based on the community-study and food-habits fieldwork. In 1946, on the verge of a doctorate, I was asked to join the OSU faculty. I went there and taught one semester; the family stayed in Chicago. Then they gave me a year to finish the thesis and get the degree, so back to Chicago. After completing the degree I returned to Ohio State, this time with the family. I was at Ohio State for one semester when I went into the Japan Occupation as a DAC [Department of Army Civilian, assimilated rank colonel]. The Japan job was in part a continuation of my association with Herb Passin. We had been associated in the Southern Illinois food-habits community study, then in Chicago as public opinion analysts. Passin got drafted into the Japan language program and was trained and shipped to Japan to start some sort of assessment program there. He had few helpers and immediately made representations to get me involved. I finally got to Japan in late 1947, and by this time the PO & SR office needed a chief. Although I took the job, most of my time was spent doing community and public opinion research in the field.

LAD: Your arrival at Ohio State marked the beginning of a ten-year period during which you, in association with Iwao Ishino, Herb Passin, Robert McKnight, and Michio Nagai, initiated a variety of projects concerning pre- and postwar developments in Japan. Although some anthropologists at that time were beginning to pay increasing attention to complex urban societies and cultures, judging from publications much of your work was quite peripheral to what was appearing in mainline anthropological journals [for example, Bennett 1952, 1956*a*, *b*; Bennett and Passin 1954*a*, *b*; Bennett and McKnight 1956]. Was this work somewhat informed by your view that anthropology should have applied or policy value [Bennett 1956*c*]?

JWB: Following my return to Ohio State in 1951, I was approached by the Office of Naval Research, which was starting an ambitious social science program and wanted me to head up a project to work up our Occupation research data. To help, I brought Iwao Ishino over. He was the last man left in our old Occupation PO & SR Division. Actually, the division was pulled out from under him, and he had to take a temporary job with the army command in Okinawa, where he conducted a political autonomy study for them. In any event, he arrived in late 1952, and we organized the ONR project. Michio

Nagai, who later became a minister of education in Japan, joined the project as our prime research assistant. At the time, Nagai was doing a doctoral dissertation on Herbert Spencer in the field of education, but under the direction of Kurt Wolff, in the sociology department.

Concerning the divergence between my Japan publications and "mainline" anthropology, I think this was another expression of my conviction that anthropology had to get out of its traditional ruts if it wanted theoretical respectability. That is, an anthropologist of this period had to prove that his narrow classical training did not disqualify him from doing applied or socially relevant work. Also, since I had spent the war doing that kind of work, I found it stimulating and interesting, far more so than the specialized tribal and kinship stuff that was in vogue at the time.

LAD: Apart from numerous articles and project reports, you coauthored with Herbert Passin and Robert McKnight a book having to do with the identity problems confronted by Japanese scholars in America and Japan [Bennett, Passin, and McKnight 1958]. And, more interesting from my point of view, you published with Ishino a marvelous study of paternalism in the Japanese economy [Bennett and Ishino 1963; see also Bennett 1958]. Reflecting on this period of work, how would you assess it in the context of your career, and what would you consider its major contribution to anthropology?

JWB: The "paternalism" book was a product of our research program. It consisted of our Occupation research data plus additional materials from research by Japanese scholars on hierarchic relationships in Japanese economic organizations. The Japanese Educated in America Project was commissioned by the Social Science Research Council as one of several overseas education projects which were hot stuff during the early 1950s because of the returning veterans, the increasing scholarship funds that were being made available for the education of foreign students, and the belief that intercultural experiences might pave the road to world peace. We—that is, Ishino, Nagai, and I—represented an ongoing social science Japan research operation, and thus we were chosen to do this project. Passin was tied in as the director of the Japan end of the study. Bob McKnight was added to our staff because we needed an interviewer who could speak some Japanese; he was the son of a missionary and had lived in Japan before the war.

All through this period of research on Japanese Social Relations—as the OSU operation was called—I never really identified myself as a Japan specialist. I didn't know much of the language, and I did not intend to make Japan a lifetime preoccupation. I did the work because I was interested in change and development and because it represented my conviction that anthropologists should work at the "national" level. Aside from professional concerns, the Japan experience was a tremendous one, and for a time I more or less fell in love with the culture.

LAD: Throughout the 13 years that you remained at Ohio State, the university had a combined department of sociology and anthropology. When you moved to Washington University in 1959, you again found yourself in a combined department. How would you assess your association with sociologists such as Kurt Wolff, Gia and Roscoe Hinkle, Alvin Gouldner, Joe Kahl? Did they have much influence on your work? How would you assess the communication between anthropology and sociology today [for an early assessment see Bennett 1948, 1954; Bennett and Wolff 1955; also see Bennett 1976*a*]?

JWB: When I started teaching, there were only a handful of anthropology departments, and they appointed their own graduates. The only jobs available for most anthropologists were in joint departments. So, I started at Ohio State in a typical garden-variety, low-theoretical-level American sociology department. I found teaching anthropology there stimulating for a while, and I enjoyed the friction and controversy. But eventually I felt I had to leave. Alvin Gouldner, in 1959, had taken over a department of sociology at Washington University. Leo, you were there when, following a seminar at Ohio State, Gouldner invited me to become part of his big buildup at Washington University. He promised to rename the department "Sociology and Anthropology," and he did, in a year or so. I found the department interesting and stimulating, full of controversial characters, but eventually the old pull toward one's own department manifested itself. In the late 1960s the sociology side of the department was having serious internal difficulties, and I persuaded the dean to let me pull out the three or four anthropologists and form our own independent department, which I did in 1967. Jules Henry, who had been at Washington prior to my arrival, did not join the new department of anthropology, because he had become habituated to the "combined" framework.

LAD: And your association with sociologists?

JWB: I found, as have many anthropologists through the years, that the precision and directness and historical relevance of some sociological thinking is precisely what anthropologists need in order to come to grips with the contemporary world. Anthropologists don't like to admit this, but it is a plain fact. Economics has played a similar role, and so have other disciplines and subdisciplines. I found that the sociologists at Ohio State, and later at Washington University, were important in educating me in the realities of modern society, from the standpoint of both ideas and methods of study. At Ohio State I received an education in the Parsonian-Weberian tradition from some of the people you mentioned before. My [1992] review and critique of Kurt H. Wolff's recent writings reflects a long association with him. And at Washington University, in the 1960s, I received other kinds of sociological inspiration which helped me retool to work in the fields of communal societies, developing countries, and the like.

LAD: Although you have continued to publish bits and pieces from your Japanese projects, when you arrived at Washington University your research interests and writing shifted markedly away from Japanese society and culture. Since your experiences in Southern Illinois, you seem to have maintained an interest in the organization of agrarian societies. What drew your attention to agrarian life in the Northern Plains and, more specifically, the agricultural economy of Hutterite communities in Saskatchewan?

JWB: First of all, I had some rural background as a child: the only one of my father's brothers that I knew had a farm in Waukesha, Wisconsin. He was a retired railroad engineer, and he raised various livestock more or less as a hobby. We spent a lot of time out there. And then during my summers in New Mexico with Beloit College, I lived on a fabulous old ranch—the real thing, not a showplace—run by an old Texas Ranger. I fell for the *Lonesome Dove* ambiance; no, not *Lonesome Dove,* the real thing. I knew that sooner or later I was going to get back to the Old West.

Well, there were numerous trips, but the professional engagement had to wait until the summer of 1960. This was the summer after my first year at Washington University. I felt free as the air. Our older son, Michael, was also at loose ends. Tom Kehoe, the archaeologist (another Beloit alumnus, but I never knew him there—after my time), wrote me advertising a dig in Saskatchewan for which he had a lot of money from the provincial natural history museum. I wrote back, saying, "If you feed my son and me, we will do what you ask us to do." He was delighted, so off we went. A great summer, although Kehoe and his wife were no administrators and most of the archaeology grad students in the crew resigned after about three weeks of work. I stayed. I wound up conducting a survey in the South Saskatchewan River valley, at that time the only undammed great Western river left on the continent. The valley was full of old frontier ranches. We went from ranch to ranch, swapping stories about pioneer days and locating campsites in advance of the inundation to be caused by the dam that was to be built five or six years later.

That fall, I wrote a proposal to the National Science Foundation for a cultural-ecological study up there and got the money. I have been going up there ever since. Because of overseas researches and travels, I have missed about three summers. Our "informants" became close, personal family friends. So, several themes came together—early admiration for rural things, the Southern Illinois studies, early love of the West, my work on natural resources, especially forestry and farming in Japan—and this all seemed naturally, in 1960, to add up to *ecology,* which was at that time beginning its ride in the discipline.

LAD: Tell us about your research among the Hutterites [see Bennett 1967; also see 1969, 1971, 1975, 1983; Bennett and Barkin 1972]. What were some of the substantive and theoretical issues that originally informed the conceptualization of this project? What particular light did you think populations of this type might shed upon the nature and character of agrarian enterprises?

JWB: I did not start out to study the Hutterites. They were there, and we were obligated by the terms of the grant proposal to study all of the inhabitants of the Maple Creek–Cypress Hills region, where you had more different modes of production than in any other area of comparable size in the region. So, I studied the Hutterites. There was, of course, another motive: I felt I deserved a crack at an intensive field study of an exotic society. I had dabbled in the Navajo, surveyed the Japanese, but never really conducted a more-or-less residential study of a society with beliefs and customs divergent from the norm. The Hutterites offered me the opportunity. And the Hutterites led to a series of thinking and traveling experiences with communal and cooperative groups, including Israeli kibbutzim, many of which at that time were ignored by social science but coming into focus because of the youth communes of the mid-1960s. So I kept at it. (One of my most enjoyable experiences—never written up—was a month with youth communes in the Taos area in 1981.) I had two central interests in communal societies. The first was the need in these societies to forgo a great deal of gratification in order to benefit the group—altruism. It is difficult for humans. How do they do it? The second was the relationship of agriculture to this problem of communal organization. It is easier to maintain communalism if you live in the country and grow your own food.

LAD: I have read a good many of your articles and, I think, all of the books that resulted from the Saskatchewan research. What began as a limited study of one or two field seasons turned out to be a longitudinal research project of 20 or more years [see Bennett and Kohl 1981]. During the course of this work there occurred a substantial shift in your research interests: for example, in the *Hutterian Brethren* [1967] you are mainly concerned with describing the economic and social organization of a communal people; in *Northern Plainsmen* [1969] you give comparative focus to the social organization and economic behavior of Indians, Hutterites, ranchers, and farmers; and in *Of Time and the Enterprise* [1982] your attention is drawn to the managerial styles of agrarian family enterprises in relationship to the sustainable use of resources in a semiarid environment. In the end, you are perhaps more concerned with development strategies and ecological problems than with the social and economic organization of agrarian communities.

JWB: Well, the "shifts" were largely due to the differing foci in the several occupational cultures. Although the Hutterite volume was published first, the Saskatchewan project began with the study of farmers and ranchers, with Seena Kohl as the chief research associate—a role she has played throughout the long period of research and writing. The Hutterites were a case of religious de-

termination of social and economic organization. The ranchers and farmers, while North Americans, were really in a developing economy, and the whole cooperatives bit dovetailed with my growing interest in economic and agrarian development. One thing led to another, and the project expanded. The shift toward more technical and managerial aspects of agriculture and development occurred because of the subject matter. It required knowledge of that kind of thing. I was disgusted with many of the early attempts by anthropologists to study farmers because they missed the whole "instrumental" side of things. It is in this context that I should acknowledge my debt to the Parsonian tradition (which came to me in the 1950s at Ohio State). The one thing that really stuck was the distinction between expressive and instrumental behavior. Anthropologists specialized, at least in the classic era of anthropology (1915–1950?), in the study of expressive phenomena and badly neglected the instrumental. In any event, I realized that you couldn't study modern agrarians without knowing something about the way the instrumental side of their enterprises was constrained by environmental considerations and shaped by forces external to the community, for example, how markets affected their behavior and engaged their cultural institutions.

In 1977 I spent a year at the Land Tenure Center of the University of Wisconsin, mainly to get tooled up for international development research. At that time, I saw development as a logical extension of all my work on Saskatchewan agriculture, ecology, communes, co-ops, and the like. Washington University had nothing on development to speak of. I had been asked to participate in several LTC conferences, especially as a result of my commune work, and this led to an invitation to spend an academic year with them. I did two round-the-world trips, partly in association with the LTC, the Agricultural Development Council, and the American Association for the Advancement of Science, studying co-ops, communes, and adaptations to arid lands. I was a pioneer in the "communal property" movement but was more or less out of it when it really got going in the late 1980s. (You know, Leo, I guess that in part is the story of my career—always in the avant garde, but always out of it by the time it becomes the main run.)

LAD: Was it your work in Saskatchewan that resulted in your association with the Office of Arid Land Studies at the University of Arizona?

JWB: The period spent in Arizona with the Office of Arid Land Studies had no direct connection with the LTC period; it was simply part of the same stream of activities in development and agrarian work. The arid-lands theme stemmed from my writings on semiarid and grassland agriculture, pastoralism, and development. The Office of Arid Land Studies at that time was run by engineers, agronomists, and geographers, who were delighted to find an anthropologist who could speak their lingo. I spent some time in Canada during the study up there working with and learning from people in the Agricultural School at the University of Saskatchewan and was really up on the technicalities.

LAD: Virtually all of your fieldwork has drawn rather significant attention to theoretical discussions concerning customs, institutions, and human behavior or discussions relating to the substantive foci and epistemological character of anthropology in relationship to the study of contemporary problems and issues. May we consider one or two of these discussions? First, the culture concept: as early as 1944 you suspected that the culture concept was something of an impediment to anthropological research, that the concept simply could not carry the theoretical weight that anthropologists had heaped upon it [see Bennett 1944*c*, 1946, 1954, 1976*b*]. What is your current thinking in this regard?

JWB: Regarding the culture concept, my most mature statement is the 1976 "Anticipation" paper in *Science*, reprinted in a recent book of essays [see Bennett 1976*b*, 1993]. Culture is simply no good as an explanatory concept, but it is useful as a descriptive concept. Anthropologists are still confused on this issue. There is a lot more to say, but it really boils down to this: In order to explain—find causes—you have to do more than describe culture. This was a secondary movement in anthropology by the 1960s—I was a forerunner and also a participant. It led to the "hyphenated anthropologies": economic, political, medical, etc.

By the way, with respect to "culture" as a way of describing a human reality of some kind, no ethnologist or "new cultural anthropologist" can possibly equal the very best novelist, especially one like V. S. Naipaul, whose *House for Mr. Biswas* manages to portray the marvelous syncretic Hindu–Muslim–West Indies–British–American mishmash that represented the behavior of the wild and sad members of his Trinidadian family. Not a word about "culture," but the fusion and the not-so-fused elements of these traditions come crystal clear, since they are presented as the living behavior of real people. And, for that matter, no development anthropologist has come close to the portrait of the despair, corruption, and disorder associated with the early stages of economic development in equatorial Africa that Naipaul presents in his *Bend in the River.*

LAD: In the analytic discussions attending your Saskatchewan research and in your writings concerning ecological and economic anthropology, you assiduously avoid subscribing to one or another deterministic theory of human behavior. Adaptive behavior becomes the focal point of your analysis, and you accentuate the significance of the concept "adaptation" for contemporary sociocultural anthropology [Bennett 1968, 1969, 1976*a*, 1976*c*, 1982]. What is the philosophical basis for this approach?

JWB: I am not sure I follow you exactly, but certainly a mind like mine, with its pragmatic, purposive orientation and with a firm conviction that human behavior is

"multidimensional to a fault," is not going to subscribe to any academic "deterministic" theory of human behavior. *Homo sapiens* is capable of doing anything he wants, and it is situation-mediated or influenced by past precedents ("precedents" is my favorite concept of culture). But along with [Alfred North] Whitehead (who influences me at repeated intervals when I get around to reading him) I allow for a huge area of indeterminacy due to the spontaneity and multidimensional capacities of humans.

LAD: I once had occasion to do a graduate paper in which I attempted to assess the impact of Parsons's voluntaristic theory of action on a selected number of introductory texts in sociology. My survey included your 1949 textbook with Mel Tumin, *Social Life: Structure and Function* [Bennett and Tumin 1949]. I vaguely recall concluding that the conceptualization of that particular text was very much in conformity with the voluntaristic theory of action as developed by Weber and others and synthesized by Parsons [1937]. Given the centrality of adaptive behavior in your research and analytic discussions, and given your reservations concerning the culture concept and your dissatisfaction with deterministic theories, either economic or techno-environmental, am I correct in assuming that you essentially subscribe to a voluntaristic theory of human behavior?

JWB: Well, I suppose that "voluntarism" comes as close to it as anything, if by the word you refer to the spontaneity and changeable character of *Homo sapiens*. But basically, I have always shied away from "subscribing" to any theory. I pick them up and use them if they work. I was influenced early on by Merton's [1957:85–120] idea of intermediate and empirical problem analysis, where you solve particular real-world problems with whatever philosophical and technical tools you have handy. "Empirical generalizations" become the product, and these are flexible and changeable. So far as Parsons is concerned, as I previously noted, his discussion of the "pattern variables" influenced me and still does. They are purely descriptive but damn useful organizing concepts.

LAD: Your interest in what we might generally consider ecological anthropology can be dated to articles you published in 1944[*d*] and 1946. More recently you have suggested that "socionatural systems" and the anthropology of resource use and abuse be given more serious anthropological attention [Bennett, Montgomery, and Scudder 1973; Bennett 1974, 1976*c*, 1986, 1988, 1990, 1993]. For those who may not have read your most recent book [1993], how do you conceptualize socionatural systems, and why is the analysis of such systems particularly appropriate as an object of study in anthropology?

JWB: I wish I had the time to really go for "socionatural systems." The idea came to me late in the game, toward the end of the writing of the "Transition" book, when I realized that the microscale cultural ecological studies made by anthropologists weren't contributing anything of real importance, since so much of the human use of resources was determined by outside forces such as multinationals, markets, and imperialism. And these forces were creating enormously complex and intertwined relationships between physical phenomena and human institutions. In addition, my brief work on African pastoralism with the Land Tenure Center brought home the reality of these complex man-nature resource systems.

LAD: It has been three years since you organized a symposium at Washington University on human ecology and the environment. I don't think at those sessions we ever had a clear view of what kind of light anthropology might shed on what kinds of environmental problems and how it might best contribute to the solution of those problems. In *Human Ecology as Human Behavior* you discuss a normative anthropology of resource use and abuse [Bennett 1993:45–76]. Could you briefly summarize your views in this regard?

JWB: Well, if you take the view that *Homo sapiens* is a multidimensional creature, then "ecology" is really a matter of how humans deal with resources *based on the way they deal with each other*. This, it seems to me, is what human ecology is really all about. Humans use the earth as an extension of human society. If you take an environmentalist view—as I do on the whole—then you have to cope with the innate destructiveness of humans, that is, the fact that they can do anything they want to or are at least equipped mentally to do so. So we come face to face with the need for coercion, restraining institutions, the use of force, and so on—all the sticky issues that Garret Hardin has never been reluctant to deal with, in spite of the fact that the humanists have criticized him for doing so. In the piece you refer to I simply assumed that the basic disposition of *Homo sapiens* is such that it is much harder to renounce gratification than it is to accept it; hence the constant intrusions by humans into the natural world, to create "resources." "Resources" is really a concept of human arrogance.

All this is prologue to what I feel about much of the ecological research in anthropology: it rarely deals with the behavioral, moral, valuational, political, or what have you, aspect of the human use of resources. Is a particular agrarian regime good or bad for its environment? Anthropologists, like other professionals, grind small axes: if a peasant society is successfully farming for a new international market, that is good because it gives them a new lease on life, etc. Dozens of studies like this could be cited. But if such production by peasants is at the expense of their own soil, then what? Should the anthropologist criticize them? Report the "negative" information? What are the costs of the perceived benefit? What balance can be struck, if any? You cannot avoid a normative view of any resources-use situation, but you also have to confess to your readers what norm you are accepting—above all, make benefit-cost inquiries.

LAD: Also in your recent book, you have an epilogue in which you detail the rise of ecophilosophy and conclude that such a philosophy will need to transcend environmentalism and conservationism in order to deal with the larger questions of the future of mankind, the appropriate forms of behavior for the coming culture of constraint [see Bennett 1993:323–48]. How might anthropology contribute to that philosophy?

JWB: It is self-evident that we need a new way of looking at the earth and our own role. Everyone says so, but how can we when we are also driven by our own technology? How can we constructively use religion when we are driven by secular concerns? When we are driven by population increases? Can anthropology contribute? I hope so. Work with communal sects that practice consumption constraint should help. Ethnological research on renunciation, on coping with limited resources, etc., might throw some light on the "constraint" problem. But basically, anthropology, like other fields of secular knowledge, is really not equipped to challenge the whole wild array of purposes that constitute the current culturally defined "nature of man."

LAD: Your response to these questions underscores the fact that while your work usually engages issues of theoretical significance, it also deals with human problems that demand solution or problems that confront public policy. This calls to mind the fact that you have been an officer of the Society for Applied Anthropology several times. How do you see your work in relation to that of applied anthropologists?

JWB: Even though it has a strong general or theoretical undercurrent, most of my work would generally be classified as "applied." After early (1950s–1960s) participation in the SAA, I began to experience disillusion. This was based on the hope, so often voiced by people like Laura Thompson, that applied anthropology could develop its own theory. Not only was that impossible but, in addition, whatever theory applied anthropologists really needed or could use did not come from anthropology; rather, it came from fields like economics and sociology. This is the real dividing line, it seems to me, between applied anthropology and mainline cultural anthropology. Aside from this, most applied anthropologists have suffered from their preoccupation with trivia. Many problems assigned to them by people outside the field were not worthy of serious scholarship. Thus, it was inevitable that applied anthropology would gradually become mainly an employment possibility and that serious study of real-world issues by anthropologists would gravitate into the "hyphenated anthropologies"—ecological, economic, political, medical, etc.

LAD: Since your graduate days at Chicago, anthropology as a profession and a field of study has undergone a great deal of change. Currently departments are dividing and the subfields of the discipline appear to be going their separate ways. Even cultural or sociocultural anthropology has been fragmented almost beyond recognition as a consequence of the organization of special societies corresponding to the interests of specialized groups of scholars—for example, economic, political, urban, the anthropology of play, of education, feminist anthropology, etc. What do you make of these changes? What, in your view, does the future hold for anthropology?

JWB: You have answered your own question, it seems to me: more of the same. The fission will continue, since it is inevitable and always has been in a field of knowledge without a clear central theme or "paradigm." "Culture" was such during the classic era, but it simply didn't work once anthropologists began asking why questions. Besides, it wasn't really human culture but the lifeways of tribal societies. So far as some sort of organizational unity is concerned, that will persist so long as there is a need (mainly jobs) for an annual meeting of the American Anthropological Association and so long as departments of anthropology exist in universities. Probably the thing to watch is the extent to which universities tolerate anthropology as a single field. But even if you have fission into separate departments of archeology and this and that, there could persist an organizational rally at national levels. Actually, contemporary anthropology is really a federation of subject-matter undertakings. This can last so long as universities are willing to support it.

As I see it, the chief problem for anthropology and its attempts to do something with contemporary culture is simply that things are so bad (in modern culture, that is) that attempts to "study" it inevitably become critiques, and critiques are much better handled by cultural pundits. Anthropologists tend to be hamstrung by their own "scientific" or scholarly tradition: anything "human" is valid, so to speak; this goes back to the old relativist dilemma which anthropology has yet to surmount.

Let us consider the problem of relativism, culture, and the coexistence of evil and good in human nature. I have, on the wall over my desk, an engraving from a 16th-century edition of Paracelsus of a human being with two heads, one evil and dark, the other good and blonde. It seems to me anthropology has never really coped with this dual nature of man and never will so long as most anthropologists teach and work in a liberal, humanistic setting. Scheper-Hughes [1992] recently did a book on violence and other assorted evil behaviors in Brazil, and she at least shows us that humans can be damn nasty. A few other works of this type exist in the literature, but I recall that Jules Henry's study of the Pilaga was disbelieved by the reviewers: people simply couldn't be that nasty to one another [Henry 1940]! All this comes from many sources: the rejection by secular rationalists of 19th-century moralizing, the Protestant rejection of Catholic doctrines about human evil, the attempts of 19th- and early 20th-century social scientists to excise moral judgments and promote the idea of cultural relativism. All these tendencies are now coming home to roost, now that the world is dissolving into mutually

antagonistic "cultures." What are anthropologists going to say about it? Are they going to admit that their invention of "culture" was in reality a discovery of the basic divisiveness in human nature?

And then there is the question of cultural homogenization and the externalization and rationalization of culture by the media. Culture was once an exotic, semiconcealed phenomenon which anthropologists "discovered." But now it is all out in the open, so to speak. Anyone can find it, and anyone does. The best commentaries and analyses of contemporary culture, as I previously noted, are produced not by anthropologists but by pundits, trend-spotters, political critics, aesthetic and psychological commentators, journalists.

LAD: What have you liked least about the discipline and the profession?

JWB: What I have liked least about the discipline and the profession is its pretension to be in the business of doing research on humanity in general, "man," while in reality it mostly specialized in a small and archaic segment. This habit (I am exaggerating, I admit) violates the most basic aspect of human behavior: its diversity, its varied response to situation, and its multipotentiality. Anthropology got off on the "culture" thing and thereby avoided an inquiry into "behavior" by conceptually merging all "cultures" into one entity, *culture.* Hence, anthropology avoided the necessity for detailed, exhausting comparative analysis. The thing I always appreciated about Malinowski's work on the Trobrianders is that alone among the ethnologists of his era he portrayed them as ordinary, confused human beings—confused, that is, over the fact that their institutions were often at war with their desires and values, as in the case of the "father in primitive psychology," the clash between the lines of descent and the affective responses, etc. He was trying to suggest that tribals were humans like us, products of the same mental and behavioral world, only with very different cultural content.

LAD: What about contemporary cultural anthropology?

JWB: "Contemporary cultural anthropology" is a kind of mishmash: anything goes. Especially hard to take for a praxis type like myself is "literary anthropology" and its attempts to duplicate deconstructivist and other arcane approaches in literary analysis. I am afraid that the attempts I have seen strike me as jejune, amateurish, and pontifical. Anthropologists once again are trying to do something that other people do better. Insofar as the discipline keeps trying to find new ways to interpret behavioral and mental phenomena, this may be good for anthropology. But the trouble is that there is no real intellectual or training base in the departments for this type of stuff. And so it becomes faddish and probably will expire after a few years, with some other borrowed fad to take its place.

LAD: In view of all this, what, then, is anthropology?

JWB: Who knows? It is futile to attempt a definition. Anthropology is what anthropologists do, period. There may be a few central themes and methods, but not really enough to create hard paradigms. In ethnology the crowning achievement is the resurrection and description of tribal culture. That is, tribal ethnology may be the one real accomplishment of the cultural side of the discipline. Beyond that, anthropology is a smorgasbord, and the real merit of the discipline is that it gives people like me a chance to do whatever we damn please and still claim a respectable academic home. I am excepting archeology from all these remarks, of course, and also the growing fusion of the study of early hominids with behavioral and cultural research, since these fields have clear paradigms and missions.

The hyphenated anthropologies, as previously noted, are generally the future of the cultural side of the discipline. The question is: What contributions do they make to the general knowledge of human society and behavior? Political anthropology establishes that ritual is an inescapable part of politics everywhere. What have we learned? Little about *why* people resort to ritual in power contests, that is, why human behavior has this tendency. And is the fact that ritual is ubiquitous really new information? Every politician knows it, though politicians don't necessarily use the term. Does the public know it? Does it make any difference in political participation if they do know it? These are all "stupid" questions, but it seems to me these are the kinds of questions anthropologists really need to ask themselves. I don't know the answers to them either.

LAD: A final question, perhaps not a fair one: How would you assess your own work?

JWB: My experience tells me that most reasonably productive people often feel that their best work is ignored in favor of a particular item which gets into the literature and is cited over and over because it made a flashy point or because authors lack the energy to dig further. In my case, the latter item is *Northern Plainsmen,* which, though a kind of popular or descriptive tract, did something a little different: it used a region rather than a "culture" or a "community" as the unit of analysis, and it featured adaptation as a socioeconomic process of survival in the modern world. However, in my personal and biased opinion, the really fine product of the 30-odd years of research in the Northern Plains of North America was the 1982 book *Of Time and the Enterprise.* This showed, in great detail, and with temporal depth, how the generational rhythms of the "farm family" interacted with external socioeconomic institutions to produce a distinctive socionatural system. The book was well reviewed, usually by nonanthropologists. Anthropologists have sometimes appreciated it—one recently called it a "classic study"—but fail to understand it. I think that the problem is that it deals with the familiar, not the exotic, and, classically, anthropologists cannot really believe that the familiar can produce valuable data.

My own personal assessment of my work takes off from the language used on the certificate of the award for Distinguished Service given me by the American Anthropological Association in 1989: for "unique achievements in integrating theory with praxis." I really had never been fully aware of this until I read it on the certificate. I was simply doing what I thought was appropriate: find "theory"—that is, something valuable in the data—in any and all pieces of research, regardless of the locus of the study or the time period represented. In other words, "theory" emerges out of attempts to understand how and why people behave the way they do in any and all situations. This is another reason I was never very enthusiastic about "applied anthropology"—that is, the institutional field as defined in the journal *Human Organization* or in the protocols of the Society for Applied Anthropology. They wanted to make applied anthropology into some special, distinctive field of its own. Of course, the real issue was the one of causation again: to understand a situation involving social power, you need insights from several disciplines, not just anthropology.

You know, the thing I am most proud of as an anthropologist is the gift of my field notes and all other papers and protocols to the communities where they were collected. The data from the work in Canada and the U.S. Northern Plains went to the Medicine Hat (Alberta) Historical Society. All the data on the Hutterites went back to the particular colony where I learned the most about the Brethren. And what was left in my files of the research protocols and data from the Public Opinion and Sociological Research Division of the Japan Occupation—about five file boxes—went back to Japan, to the Japan Public Opinion Research Association. Amen.

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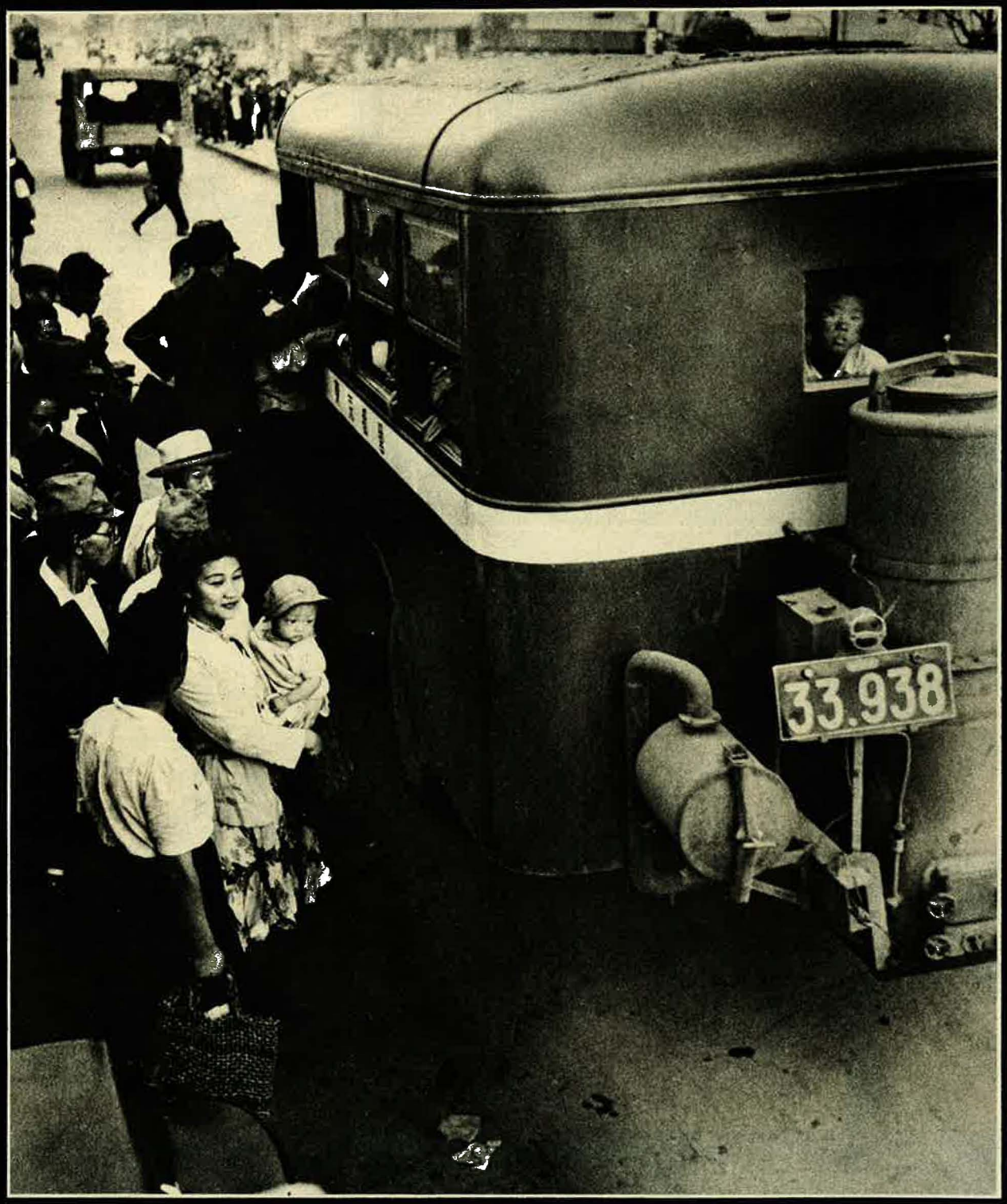
Keio U.

Ikuta
& Tanaka
Jan '51

悲劇・太平洋戦争　第２弾

# 敗戦後の首都・東京の姿は…

## 米国防総省の秘蔵フィルムから




食糧の買い出し客などでスシ詰めの都営の木炭バス。お尻に大きな釜をつけている。木炭バスは、燃料が軍事優先で民間へまわってこなかった戦争中に考案されたが、戦後も重要な交通手段として利用された（昭和21年10月）

These are pictures of postwar period that I thought I might enjoy seeing.

# ほかほかひろば

岡田熊ん蜂選

**▼コント▲**

新セリフ

「ほかほかめしやー」といったらみんなニコニコ集まってきた。

——お化け

（浦和・小山素一）

**▼回　文▲**

今朝うまし、煮しめにめしに、しまう酒。

（東京・山中由起子）

**▼川　柳▲**

ママごはん子供ライスでパパはめし

（座間・田村百合子）


**選評**　ご飯の呼び名もいろいろで、カレーライスやオムライスはライス。カレーご飯でもオムめしでもないようだ。焼きめしや釜めしはめし。焼きご飯や釜ライスではおかしい。

駅弁の名では、下に「弁当」「すし」とつくのが圧倒的に多く、ついでかにめし、とりめし、ほたてめし、たいめし、うなぎめしなどちょっと野性的に「めし」と名乗っているのが多い。

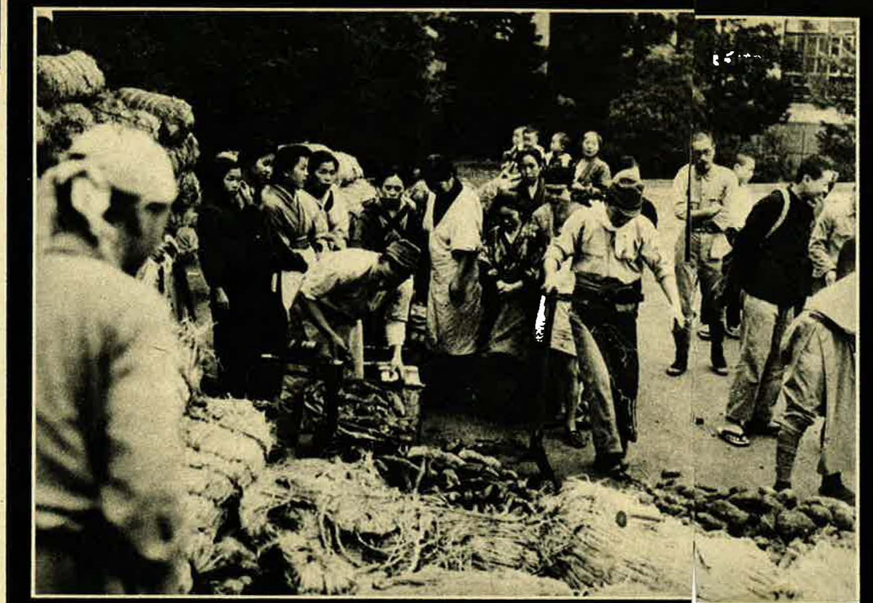
気取ったママも、車中ではローカル色豊かな〝めし〟をどうぞ。

## 作品募集

食生活、お米、農業に関係のあるほかほかユーモア（コント、川柳、迷セリフ、狂歌など種目自由・未発表作品に限ります）をお寄せください。入選作「毎週3編に各3,000円進呈」。応募作品は、一切お返しいたしません。

〒104-91東京京橋郵便局私書箱202号「ほかほかひろば」係。





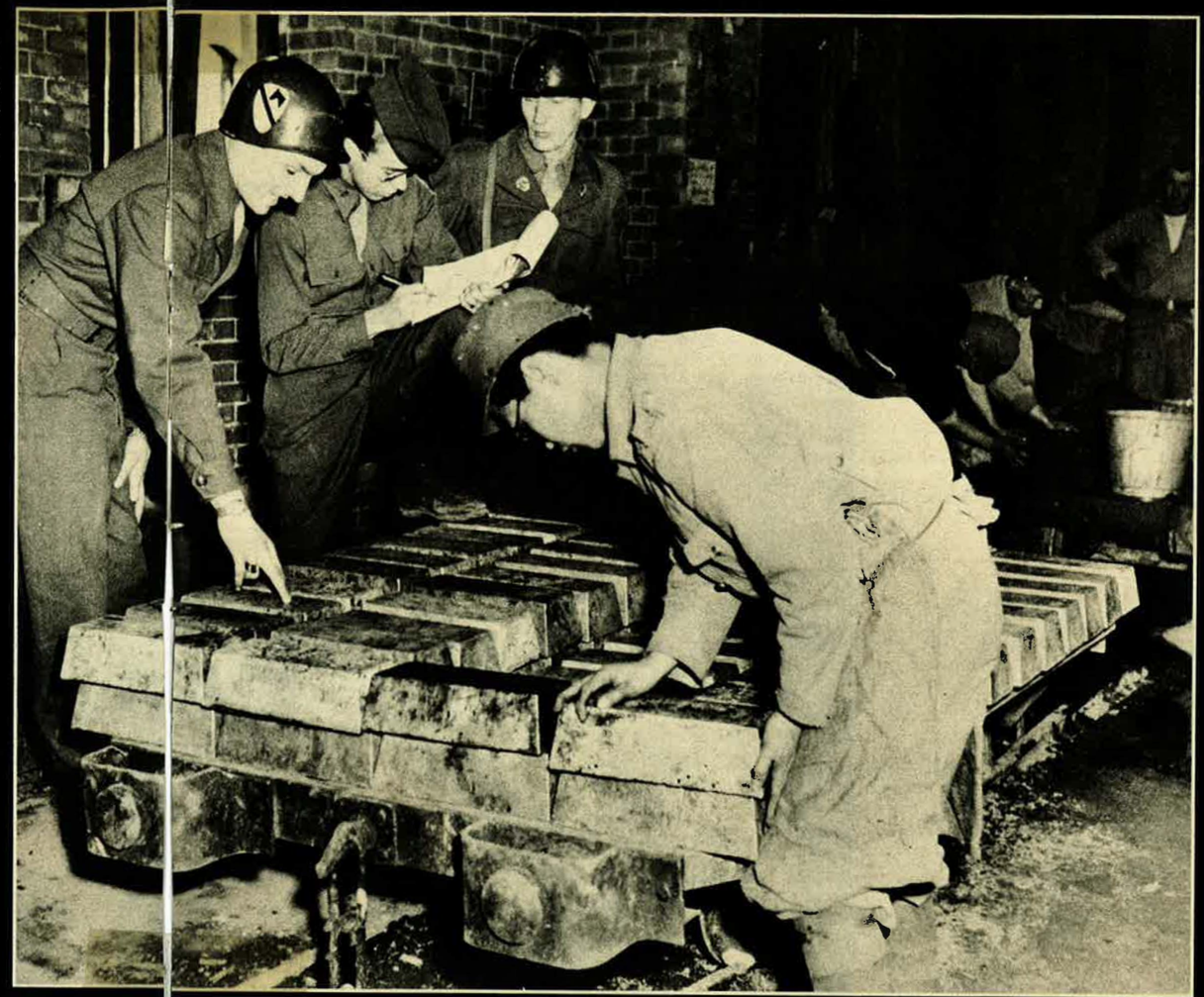
敗戦の年、コメは大凶作だった。加えて外地からの復員、引き揚げによって人口が増加、食糧事情は悪化の一途をたどり、多くの人は餓死の危機にさらされた。１日１人約２合のコメの配給は遅配、欠配が続き、１カ月以上も配給のないことがあった。写真は麻布で代用食のサツマイモの配給風景（21年９月）

昭和21年、全国的に伝染病が流行。発疹チフスによる死者約3300人、天然痘約3000人、コレラ約500人にのぼった。写真は東京駅でチフスの予防接種を受ける双子の兄弟（22年３月25日）

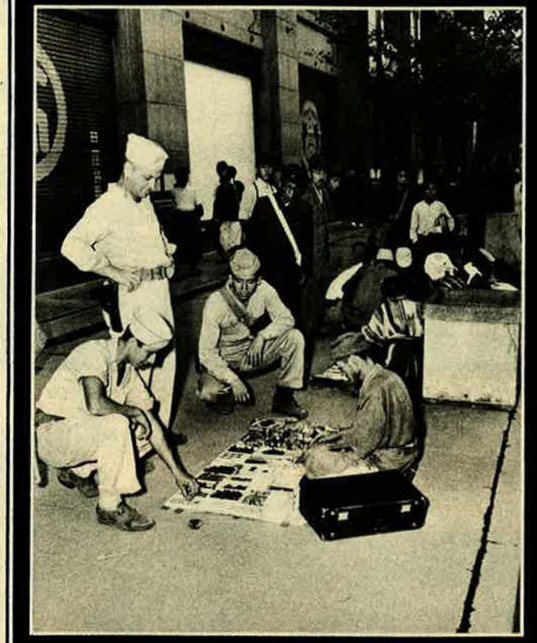
物資が極度に欠乏した都市では、いたるところに闇露店が出現し、活況を呈した。その数、都内で約７万6000軒。カネさえ出せば何でも手に入ったが、敗戦の年でコメの公定値１㌔37銭が闇値だと約33倍の12円25銭にハネ上がった。写真は日本橋・三越前に並ぶ闇露店。焼けただれた三越の壁には、戦後第１回総選挙のポスターが張られてある（21年３月）

闇市で最も繁盛したのは食べ物屋だった。ふかしイモ、かす汁、しじみ汁……。銀シャリもモツ焼きもあった。カストリ焼酎がもてはやされ、メチルアルコールを飲んで目をつぶした人もいた。写真は浅草公園の屋台、「おいしい肉入りうどん５円」と書かれてある（21年９月）

21年4月12日、東京湾で旧陸軍が隠した銀塊が発見され、米海軍の軍艦が引き揚げた。銀塊は一連の番号が打たれ日本銀行に保管された。この3カ月前には千葉県庁が旧陸軍から委託されて当時の価格で100万㌦分の銀塊を預かっていた事件が摘発されるなど、各地で金銀が見つかっている



日本橋・高島屋前で闇露店をひやかす米兵。闇市て店を張る多くは復員兵て、彼らは都市と農村を往復して食糧を運び込む一方、写真のようなささいなものを並べて今日の糧とした（20年10月）



日本郵船ビル内に設けられたダンスホールで日本女性と踊る日系二世の米軍人や民間人。彼らの多くは通訳・翻訳官の仕事に従事していた（21年9月）

昭和二十年八月十五日――この日を境に、人びとは、あの長かった空襲の恐怖から、ようやく解放された。青空を、太陽を、まともにおがむことができるようになったのである。だが、敗戦の焦土から人びとが立ち直るには、さらに大きな不安と混乱の中で、極度の耐乏生活を送らなければならなかった。街には浮浪者、浮浪児があふれ、闇市、買い出し、バラック小屋……。人びとは「飢餓の海」をただひたすら泳ぎ続けたのである。

戦後三十四年、いま私たちはありあまる食糧と物資の中で、繁栄をむさぼる。そこには、あの敗戦の姿は微塵もない。ここにあらためて敗戦後の首都・東京における人びとの生活などを見つめ直し、戦争とは、敗戦とは、何であったのかを考えてみたい。

これら写真は「月刊沖縄社」が入手した米国防総省の秘蔵フィルムである。同社はこれら写真をまとめた「東京占領」を八月十五日から出版する。（晴）

21年4月10日の第1回総選挙後、幣原内閣が総辞職。4月26日、次期政権をめぐって野党4党の協議会が丸の内の料亭「常盤家」て開かれた。左から中国・延安からこの1月に帰国した野坂参三(共産)三木武吉(自由)井川忠雄(協同)河野密(社会)20年10月に出獄した徳田球一(共産)。二度とない顔ぶれである。結局、保革連立内閣か民主戦線かて意見が合わず決裂。保守連立の吉田茂内閣が発足した

22年2月3日、皇居前広場で訓練のため行進するアメリカ軍第一騎兵師団戦車隊


九番

Keio Univ.
- old vs. new building

Tokyo Univ


中禅寺立

national Diet Library

立法府最高機関國會圖書館：東京

The National Diet Library : Tokyo

政治の最高峰國會議事堂：東京

The Diet, the highest instance for Political affair : Tokyo

總司令部と其の附近：東京

S. C. A. P. and its neighbourhood : Tokyo

再建日本の経済中心地日本橋通リ：東京　　Nihombashi Street, the Center of Commerce for rebuilding of Japan : Tokyo

Asakusa

Sumida River

Tokyo Station

平和のシンボル皇居：東京　　Imperial palace, the Symbol of Peace : Tokyo

愛と平和のニコライ堂：東京　　The Nicolai Cathedral of Love and Peace : Tokyo

交通の動脈東京驛：東京　　Tokyo Station, the Center of Communication : Tokyo

文化の殿堂國立博物館：東京　　The National Museum : Tokyo

List of items loaned to Valerie Yoshimura Nov 10, 1995

Notebook # 1

Photo of Fulbright

Leighton - project - Window Rock pictures
set of 3

- Beyond Words - by Gensenway & Rozman
- Suitcase
- Mary's Red Photo album (large) - friends
- " Red Photo album - family (small)
- Mary's family picture -
- Dan " "
- Family Picture
- Picture - Teahouse of August Moon -