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FISHING, MEMORY, AND THE STABILITY OF CULTURE COMPLEXES

BY

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A.B., University of Illinois, 1971
A.M., University of Illinois, 1974

THESIS

Submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in Anthropology
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WE HEREBY RECOMMEND THAT THE THESIS BY

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Mnemosyne and Munin

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This dissertation follows from lines of thought which other people developed before I was born and which others will explore after me. I have provided neither the first nor the last word on any of the topics covered. Here in these acknowledgements, I want to indicate portions of my milieu which have contributed to the production of this artifact but which are not mentioned in my references.

My greatest debt is to my family. I am grateful for their pride in me. Despite the fact that it has taken longer and more money than originally planned, they have shared their resources with me so that I might continue to do the things Anthropology graduate students do. But, more than the financial assistance, I am grateful for their love and their encouragement and for helping me to grow up believing life is good and nature beautiful. Each of them is my rock.

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TABLE OF CONTENTS

CHAPTER 1

INTRODUCTION

1.1 Generalities

Ever since I began reading anthropological writings, I have been perplexed and bothered by the clever ways many authors manage to ignore people in their descriptions and theorizing. A social or cultural system independent of the people who live it is unimaginable to me. This bafflement has been the source of motivation for this dissertation. In what follows, I am trying to explore how culture and the individual get together. This is my general problem.

But if left here without further focus, this problem would be unmanageable. Human Culture versus the Individual is too abstract and too ramifying to tackle directly. So, I have narrowed the subject considerably and chosen to concentrate my attention on how one person (specifically, me) experiences some limited portion of human culture (specifically, salmon purse seining in Southeast Alaska). This confinement yields one concrete example from which I shall leap to the general, for it is but an example of the larger problem.

I am interested in how one person can travel to someplace he has never before been, learn a trade, and take those skills home with him. Then, after about nine months of being away from all the physical manifestations of that trade, he can return and find that within just a couple of days he is completely comfortable again. But even here, there are too many problems and questions to deal with effectively in

a single dissertation. Therefore, I further restrict my attentions and focus upon the problem of stability. How is it possible that we human beings can achieve relative continuity within ourselves from moment to moment, day to day, and year to year? I contend that this is absolutely fundamental for the study of culture because it is the essence of cultural stability. People must be able to experience "culture" and keep those experiences lodged within themselves if "culture" is to persist.

Therefore, my goal is to conceptualize how an individual's existential performance of some portion of human culture becomes part of that individual and what concepts can be held to account for the coherency and stability of that existential performance within that individual. My thesis is that memory is the source of cultural stability and that the individual is, therefore, the causal substrate upon which and through which culture and history transpire.

The dissertation falls into two main parts. Chapters 2 and 3 are specific to salmon seining. They are ethnographic descriptions of my fishing experiences in Alaska and are intended to give the reader some of the flavor of that life. They are by no means exhaustive as descriptions nor is that their only function, but I hope they will convey a natural, one-man's-perspective on the culture complex of salmon purse seining.

Chapters 4 and 5 are my theoretical considerations. Chapter 4 searches for a way to think about culture complexes, generally.

Essentially, the objective is to describe the nature of culture's distribution in space and time. After discussing alternatives, I conclude that culture exists only in human beings, not in systems, and that its units of distribution can best be understood from within an historicism rather than some functionalism or structuralism. Chapter 5 then follows from this conclusion and evaluates several images of knowledge and memory, which could serve as the bases for a psychological historicism. By using as a yardstick how well each image conserves the basic temporality of knowing, I conclude that cognitive anthropology's favorite paradigms are misguided. I conclude in favor of a new one (described in the chapter).

In a sense, Chapter 4 attempts to dissolve the dichotomy between culture and individual and Chapter 5 the dichotomy between knowledge and behavior. This accomplished, it becomes clear that indeed memory is the source of cultural stability.

The dissertation as a whole is geared to a very personal methodological approach. The kinds of feelings I have about the coherecy of salmon purse seining could only come about from personal involvement in and with the complex. More "objective" postures would compiement but not duplicate my experiences and views. I do not deny the value of more impersonal study of culture, but I submit that my full participation comes closer to the natural situation in which culture lives. People do not "study" their culture; they live it. Occasionally, part of living one's culture is studying it, and this is true of industrial fishing where fishermen (especially the skippers) are alert to possible

ways of improving their catch. But, they are fishermen precisely to the extent that they fish and do not just talk or think about fishing. Hence, it seems only reasonable that if one wants to know how fishermen think and feel about their work that one should become a fisherman (as well as talk with them), and this is what I did.

However, the principal topic of the next few hundred pages is cultural stability and not salmon fishing. If one asks what makes "salmon purse seining" cohere as a unity and persist as a unity through time, then the title of this dissertation may begin to make more sense. The connecting concept--memory--is appropriately in between my empirical example and the more general theoretical topic. As mentioned above, however, the organization of this written exposition goes from a lengthy ethnographic sketch of purse seining to a discussion of the concept "culture complex" and then ends with conceptualizations of memory. The most important portions in my own thinking and estimation are Chapter 4 and 5, because in them I try to lay down the foundations for a new explanatory paradigm for Anthropology.

As a final introductory remark, I perhaps should explicate what I mean in saying that this dissertation is about "cultural stability." In proclaiming major interest in stability, I do not deny change. In general, I agree with Heraclitus; all things are in flux--all things are becoming. But, it seems that portions of nature become at different rates. Therefore, if rate of change is not a constant, then the problem to explain is how portions of nature can exhibit relative stability. Being an anthropologist, I am interested in cultural stability.

1.2 History of Why I Studied Where and What I Did

Given that my methodology was highly personal, I think it necessary to include a discussion of how I came to undertake the research which underlies this dissertation. Thus, this section is an attempt at honesty--what my preconceptions were, what I thought I was going to find, etc. With such considerations as the goal of this section, let me begin at the beginning.

I heard about purse seining in Southeast Alaska before I heard about Anthropology. In the summer of 1966, one of my older brothers was preparing to go to medical school, but before he entered that dark tunnel, he wanted to travel and do something adventurous. He knew someone who knew someone who had gone to Alaska and found work on a salmon fishing boat. This idea caught on amongst four graduating seniors, and they all left for Alaska. They did not necessarily expect to work on a fishing boat; they were willing to do any sort of job so long as they got to see "Alaska."

One of the students found employment in a cannery in Ketchikan, Alaska. Another ended up just travelling around. But my brother and a good friend of his stumbled onto seining jobs. One of the crew members of the Glenda Lou (a purse seining boat) was either drunk or sick and did not show up to go out fishing. Joe and his friend tossed a coin to see who would take the open position, and Joe won the toss (his friend got on another boat later).

When my brother finished the "summer season" of salmon fishing, he came home to get his books, clothes, and everything else. I was

beginning my senior year in high school. After watching his color slides and looking at other photographs, I was very eager to try salmon fishing myself. I had been fishing with rod and reel or fly fishing all my life, but the idea of fishing with nets and catching hundreds of four to ten pound fish really excited me. I decided then that someday I owed it to myself to go to Alaska and fish for salmon commercially. But, this idea slowly sank from consciousness as I got wrapped up in plans for college, football seasons, and other activities.

The next time I remember thinking about salmon fishing in Alaska was during my sophomore year in college. By that time, I was majoring in Anthropology. I was taking an area course on sub-Saharan Africa from Mary Douglas, who was a visiting professor, and she assigned Barth (1966) as part of the readings for her lecture on joking relationships. The page or two where he talked about Norwegian fishing boats immediately reminded me of my brother's experiences in Alaska, and I was attracted to the whole subject of joking. So, I interviewed my brother on the subject of joking on his boat. This was in the early summer of 1969. Alaskan fishing was back on my mind after almost three years without having even thought of it. But, again, the idea of going myself subsided as I got more and more involved in studying Anthropology. I put the idea of writing a comparative paper on joking relationships in my file of possible articles, but then never did anything with it.

Two years later in the fall of 1971, I was taking a course at the graduate level on social structure from T. N. Madan, who was a visiting professor. When he came to role theory, he concluded that it was perhaps

a very attractive way of dealing with human actions but that it could not synthesize the data it analyzed. As part of the discussion, I dredged up the small crew of an Alaskan salmon fishing boat as an example of a rich social life which role theory would mutilate. It seemed that a description of the social life on such a boat in terms of role theory would be only a couple of pages, and then it would not tell someone very much of the quality of that life. This was the second time that Alaskan fishing had come to my mind relative to some anthropological problem, and I began to think that perhaps someday I could figure a way to make salmon fishing into a research project. But, once more, with the pressures of graduate school, I promptly forgot about salmon fishing and began to look around for a geographic areal interest.

After my doctoral preliminary examinations (spring of 1974), I was deeply engrossed in polishing a proposal to study mana, kinship, and land tenure in Santa Isabel, Solomon Islands. I was also reading widely in kinship literature. For unknown reasons, I happened to wonder why I believed in "systems." This wondering began to gnaw at me more and more persistently. One day I went to talk to a member of my doctoral committee (Klaus Witz), and I told him that I was feeling very unsure about my whole way of thinking about human life. I felt I was suffering from an intellectual schizophrenia--my academician understandings of human life were not as rich or meaningful as my lay intuitions. To illustrate my problem, I used the social life of an Alaskan salmon fishing boat. As a novelist, I felt I could write freely about this subject

and capture its quality (in principle), but that as an anthropologist, I would have very little to say. A fishing boat's social life depends so much on the particular people involved, their capabilities, idiosyncracies, quirks, and habits, that role theory is much too high-level to capture it easily. The few symbolic oppositions that can be found do not provide any great insights. Klaus listened patiently and sympathetically, then asked me if I had ever wanted to go fishing in Alaska. I said yes, but that it was just a romantic attraction and I could not see when I would ever have the time to spare. He then suggested that I go there for my dissertation research. This suggestion was in line with my expressed primary interest in cognition, but it had not occurred to me as a serious possibility.

About four months later, I experienced an intellectual catastrophe. My ruling paradigm crashed in the course of a graduate seminar on cultural stability. Certain concepts--such as "rule," "context," "semantic feature," and "isomorphic structures"--lost their sparkle and attraction as explanatory notions. As a result, I began pondering what my Solomon Island dissertation would look like, and I saw that it was completely predictable. Facts would be variable, but they would be placed in completely standard explanatory paradigms that had been undermined in my thinking.

Disheartened with Anthropology in general, I once again talked with Klaus. After listening to my confusion for a while, he once again suggested doing my dissertation research in Alaska. I was enthused by the prospect of commercial fishing primarily because I wanted a recess

from academic life. Klaus felt that this enthusiasm was essential if my research were to be a success, and he pointed out that one can study cognitive processes anywhere in the world. Furthermore, he suggested that research in Alaska would be more challenging because I would be unable to lapse into simple ethnographic description of the "my folks do it this way" genre. Thinking about these things for a week or so, I finally decided to shift my dissertation plans.

I called my brother Joe and asked him the name of his old skipper. I also asked if he would write a letter to his old skipper telling him about me. When Joe sent me a carbon copy of his letter to David Peele, I then wrote a letter myself asking for a job the upcoming summer (1975).

In March, I got a letter back saying that if the Glenda Lou went seining that season (and did not just stay trolling) that he would be happy to have me on the crew. In the meantime, I had received $600 from my department's summer research funds. So, I was all set to go fish.

This, then, is the history of why I studied where I did. But, it is only part of the whole story. As soon as I heard the news that I had a job lined up on a boat and had some money to get there and back, I began to prepare my head for the impending experience.

My proposal to my department's summer research fund committee had listed some research objectives, but they were very general areas of interest rather than specific fact-finding procedures. Essentially, I wanted to put myself in the situation of a working crew member and wait

for something wondrous in that situation to hit me on the head. I knew there would be several possible directions to take and that I would have to focus on something in situ. I was interested in how "organic solidarity" actually works, how I was going to learn to fish ("enculturation"), and how routines develop and stabilize. I knew that data pertinent to these topics would abound, but I did not know any specific phenomenon which I could "study."

I decided to travel to Alaska by car so that I would have time to divorce my thinking from my graduate student milieu and so that I could keep a perspective on climatological and topographic differences. Also, it has never failed that travelling by car frees my mind of clutter and restores my wa (Clavell 1976:649).

Despite minor upsetting events during the trip, which were eradicated driving through British Columbia, I arrived in Ketchikan, Alaska, on the state's ferry system about five o'clock in the afternoon of June 12, 1975. It was pouring rain, I had no place to stay, my money was low, and the town was a great surprise. I had imagined boarded streets and little shack houses, but was relieved and also disappointed to find two high-rise apartment buildings (twelve stories), paved streets, clean gasoline stations, and businesses as usual. Within about ten minutes, my John Wayne images of "Alaska" had evaporated.

Six weeks or so after arriving, I finally went seining. I loved it! After one day, I had stumbled across my "phenomenon:" making a set, which means putting the seine in the water and hauling it back in. In this one episodic, highly sequenced, repetitive group operation I recognized the heart of purse seining.

Once mastered, purse seining is an exquisite social joy. I have always enjoyed team sports, and purse seining requires more teamwork than football or basketball. And, one can make decent money in a good season. So, in spite of a pinched nerve in my right shoulder and the sting of jellyfish on my face and the heat inside rubber rain gear and the physically hard work, I really enjoy seining, personally. It is hard for me to imagine what my dissertation might be like (those chapters specifically on fishing) if I had not genuinely liked seining. I know it would be different. Many times when my motivation for writing has been low, I have tapped into the pleasantries of my fishing experiences and been able to continue. When I have had to rehearse mentally particular episodes related to recurrent happenings of seining, I have been enervated by the mental effort, whereas if I had not personally enjoyed seining these times of forced remembering would have been intolerable. This is not to say that I love everything associated with salmon seining. Quite the contrary, there are many aspects which make me happy to be back in a university milieu with typewriters, books, and citation expectations. But seen from a distance and as a whole, I feel personally enriched by my fishing experiences beyond the limited sense of it as dissertation research. It has increased my self-confidence, calmed my mind, and given me happy memories.

In conclusion, I went to Alasks with the intention of forcing myself to do something different from a normal ethnography. I wanted to study how one individual can partake fully of a functioning culture complex. I wanted to see how much of the social life on a small fishing

Plate 1. Author being happy

boat depends on personality rather than social structure. I was oriented to a strongly cognitive approach stressing real-time functioning of thought-feelings. These were my preconceptions. And, after exposure to the situations, I responded happily and personally.

1.3 Methodology

By the term "methodology," I refer to the ways in which a researcher obtains his data. It should be noted immediately that methods of data collection are intimately related to theoretical paradigms, for one of the functions of theory is to specify what sorts of factual materials constitute significant or meaningful data. What I want to describe in this section is the overall strategy I employed to obtain my data, not the minute specifics.

Fundamental to my whole enterprise was getting myself engaged as a fully participating member of my chosen "society." Concretely, this meant I had to get employed on a salmon seining boat. I wanted to experience my object of study from within. I was to be my own informant, but first I had to undergo direct participation.

Ever since Malinowski's Argonauts of the Western Pacific, "participant observation" has been one of the trademark methodologies of Anthropology. As I first pondered how to study Alaskan salmon fishing, I naturally considered this would be my principal means of learning about seining. That is, I immediately decided to go seining myself and not just talk with fishermen, submit questionnaires, do archival studies of production, and perhaps go along on a boat as an "anthropologist."

This initial decision has had drastic consequences, but at the time, it seemed only reasonable.

The rationale for selecting "participant observation" as my principal research methodology was as follows: (1) I wanted to know how an individual learns portions of human culture; (2) I wanted to know how an individual thinks about something he has learned; (3) I wanted to observe the process whereby a small group becomes a functioning seine crew; and (4) I was keenly interested in how a crew member cognitively organizes his own work as well as how he sees others'. While such information could perhaps be obtained by other means, becoming a crew member solved them all at once. As a maxim, the procedure was guided by the idea that if one wants to know how fishermen think and feel about their work, he should go do that work himself with fishermen as his teachers, that is, become an apprentice.

In the case of Alaskan salmon seining, it is possible for a complete novice to accomplish such an apprenticeship. Most of the cultural milieu is familiar to Americans--everyone speaks English (and possibly other languages at home), participates in United States nationality, and so forth. Besides, it is quite common that young men and women come to Alaska in the summer to seek employment connected with the fisheries. I was amazed to discover that on my first boat, four of the seven of us were from "down South" (lower forty-eight states) and had come to Ketchikan for roughly similar reasons, that is, to do adventurous summer work. Thus, while Codrington (1972) and Schärer (1963) spent twenty years living with the people they wrote about, an American

studying Alaskan salmon seining can become equally familiar with his subject in much less time.

With such things in mind, I left graduate school to become an Alaskan salmon fisherman. And, I succeeded in this undertaking. My role was that of a "sponge"--I went to soak up everything I could regarding salmon fishing so that upon my return I could squeeze that wealth of information out of my informant (me).

What was initially conceived of as a one season, single-shot at fishing became an active skill which I have now exercised three seasons on two boats. Someone my first year told me that if I made it through my rookie year and still liked the work, I would probably keep coming back. In the spring each year, I feel strong pulls toward Alaska and realize now what that person was talking about. Now, I have overcome the first year's dilemma of how to be simultaneously both a participant (a fisherman) and an observer (an anthropologist), which was one of my sore points initially. As a result, I can now go seining and enjoy the change of scenery, of people, of personality. Part of me belongs in Alaska and always will.

But, my first season (summer and fall of 1975) held some surprises with respect to carrying through my planned methodology. Once I began working with my crew, I found it very difficult to come out of the "closet," so to speak, and tell them that I was studying fishing. Usually, in ethnographic fieldwork situations, the anthropologist is socially superior to his natives (cf. Richardson 1975). Alaskan fishermen do not quite see the world this way. Thinking back, I find it

difficult to imagine an "anthropologist" establishing smooth and easy rapport with fishermen. It could be done, of course, but it would take considerable social skills. But, all this was irrelevant to my first year's situation because I was first and foremost a co-worker--this was my reason for being on the boat--and my special problem was how to uphold my end of that relationship (be a good worker) while gradually arranging to conduct taped interviews with my co-workers. I had to find a way to tell them that I was an "anthropologist," but this had to be done in such a way that it remained a secondary aspect of my social posture vis-à-vis the rest of my crew.

As soon as I met the crew, they asked me what I did at home. I told them the truth: that I was an anthropology graduate student. Whereupon three of the seven on the crew (the other young men from "down South") told me that they had taken Anthropology courses in college! This was a far cry from the typical "natives" I had read about in ethnographies. Because of the nature of seining employment relations, I knew that I would have to prove myself to the rest of my crew as a good worker; they would all be evaluating me the first few weeks of the season.[1] So, I felt that I could not tell them at the outset that I was an "anthropologist" there to do research on seining because this revelation would have jeopardized my job, or so I felt. Eventually, after walking a mental tightrope between selves for about two months, I came upon a way to reveal that I was not only "the big guy" co-worker, but

[1]The first time I met my skipper, he told me this.

also doing research on salmon seining. This happened after I had proven my capabilities as a seiner, and, as a matter of fact, could have happened only then because the end of my dilemma came when I got my fellow deck hands interested in the operation of seining themselves. I slowly cultivated a sense of wonder in them, which I already felt, by pointing out how miraculous it was that we could come together as a crew to make a set. At first they did not see the miracle of which they were a part, but by persistent Socratic questioning I led them to see our work together in such a light. Once this was accomplished, I told them that I thought I would like to write my dissertation about seining, and they were interested.

Interviews with each of the other three deck hands followed, and I had some more objective data in addition to the wealth of first-hand knowledge I had learned myself. From that point onwards, I was able to ask "anthropologist's" questions without endangering my posture as co-worker. A tremendous feeling of relief accompanied this turning point, and I felt that I was on the road to "living intimately with strangers" (Madan 1975:147). I was always having to fully experience seining, and then, later reflect upon those experiences. I had to deal with my crew social relations as a full-fledged member, and then distance myself from my milieu and look at those same relations as an anthropologist. First I would wear my sou'wester fishing hat, then my academician's mortar board.

The climax of this dialectic is, of course, what I am writing now and in what follows. During the past three years, I have spent about

three months each year absorbing more "data" concerning salmon seining and about nine months each year reflecting upon what I absorbed earlier.

All together, I have seined for salmon in Southeast Alaska three seasons. The first year, I worked on the Glenda Lou, a "local boat" out of Ketchikan. My second year was again on the Glenda, but the crew of seven had two new people. This second year, I was "deck boss" and generally regarded as a good fisherman. My third season, I changed boats and worked on the Jean D. We began fishing in the Puget Sound and then went up to Alaska when that season opened. So, I have personally experienced two sorts of variations with respect to salmon seining: (1) differences resulting from slight personnel changes, but holding the boat hardware and skipper's style of seining constant (my first year compared with my second); and (2) differences resulting not only from personnel differences but also those obtaining between styles of seining and differences in boat hardware (my first and second years compared with my third). The Glenda Lou was an "Indian boat," the skipper being Haida, and the Jean D was a "Seattle boat," its skipper being a native of Ketchikan, but white and now living in Seattle. I have never worked on a boat with an inexperienced skipper, nor have I been fortunate enough to work on a "high liner," that is, one of the boats with very large seasonal catches. But, my intensive personal experience with seining is a reasonable sample.

In addition to my first hand experiences (recorded both in my memory and partially on audio-tapes made during my first two seasons in the form of a "talking diary"), I have several hours of taped

interviews with my first year's fellow deck hands. And, of course, I talked a lot with seiners from other boats.

Owing to the time requirements of being a working (and paid) crew member, I was generally unable to take notes or in some other way record specific happenings at the moment of their occurrence. That is, I was usually totally involved in the participation half of participant observing. For this reason, perhaps it would be a better description of my principal methodology to call it absorptive participation[2] rather than participant observation.

In general, it is clear that my approach to data collecting was very subjective and geared toward later introspection. What this may lose in replicability, it gains in richness, richness both in the sense of finer detail and in the sense of a broader spectrum of data. Following from the nature of my data, this dissertation is concerned almost entirely with how culture lives in the individual. In those portions where I discuss seining per se, I have drawn directly from introspection. In the more abstract, theoretical chapters, I have used introspection as the testing grounds for intuitions concerning the nature of culture's residence in human beings. To those who would follow the adage that if you cannot count something then it does not count, I would respond that human life is a sea of subjectivity from which we as humans cannot escape, rather, we must learn to swim.

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[2]This particular phrasing was suggested to me by Robert Besse about three days before we arrived in Ketchikan in 1975.

CHAPTER 2

SOUTHEAST ALASKAN SALMON PURSE SEINING, A GENERAL PICTURE

2.1 Introduction

The purpose of this chapter is to give a general feeling for what it is like to go seining for salmon in Southeast Alaska. In attempting to convey this, I shall operate at several levels of description, ranging from aspects of the geography to my own emotional reactions to specific happenings. The style of writting will vacillate between the usual ethnographic, authoritative revelation of facts to more literary, narrative recreations of moods and interpretations. Hopefully, this chapter will provide information a person interested in becoming a salmon fisherman would find useful, and, at the same time, provide a backdrop to subsequent analytic chapters.

The experiences described here come mainly from my seasons of work on the Glenda Lou out of Ketchikan, Alaska. These are the primary data for the entire dissertation, and they shall be examined in finer and finer detail later (Chapter 3 and Appendices A and B). The general plan of this chapter is gradually to click the lenses of an ethnographic microscope to higher and higher powers, to pass from the region as a whole to the town of Ketchikan to the seine boat I worked on.

I want to emphasize that this is a personalized ethnography with only limited objectivity. I do not claim to be an "expert" on the region, the town, the history of purse seining, or the habits of salmon. I would find it difficult to be skipper of a seine boat. On the other

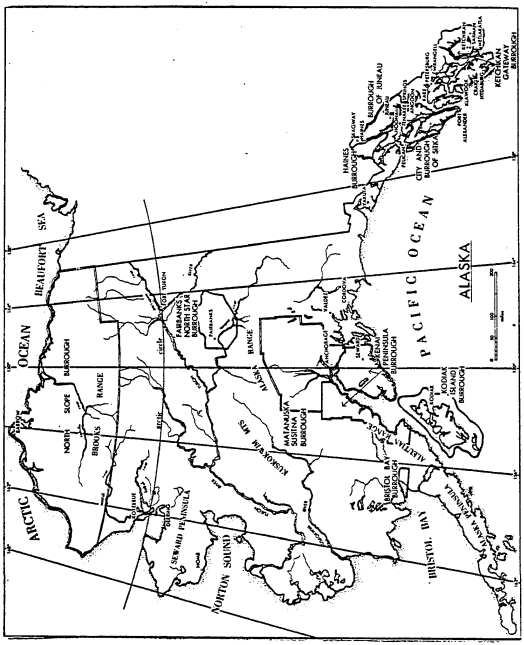
hand, I did experience the culture complex of salmon purse seining, and this is what I shall write about.

2.2 Southeast Alaska, An Overview

One way to write about a geographic region makes use of all sorts of quantitative measures--delimiting meridians, elevations, precipitation averages, miles of coastline, monthly temperature averages, and on and on. I refer the reader interested in such numerical facts to any good atlas, National Weather Bureau reports, Bureau of Census reports, etc.; they do not mean much to me. What I want to write about is what all or some of those kinds of statistical information feel like when you go to Southeast Alaska rather than read about it. So, instead of numerically describing the "setting" of my fieldwork, I shall discuss several dominant aspects of the environment as they affect someone living there.

Southeast Alaska (see Map of Alaska) is a land of grays or blues and greens. In sunshine, the visual spectrum is about equally divided into blues (the sky and waters) and greens (the forests). Most of the time, however, the cloud cover and drizzly rain mask and transform the land and seascape so that everything is a varying intensity of gray. Black and white film is sufficient to capture the normal look of things, but the occasional clear days are justification for splurging on color film.

The coastline of the large archipelago and narrow strip of mainland, which together make up the 35,527 square miles of the region's



Map of Alaska

land, is fiord-like and cut by numerous streams and rivers. Mountains shoot up from the ocean to heights of several thousand feet, enveloped most of the way in their cloak of evergreens. "Beaches" are rocky and rugged rather than sandy surfing spots.

The offshore Japanese current and the generally southerly winds produce a mildness one might not expect at these latitudes (roughly, 55°-60° north). The average annual temperature, for example, is warmer than in central Illinois, and the range is much less. Typical summertime clothing in the area would be bluejeans or corduroys, denim or flannel shirt with or without undershirt depending on the wind. Once in a while, shorts, sandals, and tank-tops are in order, but these are an unusual uniform. Incidentally, clothing codes are about the same for men and women of the "outdoorsy" class.

As the moist ocean air rises over the islands and mountains, it condenses to produce one of the dominant aspects of life in the region--rain. Tourist folders seldom indicate the overriding prevalence of rain in the area--their pictures are of clear days, mountain tops against incredibly blue skies, glaciers sparkling in the sun, etc.--but residents survive or not depending on their emotional ability to live weeks at a time without seeing those blue skies and sunshine. This is more true of the Ketchikan vicinity than of Haines or Juneau, for within the region there is a rainfall gradient from north (less) to south (more). Seattle, reputed by "southerners" (people of the forty-eight contiguous states) to have an inordinate amount of precipitation, is a sunny vacation spot for Ketchikan residents. Unlike mid-Pacific island

rain, as in Hawaii, for example, where rainfall is usual but confined to a predictable daily regime, Southeast Alaskan rain lasts all day in intermittent sprinkles.

Language habits divide "rain" into quaint categories such as "Oregon mist," "Washington drizzle," and "Ketchikan rain," and it is a frequent topic of jokes, conversation, and comments. One of my favorite bits of local grafitti was, "If it don't rain today, it's sure gonna miss a good chance." Outside the Ketchikan Chamber of Commerce there is a twelve or fifteen foot "rain gauge" which shows the annual accumulation of rain to date. A popular souvenir item is a T-shirt with the Ketchikan "rainbird"--a stork-like creature wearing rubber boots, raincoat, and umbrella. These examples reflect the permeation of rainfall in residents' consciousnesses, but there are more subtle effects of the liquid sunshine.

"Normal" summertime weather comes to mean drizzle, 55°-60° F, and southeasterly winds 10-15 mph. Umbrellas are scarce, yielding to rain suits, and eventually residents get over the idea of "rain protection" altogether. Rain makes seining more comfortable (cooler), and it is necessary to fill up and cool down the streams so that the salmon will go up them to spawn. Lastly, the dampness has a tendency to keep people indoors, which means in the bars if you live in a small boarding room or on a boat. These are some of the psychological and social correlates of the precipitation statistics in gazetteers and atlases.

The ocean itself seeps into one's awareness rather thoroughly. It is all around you, and you know it is responsible for the rain. If

you want to overcome a sense of geographic claustrophobia, you will have to travel by plane or boat because there is no highway connecting the towns and villages of Southeast Alaska to themselves or the mainland. The standard explanation for higher prices on most goods (roughly, $1.25 in Southeast Alaska equals $1.00 down south) is "freight," and this reminds you of the ocean. The towns, running parallel to the coastlines as they do, look very differently depending on the tidal conditions—tides vary fifteen to twenty feet from ebb to flood and are very strong. In these and numerous other ways, one becomes aware of the ocean.

The mountainous landscape combined with the watery aspects of the environment adds to the overall feelings of isolation and closeness. From a distance the forest covered slopes look like ideal places to hike and camp. Inside the woods, however, one discovers that it is difficult going. The terrain is exceedingly rugged, very wet, and it is hard to tell if you are standing on terra firma or just more partially decayed vegetation. According to a friend who had worked fifteen years in the logging industry, it usually takes a green worker about three months to learn how to walk in the woods easily, and that is with the advantage of cork boots (spiked soles). Up about 2,500 feet, however, the vegetation changes to Alpine and the world is alive with color, clean air, and an incredible sensation of freedom, provided, of course, it is one of the sunny days. On the average, the thick northern rain forest of Tongass National Forest is shrouded in clouds and mist and foreboding, the peaks of the mountains hidden by the low slate-colored sky.

The last "dominant" aspect of the physical environment is the rather extreme changes of the diurnal cycle as the seasons change. In summer, a Midwesterner cannot help feeling strange that it is 11:00 PM and still the sun has not set. By midnight, dusk has arrived. But the sun does not rest long and is back on the job by 4:30 AM. These long summer days hold little time for very tired salmon seiners to catch some sleep, and when in town, the "night life" gets a late start so that fishermen frequently walk back to their boats at dawn.

By now, the reader is probably conjuring images of the region. This is what I hope is happening. To some, Southeast Alaska will appear depressing, too close, underwhelming, a good place to avoid. Others, however, will get contentedly pleasant connotations from my descriptions. They will imagine cool days, turtleneck sweaters, clean air and water, hot coffee, tobacco smoke, gray ocean waves surging into rocky shores, neighborliness. I was one of the second group. I loved the scenery, rainy days, and people, and I rejoiced in the occasional exception when the blue of the sky reached down close to the horizon. This was a fortunate preadaptation else my fieldwork would have been agnoizing; people of the first sort seldom last longer than a month or two.

The people who live and work in Southeast Alaska add to its attractiveness. One of the ways they do this is by not crowding up the place with masses of nameless faces, that is, there are not too many of them. As a point of rumor, there is supposed to be one bald eagle for every two people, or vice versa. Table 1 gives some of the general features of the region's population.

TABLE 1

Basic Population Characteristics of Southeast Alaska

Southeast Census Districts	Population							Households					
		Sex		Race			in group quarters	Total			Negro & Other Races		
	Total	Male	Female	White	Negro	Other		#	Population	Aver. Size	#	Population	Aver. Size
Angoon	503	265	238	114	---	389	8	134	495	3.69	69	388	5.62
Haines	1504	786	718	1175	---	329	35	424	1469	3.46	69	297	4.30
Juneau	13556	6897	6659	11422	106	2028	212	4223	13344	3.16	479	2041	4.26
Ketchikan	10041	5232	4809	8527	44	1470	239	3006	9802	3.26	328	1463	4.46
Outer Ketch.	1676	917	759	757	5	914	53	391	1623	4.15	189	914	4.84
Prince of Wales	2106	1239	867	1497	6	603	159	560	1947	3.48	128	590	4.61
Sitka	6109	3227	2882	4611	36	1462	266	1767	5843	3.31	321	1451	4.52
Skagway-Yakutat	2157	1184	973	1338	---	819	33	607	2124	3.50	156	814	5.22
Wrangell-Petersburgh	4913	2585	2328	3759	4	1150	42	1473	4871	3.31	242	1148	4.74
Totals	42565	22332	20233	33200	201	9164	1047	12585	41518	3.30	1981	9106	4.60

This table was taken from information in the 1970 U.S. Bureau of the Census Characteristics of the Population, Vol. 1, Part 3, p. 52.

From the table, one can derive the fact that there are 110.4 males for every 100 females. Also, the average "White" household has 3.06 persons in it whereas the average "Negro & Other Races" household has 4.60 persons. Although these figures by themselves do not conclusively show that there are a lot of young "white" men in the region, actually there are.

Typically, "frontier" situations manifest unusually high proportions of young men, that is, the normal age-sex population Christmas tree is skewed by relatively large numbers of men aged twenty to thirty. In recent decades, Alaska has moved dramatically from the "frontier" pattern to the more normal, settled pattern. The indigenous peoples, of course, have always had a different demographic profile, but the white population is increasingly moving toward a more balanced picture. Seasonally, Southeast Alaska gets invaded by young workers, but they are of both sexes nowadays rather than just men. In spite of these trends which would make the population of Southeast Alaska more and more like other states, there remains a residual of the old "frontier" ethos and actual demographic statistics still show traces of Alaska's former image.

The indigenous population (Indians) forms the bulk of the non-white category. As Rogers (1967) has argued, Indian is the only category of the population that really conforms to the idea of "resident." People of other categories are likely to have been born elsewhere and very likely will move before death, even if they have lived most of their lives in Southeast Alaska. Thus, except for the Indians, Southeast Alaska is a land of "transients," be they seasonal, short-term, or

long-term. As an example of long-term transients, I knew an elderly white couple who had owned and operated a small grocery store in Ketchikan for several decades. Finally, they decided to retire, but this decision meant going back to Seattle because without income they could not afford to stay in Alaska. Thus, although they were generally regarded as full-fledged "residents" of Ketchikan, having been there for forty years or more, they moved back south (where they had been born) with their Alaskan-earned life savings.

Whether this "transient" dimension of the population is responsible for the openness and friendliness of the people I do not know, but as a generality, the people of Southeast Alaska are a very friendly sort. This dimension is an intangible and difficult to measure, but it can be sensed, even by tourists, and is part of the human experience of the region. By way of comparison, I have travelled in forty-one of the fifty states and four Canadian provinces, and I get very different feelings from different regions. Southeast Alaskans are some of the most open, goodhearted people I have found, and it is a conscious image they try to uphold.

The population is ethnically and racially heterogeneous. Without hoping to be exhaustive, the following is a list of "kinds of folks:" White, Indian, Filipino, Russian, Haida, Black, Japanese, Norwegian, Eskimo, Tlingkit, Haole, Chinese, Negro, Slav, Tsimsian, etc. These are some of the "ethnic" or "racial" words in use to describe selves and others in the region. Obviously, this list is a mixed bag for there is redundancy, inclusion and the like. "Haole" is of some particular

interest for it is used by many Indians (Tlingkit, Tsimsian, and Haida) to refer to "Whites" (Norwegian, Russian, Slav, and the rest of the Heinz 57 mixtures of European nationalities). In Hazelton, B. C., about one hundred and fifty miles from the coast, Indians used the Black Power word "Honky" for the same category. I see the Alaskan usage of the Hawaiian term "Haole" as a reflection of the overall affinities Alaskans feel for Hawaii.

The "kinds of folks" words do not necessarily partition the population at each contrast level. For example, "White" and "Indian" would normally implicate each other but the remainder of the population would not necessarily be invoked in the contrast. On the other hand, "Filipino" could contrast with "Haole," "Haida," or "Eskimo." I would be hard pressed to devise a single system of ethnic-racial nomenclature, although it could be done, because usage depends so much on who is talking to whom about whom and why. Furthermore, the task would perhaps overemphasize the phenomenon, that is, no one living there seems compelled to be faithful to a tightly reasoned logical system of words. There is no major effort to "place" people in ethnic pigeonholes. I do not want to give the impression that differences in this area are <u>not</u> perceived and have <u>no</u> influence upon behaviors, but it does not seem to be such a big pivotal point determining social life. Other ways of categorizing people seem to be much more significant in ordinary life interactions--worker versus drifter, traditional versus Youth Culture, male and female, etc. So far as ethnic-racial categories are important (as in correlations with other variables), the major categories are

White versus Indian with everything else somewhere in between.

The principal sectors of the regional economy are construction and related work, retail business, mining, logging and lumber processing, and fishing. Logging and fishing industries tend to dominate the regional ethos, and in large measure, determine the future of the region because they exploit the primary renewable resources. A more recent mode of exploiting the region's resources is the growing tourist industry. The potential for hydro-electric power is vast, but relatively untapped. For a more detailed discussion of the ways in which these economic pursuits interweave and the opportunities they hold for development, I refer the reader to Rogers.

This was the general "setting" of my fieldwork. Other than twinges of geographical claustrophobia, I reacted to it very favorably. I heartily recommend the region as a place to live or visit. It is much more familiar than most "southerners" would think, but it is still Alaska.

2.3 The Town of Ketchikan, Today

Ketchikan is Alaska's fourth largest urban area and its "First City." The former claim is based upon census data. The second reflects geographical location rather than history, for Ketchikan is the first Alaskan city on the way up from Seattle.

The image the Ketchikan Chamber of Commerce tries to foster is rather interesting. According to tourist brochures, it is "a city built on stilts" and "Salmon Capitol of the World." Also, it is a place where:

> Frontier informality and architecture are suberbly blended with Alaskan hospitality for the young and young-at-heart. Mingle with down-to-earth Alaska types such as fishermen, miners, and loggers at a rinky-tink bar, rub shoulders with a celebrity viewing Alaskan artifacts at Ketchikan's modern museum.[1]

Although a bit propogandistic, this is by and large an accurate characterization. The architecture is unusual in the older sections of town because houses are built on wooden pilings either out over the ocean or on the sides of hills. Frequently, the "yards" are wooden planks rather than grassy lawns. Fishermen, loggers, and occasional miners are quite visible on the docks, streets, and especially in the bars. The museum and library are lovely. The tourist shops, banks, and other stores often have free coffee for browsers and customers. What is missing from the self-portrayal (understandably) is the grimier, seedier aspects of the town as well as the wide basic Americana sector--neither would add to Ketchikan as a tourist attraction. I shall return to overall images of Ketchikan below, but first a few facts and categorizations.

The city of Ketchikan is on the western edge of Revillagigedo Island just across Tongass Narrows from Gravina and Penuck Islands. The jet airport is on Gravina, about two hundred yards or so from the northwestern edge of Ketchikan. The Alaskan Marine Highway (ferry system) terminal is opposite the airport. Seaplane docks line the city's "beach." The only way to get to Ketchikan is by air or water, and either way, if you take large public transport vessels, you arrive about two and a quarter miles from "downtown." In spite of these modes of arriving,

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[1] From a brochure produced by the R. W. Pickrell Agency for the Greater Ketchikan Chamber of Commerce.

cars, trucks, motorcycles, bicycles, and skateboards abound. "Dragging the main" is popular amongst teenagers and young adults just like their age-mates elsewhere in the United States.

There are 10,041 residents in the Ketchikan Census District (on the island of Revillagigedo), roughly 7,000 in the city of Ketchikan itself. Table 2 shows the breakdown of the population by race and sex.

TABLE 2

Population of Ketchikan Area by Sex and Race

Ketchikan Census District	Total	White	Negro	Indian	Japanese	Chinese	Filipino	Other
Male	5232	4470	25	582	16	-	76	63
Female	4809	4057	19	597	14	1	50	71
Totals	10041	8527	44	1179	30	1	126	134

Date taken from 1970 U.S. Bureau of the Census' Characteristics of the Population, Vol. 1, Part 3, p. 53.

About one thousand of these people are employed by Ketchikan Pulp Company and Ketchikan Spruce Mills Inc., both subsidiaries of Louisiana Pacific. KPC was built in 1954 and is located about four or five miles north along the highway from downtown. It is the largest producer of wood pulp in Alaska. The spruce mill is located downtown next to the city dock, and it is common to see Japanese, Taiwanese, and other large ocean-going freighters loading lumber. In addition to these processing industries, logging is the source of livelihood for many people, but

they generally live in logging camps on other islands, coming to town only every few weeks to "party." The point is that nowadays the forest resources tend to dominate the town's economy; strikes in these industries affect the whole town rather quickly.

Another large employment sector is provided by the fishing and fish processing industries. Now, there are only two canneries and two cold storage plants in the immediate vicinity of Ketchikan (and one cannery in Metlakatla on Annette Island)--Whitney-Fidalgo/New England Fish Co. and Ward Cove Packing Co. canneries and New England Fish Co. (NEFCO) and E. C. Phillips & Sons Inc. cold storages. Thirty years ago, there were about forty or fifty canneries operating seasonally within a fifty mile radius of Ketchikan. One may refer to the appropriate tables in Section 2.4 and better understand the reasons for this decline.

The tourist industry is perhaps the third most important source of revenues. The downtown area of Ketchikan (about four blocks total) is dotted with tourist shops. These shops tend to emphasize the Indian contribution to the region in their offerings. Silver and gold "Indian bracelets," soapstone carvings, basketry, necklaces, and fur clothing are expensive souveniers and rather indiscriminately mix local Indian cultures with "Eskimo" and other exotic traditions. The jewelry items are popular with residents as well as tourists, and seasonal workers (eg., fishermen) find them ideal Christmas gifts for friends and relatives; so the shops do not depend entirely on one or two day visitors from the large cruise ships.

There are bus tours and walking tours of the town and nearby suburbs geared primarily toward the rugged "frontier" history and/or Indian totem poles. These are quite popular all summer, and the click of Kodak Instamatics, Nikons, etc. is a familiar sound one learns to ignore. Ketchikan's "Creek Street" is my favorite spot to watch tourists. It is a beautiful stream with houses perched along its banks on high wooden pilings (remember the tides vary about twenty feet), and not so long ago it was the red-light district of town. The sign for the walking tour makes the claim that this is perhaps the only spot in the world where both fish and fishermen go upstream to spawn.

Ketchikan is a regional center in terms of retail goods, banking, and entertainment. The nearest other centers are Wrangell-Petersburg, Craig, and Prince Rupert, B.C.--all roughly equidistant in terms of travel time and about one hundred miles away. Prince Rupert is at roughly the same level with Ketchikan and Juneau in a central-place conceptualization of the region, whereas Wrangell-Petersburg and Craig are lesser centers. Prince Rupert is the closest tie-in with the continental highway system and certain amenities such as cheap draft bear. Although I have not done a central-place analysis of the region, Ketchikan seems to have the services and goods that a city of about 50,000-100,000 would have in the Midwest--it is a very compact, in terms of population, distributional center. The next level up would be either Anchorage or Seattle.

To a fisherman or logger or tourist, large aspects of the town life remain largely invisible. These "transients" tend to see only

those portions of the town life that are consciously displayed and/or very public. From associations with long-term residents, businessmen, "local" women, pulp mill workers, bar and restaurant owners, and other year-round residents. I became aware how restricted the simple seasonal visitor's vision really is. My first year, I lived in a very nice apartment in the north or west end of town and got one picture of the town. My second year, I lived in a cheap boarding house ("hotel") down by one of the fishing docks and got quite a different perspective. And my third year, I lived on my seining boat and got yet a third view. In addition, I worked as a bartender at a good restaurant/dinner club, as a singer-guitarist in the town's "nightclub," and of course as a seine fisherman. From these various positions in the social milieu of the town, I see the following social categories: (1) indigent transients or "street people" who are looking for jobs, going to bars, and generally hanging out; (2) moneyed transients who are working and thereby tied in to the town's economy, but who regard their experience as "an experience" or working vacation (this would include many fishermen and loggers although not all); (3) rich transients or tourists who exploit the town's offerings with money obtained elsewhere; (4) wage-worker residents engaged in construction, working as salesclerks, dental assistants, bank tellers, pulp mill workers, etc.; (5) small business operators--hardware, tourist shops, bars, restaurants, clothing stores, etc.; (6) industry managers, government officials, and teachers; and (7) professionals--doctors, lawyers, engineers, Coast Guard, etc. In other words, Ketchikan is a regular town. It has full-time residents

who work, buy homes, raise families, and send post-adolescent children to colleges. People of the various categories interact with each other quantitatively and qualitatively differently, just as one would expect.

Coming from a small town in the Midwest, I was struck by the firm place "transients" have in Ketchikan. Seasonal workers are very much incorporated into basic structure of the town and even make up a large portion of the town's self-image, as evidenced by the Chamber of Commerce quotation cited at the beginning of this section. In my home town, such people are marginal, not much of a factor in the general ethos, and generally "tolerated" without acceptance or incorporation. Ketchikan is very different from this.

Perhaps a more significant categorization of people in Ketchikan is one based on diurnal habits, that is, "day people" and "night people." Day people typically get up early, go to work, come home, and are asleep by ten o'clock. They are "respectable" solid citizen types, very likely with families. Night people may or may not be working, but if they are, it is likely in a job having alternating shifts, geared toward entertainment, or episodic such as fishing, logging, or construction. The favorite pastime activity of night people is "partying," which means getting drunk and laid.

The transition time of town domination by day versus night folks is from about 5:00 to 9:00 PM. During this time period, day people are eating dinner, relaxing in front of their televisions, visiting with their spouses and children, and generally being leisurely at home. Night folks are eating at a restaurant, organizing and coordinating

their evening plans with friends and acquaintances, and figuring out how much fun they want to have that night and how much they are willing to spend doing so. Entertainment choices are: going to the movie, hanging out on and crusing the streets, hitting the bars (selection of about forty-three bars), and chasing some particular member of the opposite sex. Frequently, all get done in the course of the evening.

There are four "nice" restaurant-bars available, two with live entertainment. One bar with rock music. Two bars with country-western electric music. Two bars with topless dancers. About seven or eight bars with pool tables. And one bar with a huge inventory of eight-track country-western tapes. The rest of the bars have juke-boxes and no pretentions. There are in addition several private clubs, such as Moose Lodge, Sons of Norway Hall, etc., that have bars and periodic happenings involving entertainment. As mentioned earlier, fishermen, loggers, and the like tend to frequent this sector of the town, accompanied by pulp mill workers, street people, and assorted occasional night people.

Night people generally have little to do with the Little League, Women's Softball League, the summer recreation programs, and other such typically "resident" predilections.

With these general characterizations of kinds of people and things to do in Ketchikan in mind, I shall return to overall, glossing attempts to portray the "feel" of the town.

One of the overriding aspects of this general "feel" is the high and apparently shared value on "working for a living." In many parts

of the United States, there is a fundamental cleavage between some categories like "redneck" and "hippie freak." In Ketchikan, and I believe Alaska generally, this dichotomization does not make much sense. Outward appearances such as length of hair, dress style, and certain hip linguistic patterns and phrases are not taken to mean as much as whether or not one is a working man or woman. I came to Ketchikan with a history of harassment making me see people in terms of my familiar redneck/freak mode and was greatly relieved to find it quite unnecessary baggage.

Another but perhaps related aspect of the general ethos is the sense of "frontier spirit," for lack of a better phrase. Most of the non-Indians have come to Ketchikan from "down south" and they have stayed because they like the physical environment, the climate, the people, and small town life. It is easy to stop caring about the national evening news and the person you were before. Personal style seems to be appreciated more and given wider scope. Furthermore, you may find that you are likely to like someone just because they too are living in Ketchikan with you--implying that they too like the environment and are working. Perhaps this difficult-to-describe feeling derives from the combination of small population, relative geographic isolation, and short history. An indicator of this feeling is that people can become "residents" of Ketchikan merely by living there year-round for about two to five years, quite unlike all but high prestige persons in small Midwestern towns. This is not to say that Ketchikan is an eqalitarian enclave in the contemporary United States--there are indeed status differences similar to Plainville, U.S.A. (West 1961)--but there is a

well-developed sense of "communitas" à la Victor Turner (1969).

In summary, Ketchikan is a modern Alaskan town. The streets are paved, there is no sawdust on the barroom floors. On the other hand, men and women still engage in traditional forms of the Great Chase, work is highly valued, people like to think that they could be self-sufficient in case of a holocaust, and tourists are cordially invited to visit.

2.4 Brief History of Purse Seining in the Area

What follows is a bare sketch intended to give a modicum of historical perspective on the purse seining complex, specifically as found in the Southeast of Alaska. It is not comprehensive in two senses. First, I do not trace the grand history of purse seining on a worldwide scale nor in great temporal depth. Second, I do not cover more than the major developments even within the Northeast Pacific area. For the reader seriously interested in the subject of this section, I recommend Stephen Langdon's recent dissertation (1977), Browning (1974), Rogers (1967), and their sources.

The basic differentiating criteria of purse seining, in my thinking, are as follows. (1) The fish are trapped by a net rather than entangled or hung-up. (2) One horizontal edge of the net is kept afloat, and its other edge is weighted so that the net hangs nearly vertically from the water surface downwards. (3) The ends of the net are either both attached to mobile vessels or only one is so attached and the other floats freely suspended by buoys. And (4), the bottom of the net is drawn together ("pursed up") with a separate line to accomplish the entrapment of the fish. These criteria are sufficient to distinguish purse seining from

gillnetting, trawling, beach seining, throw netting, reef netting, and other ways of getting fish out of the water with a net.

As may be noted, this definition focuses principally upon the action and construction of the net itself. The kind of vessel(s) used in the operation, including numerous items of equipment and rigging, plus alternative strategies for handling the seine combine to produce many distinguishable kinds, or styles, of purse seining. These sub-varieties can be seen both synchronically and diachronically in the continuing histories of the various seine fisheries throughout the world. But before outlining the major developments in the salmon purse seine fishery of the Northeast Pacific, a couple of general comments are in order.

The purse seine is best suited for fish which run in schools and swim relatively close to the surface, as opposed to trawling which takes pelagic and bottom fish. On the other hand, the advantage of purse seining over beach seining lies in the greater ecological (topographical) range of the purse seine; it is more mobile. Gillnetting, while perhaps equally as mobile as purse seining since the development of nearly invisible monofilament line, cannot encircle and take whole schools of fish. These advantages perhaps account for the geographical range of seine fisheries.

> Presently, the purse seine, the seine seen most often in major fisheries, takes fish at or near the surface. Its specialty is to capture such species as Pacific salmon, mackeral, the tunas, anchovy, sardine, herring and menhaden, all fish found within a comparatively few fathoms of the surface. (Browning 1974:122)

Although modified to suit the scale of the operations (cf. Orbach 1977 for description of tuna seining) as well as by personal preferences of skippers, purse seining is remarkably similar whether used in Norway for herring, off the coast of Peru for tuna, or in Alaska for salmon. Also, in at least the Alaskan salmon seine fishery (and presumably others), it is a very efficient method of catching fish.

> The purse seine probably has made more money for its fishermen than any other gear used in the Northeastern Pacific. . . . The seine is big, awkward, clumsy and wonderfully efficient in the hands of a crew who know it and anticipate its moods and do not trust it too far. (Browning 1974:144-145).

These, then, are some general characteristics of seining wherever it is found. Its design is for entrapment of schools of fish in fairly shallow water, and it is very good at this when operated skillfully.

In the history of the Alaskan commercial salmon fisheries, however, purse seining has not always been dominant, nor is it everywhere allowed.[2] During the "Early Cannery Period (1878-1900)," the beach seine was the dominant method of catching salmon for commercial sale in the Southeast Region (Langdon 1977:215). And, during the period 1905-1920, the floating fish trap took over as the dominant procurement system (ibid.:246) and continued as such until it was finally made illegal[3] in the 1959

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[2] In the 1976 Alaska Commercial Fishing Regulations, Finfish, "purse seines" were legal gear in portions of six of the ten state areas. For these purposes, "purse seine" is "a floating net designed to surround fish and which can be closed at the bottom by means of a free-running line through one or more rings attached to the lead line" (p. 76).

[3] Traps were made illegal except for those owned by Indian tribes or villages (Rogers 1967:11).

fishing regulations. With the exceptions of the early years before purse seining superseded beach seining, and the years of floating trap supremacy, purse seining has been the markedly dominant method of catching salmon in the Southeast Region of Alaska. And even when traps supplied most of the canned salmon, purse seining was a strong second (see Table 3). Today, purse seining continues as the preponderent procurement system in the Southeast Region, but it would appear that gillnetting and trolling are making inroads in terms of percentage.

In the Western Region of Alaska (see Table 4), gillnetting, both set and drift, account for the commercial salmon catch, for purse and beach seining are illegal there. The Central Region is similar to the Southeast in that there are segrated areas and districts for gillnetting and seining, respectively, but the purse seining operations engaged in those Central Region salmon fisheries tend to have smaller boats with five man crews. The "flower" of Alaskan purse seining is the fleet operating in the Southeast salmon fishery. Table 4 shows that other procurement systems can do very well, however. In trying to evaluate these alternative systems one must remember that they are insulated from each other by governmental regulations which segregate them both geographically and often temporally. Also, the various methods tend to focus on different species of salmon. So, exact comparison in terms of real, capitalistic efficiency, though possible, is rather involved and difficult.

Factors and conversions which would influence a comparison between purse seining and drift gillnetting, for example, are more complicated

TABLE 3

Salmon Catch in the Southeast Region of Alaska by Gear in Thousands of Fish and Percentage of Total per Year from 1905-1975

Period	Seine	Gillnet	Trap	Troll	Total
1905-14*	11203 (56.4%)	688 (3.5%)	7790 (39.2%)	176 (0.9%)	19857
1915-24*	14635 (35.9)	715 (1.8)	24737 (60.7)	633 (1.6)	40720
1925-34*	9746 (27.5)	704 (2.0)	23971 (67.7)	964 (2.7)	35385
1935-44*	13858 (35.3)	907 (2.3)	23161 (59.0)	1317 (3.4)	39242
1945-49*	10149 (32.9)	575 (1.9)	18237 (59.2)	1859 (6.0)	30820
1950-54*	7572 (40.7)	947 (5.1)	8491 (45.7)	1590 (8.5)	18600
1955	6435 (48.3)	959 (7.2)	4790 (36.0)	1136 (8.5)	13322
1956	9843 (53.2)	790 (4.3)	7175 (38.8)	691 (3.7)	18500
1957	7301 (56.8)	1193 (9.3)	3247 (25.2)	1120 (8.7)	12861
1958	6285 (42.1)	1104 (7.4)	6712 (45.0)	814 (5.5)	14914
1959	8832 (77.3)	1220 (10.7)	428 (3.7)	942 (8.2)	11422
1960	4023 (71.5)	705 (12.5)	188 (3.3)	708 (12.6)	5623
1961	14146 (82.9)	1263 (7.4)	1023 (6.0)	627 (3.7)	17060
1962	12710 (80.5)	1522 (9.6)	653 (4.1)	896 (5.8)	15782
1963	20156 (88.3)	1532 (6.7)	95 (0.4)	1052 (4.6)	22835
1964	20067 (85.8)	1750 (7.5)	381 (1.6)	1188 (5.1)	23387
1965	12491 (81.8)	1700 (11.1)	39 (0.3)	1044 (6.8)	15275
1966	22708 (86.3)	2068 (7.9)	644 (2.4)	880 (3.3)	26301
1967	5152 (73.0)	1115 (15.8)	11 (0.2)	783 (11.1)	7060
1968	27324 (89.8)	1646 (5.4)	251 (0.8)	1214 (4.0)	30435
1969	5101 (71.3)	1258 (17.6)	31 (0.4)	764 (10.7)	7153
1970	12174 (82.0)	1923 (12.9)	109 (0.7)	645 (4.3)	14851
1971	10496 (79.7)	1851 (14.1)	--	816 (6.2)	13163
1972	14271 (79.0)	2136 (11.8)	433 (2.4)	1214 (6.7)	18054
1973	7316 (69.8)	2125 (20.3)	43 (0.4)	994 (9.5)	10479
1974	5582 (62.8)	1740 (19.6)	113 (1.3)	1448 (16.3)	8884
1975	3926 (69.0)	1065 (18.7)	113 (2.0)	582 (10.2)	5686

*--Annual averages.

1. Sum of percentages in a year may range from 99.9 to 100.1 owing to the fact that catch figures have been rounded off.

2. The annual averages calculated between 1905 and 1954 are from Rogers (1967), Table 26, but the accompanying percentages are not from his Table 27.

3. Catch figures from 1955-1959 are from the U.S. Bureau of Commercial Fisheries, Statistical Digest No. 50. 1960.

4. Catch figures from 1960-1973 are from Alaska Catch and Production, Commercial Fisheries Statistics, Statistical Leaflets.

5. Catch figures for 1974 and 1975 are from worksheets of the Alaska Department of Fish and Game.

TABLE 4

Salmon Catch in Alaska by Region in Thousands of Fish and Percentage of Total per Year from 1951-1973

Year	Southeast	Central	Western	Total
1951	30947 (62.5%)	13214 (26.7%)	5350 (10.8%)	49511
1952	17173 (35.0)	19462 (39.7)	12396 (25.3)	49031
1953	11562 (31.2)	17994 (48.6)	7493 (20.2)	37049
1954	16542 (37.3)	21058 (47.5)	6705 (15.1)	44305
1955	13322 (33.6)	20622 (52.0)	5685 (14.3)	39629
1956	18500 (36.6)	20760 (41.0)	11336 (22.4)	50596
1957	12861 (37.4)	14086 (41.0)	7428 (21.6)	34375
1958	14914 (36.4)	19836 (48.4)	6257 (15.3)	41007
1959	11422 (45.4)	7282 (29.0)	6429 (25.6)	25133
1960	5623 (13.2)	19489 (45.9)	17377 (40.9)	42489
1961	17060 (37.9)	14235 (31.6)	13740 (30.5)	45035
1962	15782 (25.1)	37591 (59.8)	9437 (15.0)	62810
1963	22835 (48.1)	20426 (43.0)	4219 (8.9)	47480
1964	23387 (35.6)	33190 (50.5)	9130 (13.9)	65707
1965	15275 (27.1)	15752 (28.0)	25328 (44.9)	56355
1966	26301 (41.1)	24658 (38.5)	13083 (20.4)	64042
1967	7060 (33.8)	8132 (38.9)	5698 (27.3)	20890
1968	30435 (48.9)	24656 (39.6)	7209 (11.6)	62300
1969	7153 (17.1)	26171 (62.4)	8585 (20.5)	41909
1970	14851 (21.7)	29571 (43.2)	24031 (35.1)	68453
1971	13163 (27.7)	22593 (47.6)	11742 (24.7)	47498
1972	18054 (56.5)	9437 (29.5)	4468 (14.0)	31959
1973	10479 (47.0)	8345 (37.4)	3495 (15.7)	22319

1. Sum of percentages in a year may range from 99.9 to 100.1 owing to the fact that catch figures have been rounded off.

2. Catch figures from 1951-1959 are from the U.S. Bureau of Commercial Fisheries, Statistical Digest No. 50. 1960. Records from 1960-1973 are from Alaska Catch and Production, Commercial Fisheries Statistics, Statistical Leaflets.

than simple calculations of how many salmon per unit of gear. What determines "efficiency" in the capitalistic, entrepreneurial milieu of commercial fishing is cash returns per dollar investment both in capital and labor. Thus, facts like different prices per pound paid according to species of salmon, different share systems in purse seining and gillnetting, seine boats and equipment being more expensive than gillnet gear, etc. make exact comparison an intricate process requiring considerable and varied data. Table 5 provides the basic and simplest data--total catch by gear, species, number of unloadings, and the maximum number of gear units (boats) unloading in any given week during a season. These figures enable one to compute that purse seine operations catch many more fish per boat per season than either kind of gillnetting and trolling (roughly twenty-eight times more than trolling and seven times more than drift gillnetting). But, in light of the factors mentioned above and the effects of governmental regulation which are already part of "catch records," I think it sufficient to say that purse seining is very efficient in terms of relative cash returns compared to the other current alternatives provided there are lots of salmon to be caught. But, if the runs keep dwindling (see Table 3), then its relative advantage will decrease.

When purse seining appeared in the United States, it was very similar in many ways with current practice, but also dramatically different. In order to appreciate the developmental history of Southeast Alaskan salmon purse seining, I shall divide discussion of its major modifications according to whether they concern the seine itself, the vessel and skiff, the gear, or the basic labor of seining.

TABLE 5

Salmon Catch in the Southeast Region of Alaska by Gear, Species, Number of Landings, and Maximal Units of Gear Unloading in Statistical Week from 1971-1975

Year	Boats*	Land-ings	Kings	Reds	Cohos	Pinks	Chums	Totals
				PURSE SEINE				
1971	268	7910	4799	162806	326264	8505631	1496399	10495899
1972	362	12759	16800	323966	390321	11370835	2169523	14271445
1973	308	7017	8751	348679	129593	5609519	1219440	7315982
1974	305	6461	6759	235934	166682	4174219	998459	5582053
1975	249	3564	2056	61878	70201	3410938	381307	3926380
				DRIFT GILLNET				
1971	298	9660	15718	328774	159143	654434	435737	1593806
1972	375	13308	25142	449019	275428	443843	744150	1937582
1973	462	13532	24469	532150	124338	652373	524067	1857397+
1974	543	12091	15476	363834	186543	337768	665779	1569400
1975	339	6840	9087	108334	102331	350449	297655	867856
				SET GILLNET				
1971	98	3547	2041	130743	37734	80192	6367	257077
1972	114	3127	2467	134049	46289	3087	12486	198378
1973	153	4595	2730	128284	41771	16995	8995	198775
1974	147	3758	2214	82413	77526	4247	4185	170585
1975	96	2979	2224	73260	37403	80043	3761	196691
				TROLL				
1971	na	19848	311420	929	391241	104548	7599	815737
1972	901	21201	242285	1060	791808	166777	11688	1213618
1973	976	24930	307648	1222	540322	134583	10445	994220
1974	1118	29796	322129	2602	846422	263466	13805	1448424
1975	477+	17464	287337	1098	214160	76873	2784	582252
				TRAP				
1971	--	--	--	--	--	--	--	--
1972	4	92	135	8139	4688	415242	4518	432722
1973	4	18	25	1118	324	41692	226	43385
1974	4	38	15	2615	1006	109053	375	113064
1975	3	16	3	621	3262	108217	1108	113211

*"Boats" here means the maximum units of gear unloading catch in any given week.

+Beginning with the 1975 records, "Troll" is broken down into "Power Grudy" and "Hand." Power gurdy trolling had 477 units unload during its maximum landing week, but hand trolling had no such figures available.

+The 1973 total for fish caught by drift gillnet differs from the figure given in Alaska Catch and Production, Commercial Fisheries Statistics, Statistical Leaflet No. 26, p. 10. There the total was given as 1926397. This difference accounts for the discrepency between Table 3's 1973 total for gillnet and this table's 1973 sum of drift and set gillnet totals.

Data from Alaska Department of Fish and Game worksheets.

The Purse Seine--

> The operating principles of the seine and the trawl are exactly the same as they were when their web was weaved of cedar or papyrus or hemp or whatever was used for net building along the coasts where they were first fished. Only the material and motive power have been changed much. (Browning 1974:122)

The purse seine appeared on the Atlantic coast of the United States in 1826, and by the late 1870's had found its way to Puget Sound and the mouth of the Columbia River. In 1894, the purse seine was introduced to Californian waters where it was used for sardines and mackeral. But, other than a few modifications, today's purse seine has changed little from the first one to appear in this country over one hundred and fifty years ago (ibid.:120, 150, and 152).

About 1955, the structural modification of tapering the "bunt" end of the seine was devised. This enables a set to be made closer to a shore where the bottom does not just plummet down to ten, fifteen, or more fathoms. It also saves some money on materials, but the primary advantage is as above.

The other "major" change in the purse seine itself has come about through the development of synthetic fibers. This far-reaching chemical revolution has had significant effects on net fisheries. Nylon has replaced cotton webbing, and although a tarred cotton twine is still used for some patching, braided nylon twine is usually used for initial construction of seines (e.g., hanging the lead line to the webbing). In similar fashion, nylon ropes have superseded hemp. The purse lines are made of a two-layered braided nylon that has tremendous strength and also does not kink easily. The other synthetic lines (not twines)

are the usual three-section twisted rope, but much stronger and durable than comparably sized hemp rope. The third area of synthetic replacement is the "corks." Prior to the late 1950's, Spanish corks were used to keep the seine afloat, but by the end of a season they would be water-logged and heavy with little buoyancy left. Today's synthetic corks give about two or three times greater buoyancy per cork and are more durable.

Taken as a whole, the seine has not undergone major structural changes, but new materials have made it stronger and markedly increased durability. The durability has had consequences for the seine fishermen. With the old materials, a seine had to be "hung" (assembled) each year before the beginning of a season--taking off ruined corks, replacing key lines, adding a new webbing section, etc. But today, seines can last several seasons before requiring major work. Thus, as "hanging seine" is done less often, I think the technical knowledge involved will become even more esoteric and specialized, with "experts" acting as the repositories of what was once a more generally distributed skill among seiners.

The Vessel and Skiff--

The purse seine vessel of today is known as the "Western combination," or "Nothern seiner." This style of vessel ranges in size from smallish (ca. forty feet) Kodiak seiners to the large United States tuna seiners (up to two hundred and fifty-eight feet). It is a very versatile vessel design (hence, the "combination") and can be re-rigged

to accommodate trolling, crabbing, seining, and gillnetting (ibid.:103-108).

The basic concept of the Western combination vessel is to give plenty of room on deck from the stern to about midships (see Plates pp. 101-113). Prior to the development of this design, the usual commercial fishing vessel from Scandanavia to Japan (stern trawlers excluded) had the superstructure aft and the work area forward. This older design is not suited for seining, however, and even before the Western combination, beach seiners in Alaska had come up with a squarish-sterned large rowboat (ca. twenty to twenty-five feet) which was the standard boat during the "Early Cannery Period" (Langdon 1977:216).

In 1903 the first gasoline powered seiner appeared on Puget Sound, and by 1912 almost all the boats of the salmon and halibut fleets were powered by fossil fuel (Browning 1974:120). In Alaska, the first use of power boats in the seine fishery resulted in the "side-rigger," a three-vessel unit including a small skiff, the old square-sterned seiner, and the powered launch which towed the unit. Such "side-riggers" were prevalent in Southeast Alaska from about 1910 to 1920 before the motor powered seiner arrived (Langdon 1977:240-241). In that same period (late World War I), the first true Western combination was developed for the Puget Sound seine fisheries. This design moved northwards and has become completely dominant in the seine fisheries of the Northeast Pacific (Browning 1974). Today, the typical salmon seiner is diesel powered with lots of horsepower and either wood, steel, or fiberglass hulled.

Until the mid-1940's, seine skiffs in Alaska were still powered by human muscles; they were rowboats. Then, someone got the bright idea of putting outboard motors on them, and in just a couple of years, they were powered by inboard motors and eventually diesels (Langdon 1977:260). With the use of motors in skiffs, a structural modification occurred of making skiffs deeper and more rotund. Some of the fancy new diesel powered skiffs cost around $16,000 and are more powerful than many of the slightly older main boats.

Thus, summarizing the development of modern Alaskan salmon seine vessels, the principal change has been the use of fossil fuel for propulsion. Overall size has gotten larger (from roughly twenty to fifty-five feet) but is maximally limited by law. This change from human to mechanical power has brought with it a change in the basic strategy of seining, however. Before the main vessel was motor powered, seining was done mainly at or near the mouths of streams, hence beach seining was reasonable. But, once the main vessel became motor driven, fishermen could set around schools of fish and using the purse seine take them in a much wider range of sites. Nonetheless, the basic idea was still to encircle a school. Then, with both the main boat and the skiff motor powered, purse seiners could drag the seine through the water and/or hold their own against strong tides. And, the basic idea or strategy changed from "encircle as rapidly as possible" to "scoop them up" (ibid.:261-263). The upshot of all this is not only that seining is physically easier (and there are other reasons for this as will be seen below) but also that seiners today can fish in many more places than they could at the turn of the century.

The Gear--

Major developments in gear are of two kinds. On the one hand, there are the advances in electronic equipment (radios, radar, sonar), and on the other are important items of seining hardware.

Since around 1930, seine boats have commonly had radios aboard. At first, these were "receiving" only, but today include VHF and UHF "sender-receiver" radios as well as short ranged walkie-talkies. The walkie-talkies are used for sending instructions from the skipper on the main boat to the skiff while making sets, and in this context have largely replaced the systems of hand or flag signalling. The VHF and UHF radios are used to talk with other boats and the cannery offices, as well as get news on weather, extensions of fishing time, and so forth.

The principal effects of such relatively long-distance communication devices are twofold. First, the fishing boats can stay out of port and still keep informed of fishing schedules and related concerns. Second, during the day or two before a fishing period ("opening"), teams of boats can scout larger areas for fish and relay their information to the others involved, usually by means of some simple code system.

In addition to radios, many seine boats have radar, and most have sonar. Both devices began appearing on seine boats in the late 1940's. Radar is handy in bad weather (fog) and for night travel. Thus, many Puget Sound boats which also seine in Alaska find radar almost indispensable because they have to cruise back and forth along the Inside

Passage. Similarly, several "west coast" boats (ones which frequently fish out of Craig, Alaska) find radar very worthwhile. But, radar is much less common on Ketchikan "local boats," presumably because they do not travel in relatively unfamiliar waters nor at night as often. Almost all boats have sonar in the form of a depth sounder, but "fish finders" are not nearly as common on seine boats as they are on trolling boats. Navigation charts are universal, however, and personal knowledge of the waters is still highly desirable, for electronic wizardry cannot really replace lore. In general, though, since the late 1940's and early 1950's, it has been much easier for skippers to seine in relatively unfamiliar waters thanks to their electronic equipment.

With respect to basic seining hardware, there has been considerable change since the turn of the century. In the 1910's a dead roller was put on the stern of the main boat to facilitate hauling in the seine. Later in that same decade and after the boats had engines, a winch was used to pull in the purse lines, and a block and tackle suspended from an angled boom made hoisting up the bottom of the seine much easier. These two modifications together made it possible to put more weights on the lead line of the seine--an advantage when trying to entrap swift fish in strong tides. About 1930 the dead roller was motorized. This made hauling in the seine even easier. Also, the roller was placed on a lazy-Susan type platform or turntable so that the seine could be let out over the stern but retrieved from the side of the boat (Langdon 1977:250). This was the basic hardware of salmon

seining when the two "ole timers" from my first boat (Skipper and Frank) learned their trade, and there were no further major developments until the 1950's.

The crew of a purse seiner consisted of eight to ten people until the 1950's. The work was physically demanding even after the boats had engines, winches, and the block and tackle to lift up the leads and rings. Hauling in the seine still required strong arms and backs to drag it over the roller and drop it on the turntable. On the good side, the crew did not have to bother with jellyfish flapping about in the air and therefore did not wear "raingear" unless it were raining. Frank told me that all things considered he preferred the old way of seining, that is, before the Puretic Power Block.

The power block was invented by Mario Puretic in 1954. The test model was rope driven rather than hydraulic owing to finances not conceptual design. By 1955, the device was in great demand, and its Seattle manufacturer was swamped with orders. And by 1960, all self-respecting seine boats were using this new aid to hauling in the seine or had switched to using the drum (Browning 1974:147-148).

The drum was roughly contemporary with the Puretic Power Block and was designed to make hauling in the seine easier, like the power block. Although already familiar to British Columbian seiners, the first American seiner to try drum fishing was a Puget Sound boat in 1950. Its popularity grew slowly amongst Sound seiners owning both to the newly developed alternative of the power block and to Alaskan banning of the drum (ibid.:146-147).

Both the power block and the drum methods of retrieving the seine were much more efficient than the roller and turntable system, the drum more so than the power block. Recognizing this advance in seining efficiency, officials in Alaska banned both the drum and the power block during the period 1954-1958 as part of a general legislative action in the direction of conservation of their salmon resources (Rogers 1967:102). In 1958,[4] Alaskan officialdom decided to permit the power block, but continued the ban against drum seining and finally made fish traps illegal. The state of Washington, however, has always allowed drums and power blocks.

The last "major" technological development in seining gear is the brine tank. This is perhaps equally or more so an advance in vessel design as in gear. While tender boats have had such systems of chilled sea water for years in various fisheries, it is only recently that salmon seine boats themselves have had them. Most Alaskan seine boats (they cannot be longer than fifty-five feet on the keel) still use ice to preserve fish in their holds if those fish are not unloaded within the same day. But a few of the "fancy" new boats have brine tanks. From the fisherman's viewpoint, the advantages of such a refrigeration system are two: (1) in rough seas, the brine tank gives added ballast; and (2) salmon (primarily humpies) kept in brine until unloading seem to weigh nearly half a pound more per fish.[5] As a crew member, the

[4] My skipper told me he got his first power block in 1958.

[5] Based on average weight per fish (all humpies) during the 1977 season comparing the Icy Bay (with brine tank) and Jean D (without tank). The two boats fished near each other almost every opening, so the difference is not likely a reflection of catching fish from different spawning streams or at different life-cycle stages.

second advantage looms large and is apparently related to retention of water in the fish's respiratory and digestive systems.

The changes in gear have had significant ramifications in the work itself. The electronic advances have made it easy to get information of various sorts--environmental, legal, and social--without actually having to cruise back into port and deal face to face with authorities. Innovations in hardware have greatly reduced the muscle fatigue of seine fishermen as well as enabled them to make more sets per day and in a wider range of environmental conditions. In fact, purse seining has become so efficient, if allowed unrestrained technology, that certain items have been made illegal in order to give the fish a chance. The Alaskan ban on drum seining has already been mentioned as an example of this legislated inefficiency, but British Columbian seiners have their handicap too. They cannot use power skiffs. Certain advances in seining technology (such as brine tanks and "bow thrusters"--which are used on Norwegian and Icelandic herring seiners and do away with the need for skiffs) are difficult to apply to Alaskan salmon seine boats because of their limitation on maximum size. The advances in gear have reduced the crew size from eight to ten down to five or six (although some boats still have seven or eight). And, speaking as a worker on a seine boat, the advances have made what was once only a mild irritation to the hands and wrists into a non-debilitating but nonetheless very painful "seiner's curse"--red hot jellyfish in the eyes, nose, or mouth. But then, every occupation has its hazards and at least seining is not boring.

Southeast Alaskan salmon purse seining today uses a seine made of nylon webbing, nylon twine, braided nylon purse lines, polypropelene ropes, specially designed metal clasps and hooks, and it is very strong and durable. The vessel is a Western combination design of about fifty to fifty-five feet and powered by a very large diesel engine. The skiff is a powerful aquatic tractor. Radios of various sorts blare most of the day as the depth finder bleeps quietly in the wheelhouse. The Puretic Power Block dangles from the boom's "gooseneck" in the company of two block and tackles, while underneath on the deck, the power winch stands ready for any heavy work. The six man working crew lounge about in the cabin, on deck, or in the focsle waiting for news from the Alaska Department of Fish and Game to announce legal seining periods and places. No longer can this complex operation take salmon from the mouths of streams, no longer is it technologically bound to them either. This seining unit is indeed mobile, capable of making sets wherever the skipper wants. This is the current state of the art after a hundred years in the area.

2.5 Going Seining, A Personalized Narrative

Seining is an occupation in addition to whatever else it may be. The immediate goal is to catch lots of fish, and this converts directly to cash. In order to partake of this culture complex, first one has to get hired, so this is where I shall begin--with the relations of employment.

There are three structural positions on the boats with respect to their economic organization. The first of these is the owner of the

capital goods. The second is the skipper, captain, cap', skip, boss, etc.--the executive manager of the operation. The third position is crew member. There is but one skipper on each boat at any one time. The crew (here referring to all but the skipper who actually work on the boat) consists of from four to seven people, but almost always of five or six. The capital goods owner is frequently the same person as the skipper, but often one of the fish-buying companies or an individual not actually working on the boat. (About one third of the salmon seining fleet is privately owned and operated.)

The general set up is that the owner hires a skipper (possibly himself) to run the boat during the seining season. Skippers must own their own gear, minimally a seine, and have liability insurance in case they should take it upon themselves to run into dock pilings or some such egregious behavior, and their position on the boat is contractual, extending for the duration of the seining season.

The skipper hires and fires the crew. The crew "works for" the skipper; he is the "boss." Each crew member receives one "share" of the monetary value of the catch. In the not so distant past, say twenty years ago, it was not uncommon for a rookie or green hand to receive only a half share rather than a full one. The rationale for this practice was that the rookie was in this way paying for the opportunity to learn a money-making trade and that he was most likely not doing his fair share of the work owing to his ignorance of the requisite skills. The typical rookie today, however, would not tolerate such a deal, for unlike the young high school kids who used to put up with this as part

of their summer job initiation into a life-time career, the rookie of today is very likely to be a "young adult" who has been around and worked in other jobs.

The crew members sign no contracts nor in any other way make formal agreements with respect to holding their jobs for specified lengths of time or exactly what their jobs will entail. The closest equivalent to a formal signing occurs when the skipper has the crew sign and fill out their Federal W-2 forms for income tax and fill out the application for a commercial fishing license, but this may happen after some work has already been done. Hiring consists in an asking for the job on the part of the crew member and an approval on the part of the skipper. It is a verbal understanding. The directionality may be reversed--a skipper may ask a person if he would like to work on a boat--and the situation is thus not a verbal ritual. The point is that the process is governed by very informal and moral principles rather than by intricate specification and contract.

As mentioned, distribution of the monetary value for the collective catch is on a "share" system. Relative to this, there are but two categories: capital goods and labor. The skipper and his crew comprise the labor, and the capital goods owner receives payment in shares for his hardware. Typically, there are from ten to twelve shares on a boat. Each crew member receives one of these shares, the skipper receives from one to one and a half shares for his labor and from one to two shares for the seine, the remainder going to the owner of the boat, power skiff, and power block. Boats vary in how the total shares are

allocated, but two concrete examples will illustrate the general patterns.

The Glenda Lou had twelve shares. It was owned and operated by the skipper. Each crew member received one share, the skipper got one and a half shares, the seine got two shares, the boat two, and the power skiff one-half share. Since the skipper owned all capital goods, he received a total of six shares, one-half the total. But the cook was also his wife, so between the two of them, they took seven-twelfths of the total catch value.

The Jean D was owned and operated by its skipper, also, but he jointly owned the boat with his brother. It had a total of ten shares. Each crew member received one share, the skipper took one share for his labor, the seine got two shares, and the boat got two shares. How the skipper and his brother divided up the seine and boat shares was none of my business.

The only guidelines I became aware of in terms of being "fair" to the crew was that the total of the crew's shares should be about half of the total. Thus, on a boat having six crew members (skipper not included), there should be no more than twelve shares, a boat with five crew members should not have more than ten shares, etc. As the cost of gear, primarily seines and boats, continues to rise, however, this sense of what is "fair" will modify in favor of the capital goods owner.

So long as fishing is good, it behooves a skipper to purchase his own boat. In such a case, he can rake in half of the value of the fish each year. Although it may take a decade or more of seining seasons to

pay off the original costs in addition to repair and maintenance costs, it has been a sound business venture in the past. Most skippers, today, however, would advise against trying to become owner of a boat. The reason for this is simply the sharply declining salmon returns and the concomitant governmental restrictions on fishing time. If you are a good fisherman, of course, it still pays.

The logic of the share system is very different from wages and salary work, but similar to some forms of piece-work. In the kinds of production where piece-work is monitored with a group of workers as the basic production unit, the logic is very similar to the seining share system, yet different in one fundamental respect. If one contrasts, for example, the common Midwestern job of "hauling hay" and salmon seining, they are alike in that each worker can calculate how much each unit of product is worth in dollars and cents, and each type of work requires a group effort, but the salmon seiner knows the relative percentage he will make versus his employer (the farm worker likely does not, only how much each bale is worth to him). Furthermore, the hay hauler knows how many bales are out there in the field, hence how much the job will be worth to him, whereas the salmon seiner imagines that he will make a fortune without knowing in advance exactly what his catch will be. The circuitous upshot of the salmon seiners' share system is a great emphasis upon teamwork with everyone trying to do his full share of the work in order to maximize the possible returns. Thus, this system of remuneration, if not a partial cause of, is quite compatible with the ideal "seining ethos" (described later).

One of the unusual aspects of seine boat employment relations is that the crew members generally will not know the specific share system on their boat unless they have worked on it before. It is definitely an imprudent move to ask a skipper what his share system is when asking for the job (especially if he is also owner). I know of one case when a crew member was "let go" as a result of such questioning. The question by itself was not sufficient cause for the firing--the skipper was not too pleased with the man's performance up to that point--but it was the immediate cause. Of course, not all skippers would take such offense, but given that there are lots of people looking for work on seiners (the street people), that skippers are co-workers and not distant executives, and the general ethos emphasizing cooperation and a general "gung-ho!" attitude, it is perhaps wisest merely to expect a fair deal and not risk implying lack of trust. Although dealing in a high conflict-potential relation, that is, employer-employee, explicit confrontation is counter-productive for everyone concerned, and even subdued confrontation (e.g., back-biting) is approved of by no one. If one wants to know a boat's share system (and it is something the fisherman should be interested in), the best way to find out is to ask someone who has worked on the boat in the past. That is, one should find out through informal channels, hear it on the grapevine. Furthermore, it is wise to find out before hiring out on the boat. Experienced fishermen would consider this a factor in deciding which boat(s) they want to work on; rookies cannot generally be so choosy both because they may not be able to tap in on the grapevine and because they are

not in such a strong bargaining position anyway. (I should perhaps add here that there is no fisherman's union in Alaska, quite unlike almost every other occupation.)

Assuming that all of these intricacies have been glossed over and by hook or crook one has landed a job on a salmon seiner, what then will it be like?

To be precisely accurate, I cannot say what it will be like; I do not know. Each boat is a little different from all others. The spatial lay out of the boat, the particular skipper's personality, the crews' compatibility, the condition of the seine, etc. will all combine to create variation. But, there is a strong sense in which all seining operations are very much the same. This same-sense is what I mean when referring to the "purse seining complex," and it is reflected in my confidence, similar feelings found in other experienced seiners, that I could work on any seine boat. How then to communicate this "essence" of purse seining?

After attempting up to fifteen pages of "objective" description, I have decided upon a personal, very subjective style. What this mode of narration lacks in generality it seems to gain in concreteness and richness. Therefore, what follows makes little effort to discriminate between what is specific to the actual conditions of my experience with purse seining and what one might find in other cases of its actualization. This does not mean that my experiences incorporate all aspects of the seining complex, or that I fully participated in the range of possible variations, or that I think I "know" salmon purse seining

completely having partaken of it three seasons on two different boats and with three different crews. Rather, it is an acknowledgment of my "objective" incompleteness. In defense of this approach, I would remind the reader that no one can be fully objective. Anyone who goes seining experiences only a certain portion of the entirety, and it is these experiences that form in that person the unity which himself and others think of as the culture complex of salmon purse seining in Southeast Alaska. With this preamble, I shall attempt to take the reader "with me" on my first season of fishing on the Glenda Lou, only occasionally making points about general features of seining.

I heard that I could work on the Glenda Lou in a letter March 20, 1975. From that point on until well into the seining season, I could not come to a decision about what kinds of clothes to wear while working on the boat.

I arrived in Ketchikan June 12th with my car crammed full of assorted junk. After finding a place to stay, I discovered that it would be a few weeks before any seining would begin. This made me agitated and worried that my money would run out, but I used the time to learn about the town, indulge in some personal romaticizing about being in Alaska, and walk around the docks and beaches. During this time and even for a while after I eventually began seining, I was ill-at-ease in town. I was very withdrawn, regarded the bars as "rough" places, and generally felt I did not belong. This changed in the course of the season, but the change reflected my whole confidence as

a "fisherman." Looking back and given my twofold task (to do well as a seiner and to identify a decent dissertation topic), it was very fortunate that I had those weeks to compose myself sufficiently so that new experiences could enter into me rather than forcing them into pre-existing emotional molds and interpretations. In any case, for the first few weeks I was in town, I spent most of my energy getting at peace with myself and zooming in on the more obvious aspects of the setting.

Several novels, solitary walks, and three weeks after arriving, I finally met Skipper, Annabelle, and Frank. They had been out trolling for salmon, and I was exhilarated just to see the boat my brother had worked on nine years earlier and I would be working on. Although my brother had not known Annabelle, he had told me about Skipper, Frank, and several others. The first meeting was thus very much like old home week once-removed. They asked me all about my brother and told me how much they had enjoyed having him work on the boat, etc., etc. I kept trying to call Skipper, "Captain Peele," figuring that since I did not know what the situation called for, it was best to be demonstratively respectful. They had come back to town to unload their iced fish, see Ketchikan's Fourth of July celebrations, and meet with the crew. I left the boat with a salmon and instructions to be back July 5th to meet the other fellows and do some work. Already, I had learned from the conversation that being a "good worker" and a "fast learner" was my goal on the boat.

When I met the rest of the crew, I recognized one of them as a fellow I had played pool with in a bar a couple of times. He knew both

of the other fellows; all three of them were from St. Paul-Minneapolis. All were in their twenties, white, and sort of medium "freaks." Seeing them, I was very glad that I had not cut my hair short before coming up.

After shaking hands and exchanging pleasantries, we were almost immediately put to work trying to fix a leak that dripped into the focsle from the anchor winch. Frank left at this point to go about his business in town. Skipper told us to take the anchor winch off so we could caulk the deck under it. This done, he proceeded to caulk the deck; we watched. After about three minutes or so, Darrell said, "Let me do that for you, Skipper," and took over the job, the rest of us continuing as audience. Darrell's actions immediately impressed me with the idea that we were not supposed to let the Skipper do any manual, tedious work, so I was the next "volunteer." Each of us continued in rotation until we were finished with it, then we left the boat and went our separate ways to get ready to go over to Kasaan the next day and pick up some gear. All four of us "deck hands" had been appropriately eager to work, and it had become clear to me that the four of us (Darrell, Richard, Sk'eg, and myself) were supposed to do all such work.

Then began a week of "hanging seine." We did a full job of it, not just patching holes. Hanging seine means building a seine. The last time it had been done on the Glenda Lou was the year my brother had worked on the boat (1966), but it had already been done before he started work. Seines, in Alaska, cannot be longer than two hundred and fifty fathoms (five hundred yards); this is a law. So, most seines, ours no exception, are two hundred and fifty fathoms long and generally

about ninety feet deep (fifteen fathoms). The depth of the seine is legislatively regulated, but the nature of the fishing areas make a three-strip seine more serviceable than the deeper (up to five-strip) seines found in the Puget Sound. Also, seines vary in more ways than simple length and depth.

I suppose the first decision while making a seine is whether it will be used in a half-pursing or full-pursing operation. Then, the skipper would decide how "flat" or "bagged" he wanted the seine to hang in the water, whether to taper the ends or not, whether or not to use "snap rings" and how many, and on and on. None of these considerations would have meant anything to me at the time, however, even if they had been discussed. I simply followed instructions and gradually learned that we were going to build in a uniform "bag" by having a ten to eight fathom ratio between the cork line and the lead line respectively.

The crew would meet at the boat each morning and cruise on up to the old Sunny Point cannery where there was plenty of room to stretch out and hang up the various parts of the seine. First, we cut the knots off of the old cork and lead lines. Second, we added new corks and leads to certain sections of the lines. Third, Frank and Skipper cut off the bottom web strip and laced the new strip to the top. Fourth, we began tying the webbing (all three strips now being connected) to the cork and lead lines and connecting the "bridal" lines to the lead line. In the meantime, we patched all the holes in the webbing we could find ("sewed seine"). In addition to these more obvious, major parts of the seine, there are numerous items of hardware and many little jobs

associated with the finished product. Figures 1 and 2 illustrate the principal parts of a seine hung the way we did ours. (For more information concerning hardware, skills, and styles of hanging a seine, see Browning.)

In the course of this week of hanging seine, the crew got to know each other, the two rookies began their indoctrination to the boat's ways, and all four of us deck hands learned a lot about the seine and tying knots. In this last regard, I was pre-adapted, having been a good Boy Scout and learned how to splice and tie the basic knots.

Skipper was the authority on how to construct the seine, and Frank was about equally skilled at it. In the days before nylon web and twine and ropes and synthetic corks, seiners had to hang seine each year, so they had plenty of experience. The four of us deck hands were very ignorant by comparison, but Darrell and Richard both knew how to "sew" seine, Darrell better than Richard, from their previous year of seining on the Glenda. Sk'eg and I had to learn everything afresh. So, the initial organization of work had three informal "teams," each working on different aspects of the whole job. Skipper and Frank laced the web strips together and cut them to fit; Darrell and Richard sewed up holes in webbing; and Sk'eg and I got the more factory-like jobs of threading corks and leads on new ropes. This organization reflected itself not only in terms of job assignment, but also spatially in the warehouse area.

By the third day, I had learned how to sew seine and could do it almost as well as Richard. By the fourth day, largely owing to my

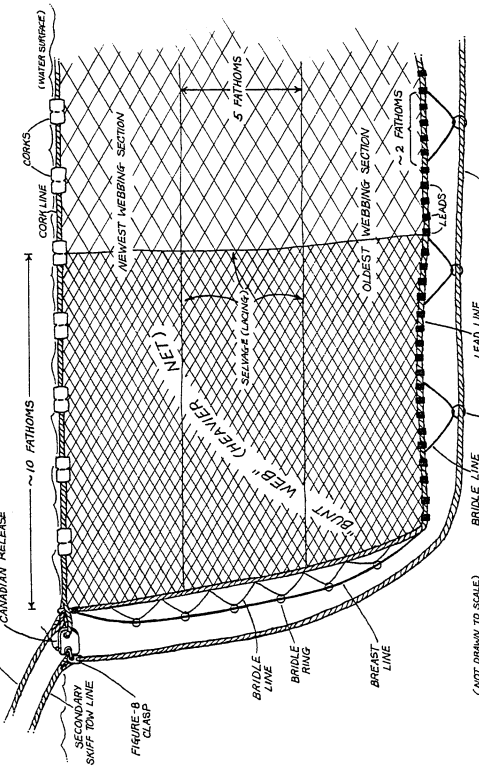

Figure 1. Skiff's end of the seine (bunt end)

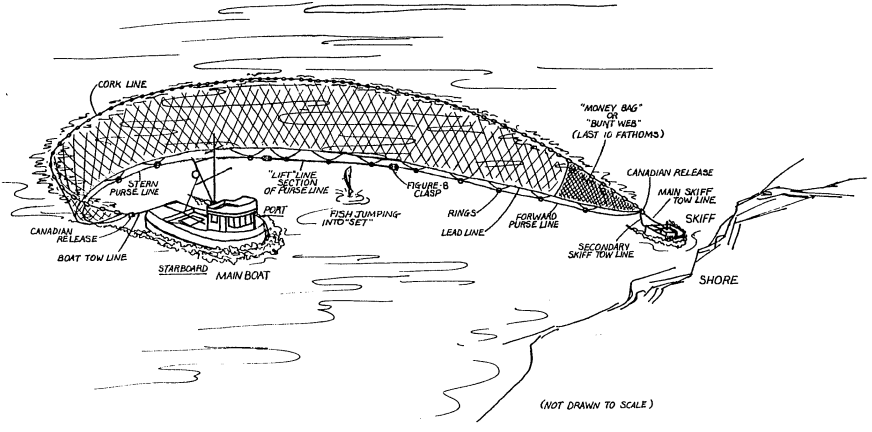

Figure 2. Major parts of the seine (set is to port)

aptitude at splicing (needed to get the bridal lines ready to connect to the lead line) and the rapidity at picking up sewing (essentially, it is just tying sheetbends), I got to help Frank hang the webbing to the cork line. Darrell helped Skipper, and Richard and Sk'eg got stuck painting all our knots with floor hardener to keep them from slipping after getting wet. Thus, by the end of the fourth day, the work organization had altered. I was regarded as next to Frank and Skipper with respect to skill with twine and knots, Darrell was very close to me (better at sewing), and then Richard and Sk'eg. None of this meant much with respect to our overall status on the boat or what we would do while actually fishing. But it was interesting in terms of changing modes of composing the informal teams. The first few days, the teams and their jobs directly reflected the general estimation of knowledge and skills--Skipper and Frank doing the most important work, Darrell and Richard next, and Sk'eg and I the least. But in the final assembly work, I was put with Frank and Darrell with Skipper purposively to balance out skill differences, thus keeping us more temporally synchronized as we worked our way along in ten fathom units, and Richard and Sk'eg were given lesser jobs. This latter organization made the overall knowledge and skills estimations invisible, for no longer was it reflected either spatially or in terms of jobs (although Richard and Sk'eg formed a visible unit).

All of this sounds as if I am bragging or quite trivial, but one reason I go into so much detail on such a small aspect of the whole season is that no one seemed to feel especially slighted or aggrandized by

Plate 2. Author "hanging seine"

I am attaching the webbing to the cork line.

what happened. As would be expected, Richard and I were more aware of the subtle status changes, but neither of us thought much of it relative to the upcoming season of working together. Everyone was willing to do whatever in order to get the whole job done. This is the essence of a good seining ethos.

During this week of preparatory work, many of the minutiae of seining seeped into my thinking almost semi-consciously. Darrell had been confirmed as "deck boss" July 7th, the first day of this work. I had come to realize that seining and fishing generally seemed to use quite different muscular patterns than any work I had done heretofore; everything seemed to require some kind of pulling rather than pushing. My body registered this difference with unusual aches and pains. The immediate environment of fishing (i.e., boats) exploits the vertical dimension of space rather than the horizontal, especially through the use of block and tackle systems. And, of course, there were a multitude of small accommodations. A sharp and easily accessible knife was a frequently used and very important tool. The size of the toilet on the boat was horrifying. Coffee and tobacco seemed to take on extraordinary significance socially rather than merely personally. And, of course, I learned more and more about what seining was going to be like. This list of small but integral aspects of the whole experience could be extended further. Looking at this time range from a broader perspective, it was my introduction to life on a fishing boat. These details modify somewhat from boat to boat, as I found out my third season when I changed boats, but they nonetheless have a certain coherency in me in

spite of particular alterations and variations. They form a ground from which and because of which I feel confident I could now work on any kind of boat more easily than I could have three years ago. All got further and further engrained in me as the season went on and as I have worked more seasons, but even after one week of being around the boat for ten hours a day they had made deep impressions in me. By and large, the kind of learning I was doing involves an ability to do something rather than an understanding of that something, it was an active knowledge rather than a passive comprehension.

Whenever dealing with Skipper or Frank in a potentially instructional situation, my whole attitude was that of a respectful student. Having been a student practically my whole life, this was rather easy to perform. But, in universities, one is supposed to accompany a displayed desire to learn with a contained critical, skeptical outlook. On the boat, I dispensed with the critical, evaluative component, for its corresponding behaviors would have been interpreted as "know-it-all" arrogance, most particularly inappropriate for a rookie. Besides, it was not seen as the Skipper's job to teach us all he knows about seining and salmon. We were supposed to learn what we needed to know to do our work, and Skipper and Frank would help in this. Anything more was outside the reasonable expectations of the employment relationship. Thus, many questions a "student" would find reasonable just went unanswered unless Skipper or Frank happened to be in a good mood and felt like "schooling." In other words, the Glenda Lou was a working seine boat, not a fieldschool in seining. Such "training" did occur

in infrequent and small doses later, but first a person had to prove himself as a worker and become liked as a personality. For example, a "student" might ask, "Why are we making the seine have a 'bag' in it?", and do so in a non-sassy way. The sort of answers he probably expects are of the kind which would relate the underwater shape of the seine with habits of salmon, depth of waters to be fished in, economizing on costs of rope, and so forth. He hopes rapidly to advance his understanding of seining by receiving a synthesized answer confirming his expectations, and he also hopes to impress the knowledgeable expert with the astuteness of his question. The skipper of his boat, however, may see this as an impertinent question for it presupposes that there are other ways to construct a seine, that one has chosen from amongst these possibilities, and that the student wants to evaluate the skipper's decision. The result may well be an answer something like, "Because I decided to," or just a dreadful silence.

Such was the case on the Glenda Lou. Skipper might answer certain questions of the sort in the above example, but first the "student" will have already demonstrated by being a good worker that the "student" has confidence in Skipper's decisions and/or he will be on friendly enough terms that Skipper has informally agreed to be an "instructor." Either way, to get a meaningful answer, one must have a history of happy interactions with Skipper. One is tempted to see Skipper's kind of response as resulting from what he may see as a threat to his authority. This may be partially true, I do not know, but in a more favorable light, I think it reflects his whole perspective on how one should learn anything

whatsoever. He is a firm believer in the "learn by doing" method rather than through verbal instruction. This is a very satisfying way to learn once you stop hurrying and appreciate the depth to which actual experience impresses itself in you. Instruction via talk does not have the force that this kind of loose apprenticeship musters. In addition to the sort of arguments Polanyi (1968 Chapter 4) gives for the richness of the apprenticeship method over a verbal instructional method, there is a difference in one's whole psychological orientation to learning. It is learning to slow down and enjoy the task of learning rather than being in a hurry to prepare. It was a most refreshing experience to learn things in this way.

Perhaps of most significance for the seining season that followed, the crew got on very well together during this week of close and long contact hanging seine. We all seemed to share similar ideas of what kind of crew we wanted to be and be with. We all wanted to do good work, but we wanted to enjoy the work as well. Speed with quality was a common goal. This was explicitly formulated in two "team" pep talks, but grew more pervasive in each of us through dyadic conversations in which we discovered in each other individually the announced group hope and plan. Laziness and slackness hurt not only the others but also oneself because while fishing only the whole crew brings in the catch. This general subject matter was first brought up by the Skipper the day we all met each other, and it came up several times more before we actually began seining. It was regarded as almost a necessity that everyone agree on this correct attitude if the upcoming season was going

to be a good one. Discussions originated not only from the Skipper, but also Darrell and Richard, and everyone responded well to the ideas. Frank was understandably less "gung-ho" than the four of us deck hands; he already knew what would happen and was more inclined to let the season test us rather than try and prepare us to pass with "A's." But even Frank did his part to inculcate this seining ethos in us.

Besides the very time-consuming job of hanging seine, we also had to get our power skiff and power block running. They had sat idle since the previous season, so we had to check them.

As it turned out, the power block was busted (I did not know what was wrong with it) and the skiff was very difficult to get started. We sent the power block to Seattle to be repaired, but the skiff was fixed by the local NEFCO (New England Fish Co.) mechanic. Arrangements were made to use a company power block until we got ours back. Owing to these rather major mechanical problems and several small details of hardware on the boat, I began to realize that the Glenda was not exactly a "tight" boat. I saw that I would have to be alert to possible equipment failure and avoid being in hazardous spaces while working, but at this time, I did not know exactly where the safety spaces were. I could never have but a minimal "trust" in our equipment. In spite of this, I still was incredibly eager to start seining.

The Fish and Game people were not obliging to my desires, however, and there was a rumor going around that perhaps the season would not open for the inside waters until the first week of August. The pink salmon run was supposedly very poor, and there was an even worse rumor

that there would be no inside waters seine season at all.

With all, or most, of our preparatory work finished and the season still several weeks away, the Skipper decided to go out trolling again for a couple of weeks. Richard and I were both getting bored with town life and poorer every day we stayed, so we asked if we could go along trolling. I also regarded the excursion as a golden opportunity to familiarize myself with the boat and get to know Skipper, Frank, and Annabelle better. They agreed to our coming with them; the deal was that we would do some small chores and pay for our own food.

The trolling trip, which lasted from July 17-25, was very significant for my whole first year's experience. Trolling is very, very different from seining. True, it was the same boat, almost all of the same people, and we were catching the same kind of fish (salmon), but the entire quality of life trolling is much more subdued, relased, mellow. The difference came to be symbolically felt in me in a rather peculiar way. As mentioned above, coffee drinking takes on exaggerated importance on the Glenda, this is true whether trolling, seining, or in port. While trolling, however, Frank uses a large thermal coffee cup with his name lettered on it; seining, he uses a standard restaurant-type, ceramic mug, and lets me use his trolling cup. Now, the ways this expresses the difference between trolling and seining are: while trolling, one has lots of time to do the work, there is no big rush to get anything done, so a large thermal coffee cup is ideal in that it holds lots of coffee and will keep it warm for a long time, but while seining, one does not have time calmly to sip coffee, rather it frequently gets gulped down, and a

large thermal cup would hold too much and keep it too hot. By the end of the trolling trip, having heard a lot about seining, I still did not appreciate the difference. It took exactly one or two sets my first day of seining to figure out why Frank was letting me use his cup and why he had made a point of claiming his ceramic mug.

For the sake of recording even sketchily the chronological development of my seining competence, I should mention some of the things that I learned while trolling.

Perhaps the most important thing I learned was done semi-consciously. I learned the "space" of the boat. Anyone who first walks onboard a seiner is struck by the compression of space and the efficient use thereof. There are very definite "pathways" and routes to and from certain points, and these eventually become very deeply embedded in the muscle and motor patterns of people working on the boat. At first, however, one is unsure what surfaces can be walked on, how close to the gunwale one can stand without danger of falling overboard, how to turn when going down the ladder to the focsle, and so forth. Movements such as gracefully stooping the head to one side while stepping into the galley (to avoid banging head on top of door), doing a couple of sideways shuffle steps rather than simply trying to walk between the galley and the rigging, stepping up to the foredeck as the boat rolls the opposite way from the side you are on, and numerous other subtle modes of handling one's body all reflect that one has gotten "in tune" with the space of the boat. In a different but equally important sense, identifying "busy" and "peaceful" spaces is also part and parcel of getting

harmonized with the boat spatiality. Do not stand in the galley door, do not perch between the power winch and mast; on the other hand, if you want to be alone outside, sit on the stove's fuel tank on top of the cabin or on the hatch covers or up on the bow. Trolling, these busy and not-so-busy spots are hard to recognize, but seining, they become very clear except perhaps to insensitive rookies. By the end of the trolling trip, I had internalized much of the boat's space and my movements onboard had become much more graceful and dance-like. Such harmonization involves more than simply "getting one's sea legs," however, for it concerns getting in tune with a specific spatial layout in addition to getting used to the action of waves.

In terms of purely technical knowledge, I learned where the deck hose switch was, how to turn on the power winch, where the mast light switch was, how much fresh water our tank held and how much we could use for personal washing, how to shut the engine off, how to raise the anchor, how to change gears and accelerate, how to clean and ice salmon, and where the cabin and focsle lights were. I also learned how to tell the differences among kings, cohos, and humpies (kinds of salmon), even if I was not so good at it.

During the trolling trip, I also experienced the singularly most significant happening in terms of my build up of the "seining ethos," and I should like to go into that in some detail at this point.

The event was that Frank and I had a conversation on the stern of the boat as we sat there watching the trolling poles. I asked him how he got into fishing. He, true to his mode of dealing with rookies, just

sat there not answering or saying anything for about thirty or forty seconds, long enough for me to feel that he was not going to respond and that it must be a dumb question. But, then he began to tell me in a nutshell his life story seen in terms of career episodes. In his brief narration, he made the following points: (1) He moved from a cannery on the Portland Canal to Ketchikan when he was seven so he could go to school; (2) When he was sixteen, he got on a seiner for summer work and continued to work on boats for summer work through high school; (3) For the first few years, he "didn't learn anything," he was "just a young kid;" (4) Then he got on a boat with an old skipper and "got serious" about "learning a trade," so he began to learn about fishing; and (5) He worked on a couple of other boats, then about twenty years earlier he began working for Skipper, and he has stayed on the Glenda ever since because "Skipper is so easy-going, not like some who yell at you all the time; he's easy-going."

At the time, I felt this opening up on the part of Frank was very remarkable and could tell even then that it affected me strongly and diffusely. He went on from his quick autobiography to talk about seining, what it used to be like and what it would be like for me. More important than anything he said in words was a certain gleam he got in his eyes as he remembered out loud the old days. I had seen a similar look in Skipper's and even Anabelle's eyes on certain occasions when the conversation had turned to seining. The whole feeling seemed to be that seining was <u>real</u> fishing, and this impressed me deeply. I shall not dwell further on this subject and shall leave it with the generalization that I too came to regard seining as <u>real</u> fishing, something

to feel proud of being able to do. But, this generality does not capture the profundity or the depth and range of affinities that being a seiner has had upon my self-image and self-confidence. It is perhaps yet rippling into nether regions of my psychology. The seining ethos, then, for me, has as principal ingredients the following: high value and satisfaction for and from teamwork, an attitude that relishes efficient competent work, a self-image of ruggedness and toughness, and large helpings of pride deriving from being a modern participant in one of the oldest occupations (fishing on the sea).[6] All these things were locked into Frank's "gleam," and subsequently, I have found similar feelings in most seiners I have dealt with.

We got back from trolling July 25th. At first, I was elated to be in town again. I could walk in a straight line for more than fifty feet, I could hear my music, play pool, and drink booze. But then these things lost their charm, and I was even more eager to go seining.

Rumors were floating around, all gloomy, but finally, the Fish and Game Department announced the first inside waters opening would be August 8th.

August 6th, we made the final preparations. We had to convert the Glenda from trolling to seining gear. This involved cutting plywood and working it so that it covered the "cockpit" in the stern, taking off the power gurdy hydraulic systems, attaching our borrowed power block to the gooseneck on the boom and hooking it up to the hydraulic lines, and

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[6] For some, this ethos merges with and strengthens a macho constellation of feelings, but it need not. Note that some seiners are women.

several little alterations of rigging and ropes.

The next day we bought our groceries, brought our personal gear onto the boat, went to the old cannery and got our seine and power skiff, and left town to travel up to Deer Island, where the opening was. The crew was excited and emotionally high. The sun was out in full force, revealing the beautiful colors of the land, and fish were jumping along the coasts. Each time one of us saw a salmon jumping, he would hell "Aye-oh!". Skipper said this was Haida for "fish (salmon) jumping," but we Haoles could not help noting the strong phonetic similarity with the Lone Ranger's famous prod to his horse Silver.[7] As the number of "Aye-oh's" mounted, we got more excited about the possibilities of making a lot of money the following day. I was also excited that finally the opportunity to find a dissertation topic was coming to pass. It was a very fine time.

By the time we got to our anchorage for the night (just east of Deer Island), the sun was setting. There were many (more than thirty) boats within a couple of miles of us, and I quickly realized that salmon seining is not the sort of fishery where boats scatter out on the sea. In spite of the congestion, there is still room for skippers to make good and not-so-good decisions about where they want to fish in an opening, especially later in the season when the area opened up to seining increases.

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[7]This was even stranger for a coho salmon is often called a "silver." Puns were very popular on my first year's crew, and this one was a favorite. It also helped set us apart as an "Indian boat" from non-Indian boats who announced fish jumps with "Jumper!".

Until this time, there had been no definite assigning of jobs. Skipper had made little comments off and on, but he had not really made anything final or comprehensive. Back when we were hanging seine, I had thought in terms of $Person_i \leftrightarrow Job_i$, and in this mistaken understanding (cf. Chapter 3) had gleaned that I was going to be put on the cork line (whatever that is?), Sk'eg was going to get lead line, Frank was our skiff man, and Richard and Darrell were going to do something else. While loading the seine on deck in route to Deer Island, it had become clear that Sk'eg was doing leads, Richard web, and I corks. Still, I knew that this was not all there was to seining, so I was not surprised when, after checking over our gear set-up, Skipper gathered us together on the deck and gave a very brief partially unintelligible announcement of the crew organization. In close paraphrase, his speech was something like:

> Well, let's see. Darrell, you and Richard will do the pursing up this year. You two (Sk'eg and I) do the other things. Suzie (Sk'eg),[8] you're the skiff man with Frank. Richard, you'll do the web again; John, you do corks; Suzie the leads; and Darrell, you do what you did last year.

This, then, was the only formal job allocation Skipper made to us as a group. From experience my second year when I was deck boss, I can fairly safely say that he and Darrell probably talked about who should do what and that Darrell would operate the inside drum of the power winch rather than Richard. But, it fascinated me at the time and it still does that

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[8]David is Sk'eg's Christian name. He "earned" his Haida name, Sk'eg, by biting the nose off of a dog salmon later in the season, and that is a story in itself. But, until this strange behavior resulted in his Haida name, David was called Suzie to avoid the confusion of having two Davids on the same boat.

this terse, jargony speech was the first and last time that jobs were assigned by authoritative decree. If the reader cannot figure out what seining is like from the Skipper's assignments, then he knows how I felt that night because it did not mean very much to me either. The instructions were largely opaque. I was expecting to have some sort of a lecture on the operation of "making a set" (this is jargon for all activities involved in putting the seine in the water through to getting the fish on deck and the gear set-up again for the next "set"). Instead, I got some sort of semi-coded message and the obvious prophecy that "You'll see tomorrow."

Skipper then took Darrell aside and reviewed with him certain points of the operation that Darrell would be responsible for. Frank took Richard, Sk'eg, and me aside and showed us how to fasten and unfasten "figure-8" clasps, and we practiced doing it. We all checked around the deck making sure that all lines were connected properly, the davit areas had the proper equipment, and so forth. Finally, we detected (it was not Sk'eg nor me) that we were short one line attached to the "bag" end cork line. There should have been a line attached about five fathoms from the end of the cork line so that the cork line could be tied to the bow of the boat at one stage of the "set," but it had been forgotten. Other than this omission, everything seemed to be satisfactory to those who knew what was going to happen, and they rigged up an alternative way of getting the cork line connected to the bow. Last check completed, we all went to sleep. It was about midnight or one o'clock and the northern lights were out.

Four-thirty in the morning of August 8th, the engine cranked up and we all got up from our bunks, dressed, and staggered into the galley for coffee. It was still fairly dark out though past dawn, because there was a low cloud cover and off-and-on drizzly mist. Other boats were already prowling about with their mast lights on, other crews crowded into galleys having coffee, other rookies wondering what was going to happen and if they could learn quickly.

We hoisted the anchor and cruised over to the "hook-off" (spot where the water gets deep rapidly enough so that the skiff can take its end of the seine right up to the shore) Skipper had selected to try first. We were the second boat there "in line," the Louann, a boat friendly with us, first.

My excitement was comparable to an eight year old's Christmas morning, and the others were similar, Frank less so. I was completely happy. I was half lost in relishing a new self-image. There I was dressed to the hilt in my new Norwegian rain gear, Oom Paul pipe stuck in my mouth, and on a fishing boat. Ah, if my Viking ancestors could only have seen me in my glory. I found all sorts of comforting analogies to my anxiousness from events in my past--the moments just before the curtain goes up in a play, standing on the forty-five yard line waiting for the opposing team's kick-off, standing in the hallway waiting for my doctoral oral examination to begin, etc.

Eventually, six o'clock came (the beginning of the opening) and the Louann cruised up close to shore and their skiff cut loose, the seine flying off the deck of the boat as the skiff headed for shore and the

boat headed perpendicularly away from shore. Knowing that we were next at this hook-off, I watched the Louann like a hawk. I saw a friend of mine grab his "plunger" (a long pole with a metal cup on the end) and begin splashing and popping the water between the stern of the boat and the seine. Other people on the Louann were doing things I could not interpret, so I decided that I would start plunging at that point in our set; it would make me look busy and give me time to watch what else was going on.

After about thirty minutes or so, the skiff of the Louann revved and headed back for its boat. Then we cruised up to the hook-off spot (the tide had moved the Louann's seine away). Frank was in the driver's seat of the skiff, Sk'eg pearched in its bow holding the cable that was connecting the skiff to the boat. Darrell and Richard stood by the niggerheads of the power winch ready to release the skiff, and I stood on the hatch. Skipper was at the wheel on top of the cabin all eyes on him. He brought the boat around, looked around hesitantly assessing the decision, and then nodded his head. Darrell shouted, "GO!", Richard simultaneously slacked off the tow line shifting tension in such a way that the pelican release popped open, and the skiff was free. Sk'eg pulled the cable into the bow of the skiff as Frank whirled it around 180° and headed for the hook-off spot. My seining experience was fully underway. What a rush! The seine was flying off the stern, the purse line shooting through the rings on the ring bar, everyone was energized.

As the seine came to its end and stretched the boat tow line out, we wrapped the chain on the double block around it twice and hooked it, then hoisted the tow line up a few feet so that the line would not rub

on the stern as Skipper maneuvered the boat. Then I headed for my plunger and safety. Darrell and Richard began getting the boat ready for the skiff's return. They turned the port davit out (we were setting to port), put two chains on the davit, opened the davit pulleys (purse blocks), moved the purse line boxes over to the area of the starboard davit, made sure the line Frank would use to pull the boat out of the set was easily accessible to him and connected to the deck and bow cleats, and hosed off the deck. (See Figure 3 for the positions of these activities.) Then they came over to me and relieved my tired arms. Richard gave me my one and only "seminar" on plunging, I had been using entirely too much energy. They began to tell me what I should do for the first few minutes after the skiff came back.

Sk'eg would hand Darrell the secondary skiff tow line,[9] and Darrell would run it through the forward davit pulley, put a couple of wraps around the inside drum of the winch, and begin pursing. That left Richard and me to get the boat end of the purse line started. I should stand holding the boat tow line, and after Frank drove the skiff out of the set (after Sk'eg had jumped back on board the boat), Richard would lower the double block. I should then take the double block off the boat tow line and pull it down to the deck so it would not swing freely in the air. Then Richard would untie the boat tow line from the winch niggerheads, and I must very quickly take the line over to the stern

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[9]There is no "name" for this line that I know of. The nomemclature of seining hardware is by no means exhaustive. I have had to invent numerous "names" in order to construct a written exposition. A seiner would know what I mean, but not necessarily use all the same terms.

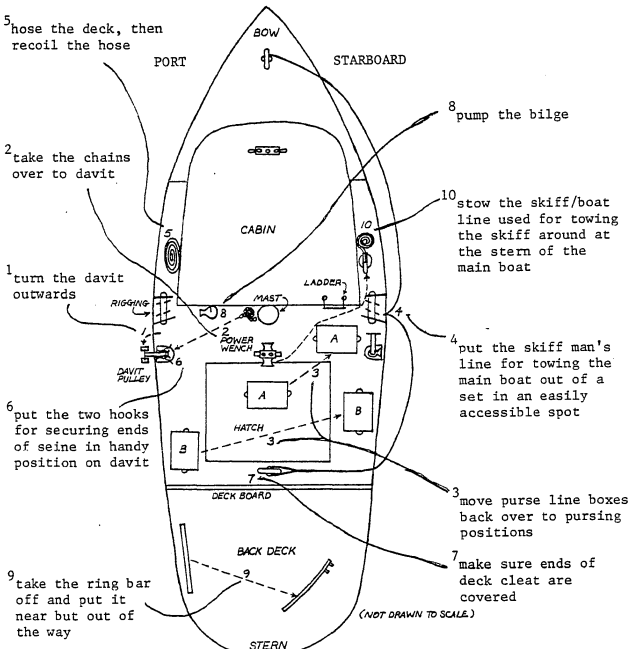

Figure 3. Usual jobs preparing for the skiff's return (set is to port)

davit pulley, put it in, and shut the clasp. This had to be done quickly because once Richard untied it from the niggerheads, only Richard's holding onto its end kept it from floating away; but if I moved fast, there would not be much tension on it. Once through the davit pulley, Richard could begin pursing it up.

Of course, their instructions were very different from my written account of it. They did not bother naming parts of the hardware, nor did they explain why I was going to do what I was. Rather they just told me to do several things in a linguistic form something similar to: "Put that (point) through there (point)." Furthermore, my accounting reflects far too much my current seining competence. Here I have singled out the important motions and movements, both of hardware and personnel, I have reconstructed the temporal relations in too much detail, and I have generally based the description upon my more advanced understanding of functional, spatial, and temporal patterns of seining. As a result of this, the reader now has a more detailed, if less visually concrete, idea of what happens in the first thirty seconds after a skiff returns to its boat than I did that first morning. All I got from their "instructions" was a vague advanced warning that things were going to get very hectic and very busy. I just concentrated on my responsibilities: getting the yellow line above my head (the boat tow line) freed from the double block and through the davit pulley (that one right there →). If I did that, other folks would take care of everything else.

At this point, while we are waiting for Frank to bring the skiff back, I should give a better feeling for how well I understood seining

at that time. I shall attempt this by giving three ways of answering the question, "What do you do on the Glenda Lou?".

(1) Replying to friends, a tourist, someone in my department:

So, you want to know what seining is. Okay.

Well, there's this big net about a quarter-mile long, and a little boat takes one end of it, and we make a big circle with the net in the water. When you close the circle, the little boat gives its end back to the big boat, and the fish are now inside, but still able to get out through the bottom. So, we purse up the bottom of the net, just like drawing the strings of an old fashioned ladies' purse--that's why it's called "purse seining." Once this is done, the fish are trapped in the seine with no way out unless they jump over the top, and that doesn't happen very often. All you've got to do then is haul in the net and drop the fish on board. It's just that simple.

Why do we use the little boat? Because the alternative is to tie an end to a tree or something; the little boat, or skiff, makes you mobile. In the old days, they used to have to row the skiff. That would really be hard work.

Any other questions?

(2) Replying to someone who seemed really interested, or who might want to try seining but of whom I was not especially fond:

Well, to begin with, there are six people who do all the work. The skipper stays on the main boat and runs the show, the skiff man manuevers the power skiff around, and the other guys are deck hands.

When the skipper gives the order to cut loose, the skiff man and one of the deck hands take off in the skiff with one end of the seine. They go out and hold the net open so that the fish swim into it. It's a long net, about a quarter-mile or so. When the skipper thinks it's time, he tells the skiff to bring its end of the seine back to the main boat. Now, the seine is in a big circle with both ends tied to the boat. Two guys start pursing up the bottom of the seine by wrapping a line run through rings attached to the bottom of the seine around the drums of a power winch on the boat. This closes the bottom of the seine so that the fish can't swim out. While they're pursing up, as it's called, two other guys are running around doing assorted things to keep the seine from getting tangled up. The one fellow who went for a ride in the skiff is back by now.

When the pursing is finished, we hoist up the bottom of the seine and drop it on the deck. Now, the fish cannot get out. All that is left to do is haul in the seine until there is just a little bag of fish in the water, then hoist the fish up and drop them on deck. The hauling in part is rather fun because it's super teamwork and you get a kick out of making really good piles. When you hear someone talking about being "cork man" or "lead man" or "web man," or "doing" corks, leads, or web, he's talking about this phase of the work. The skipper

doesn't do this hauling work; he just runs the power block and generally watches for problems.

Got anything specific you'd like to know?

(3) Replying to another seiner, probably a rookie:

Well, we've got a seven man crew, but only six actually work making a set. Our skiff man has worked for the skipper a long time and really knows a lot. Two of the guys have one year's experience and that was on the Glenda. And then there's myself and another rookie.

One guy is a good mechanic and tight with the skipper so he's our deck boss. He generally just watches over things, sort of a trouble-shooter, and then he purses. The other experienced guy is good at web and he purses too. The two of us green hands do the other things. I do corks and plunge and pull corks on the stern. Do you guys pull corks on the stern while pursing? The other rookie pulls up the breast lines and is jump man and piles leads. Oh yeah, when we brail--it should happen more--I get to handle the brailer.

How do you guys do things?

As I stood on the stern of the Glenda plunging and waiting for our skiff to come back, my total understanding of seining was somewhere between #1 and #2, closer to #2. My brother had prepped me with some facsimile of #1 before I started travelling to Alaska, and I had even given some version of it to a couple of tourists in Ketchikan. I was hoping that hauling gear would be fun, but I had heard about the jellyfish and was not sure. Also, example #2 reflects a bit too much segmentation of the overall process into distinct, functional phases. Example #3 was far beyond me that morning; it reflects such a thorough, though a bit wordy, comprehension of seining that most everything is taken for granted and only the key jobs are mentioned. Of course, there are an indefinite number of levels of detail and/or style in talking or thinking about seining, but the three above are representative of the spectrum of short stetches. It took me two or three openings to feel comfortable enough with seining to be able to organize it after the

fashion of #3, and at about 6:50 AM that morning, I was still at the "Well, the little boat brings back its end . . ." level.

Richard was telling me what the supposed function of plunging is and generally being a bit skeptical when we noticed the skiff had begun its trip back to us. He handed me the plunger and walked on over to the area of the power winch, so I kept plunging until the skiff was just a few yards away before hurrying over and sliding the plunger on top of the cabin where we kept it.

My hands were now tight around the yellow line (boat tow line), and I stood there in the middle of the back deck waiting for Frank and Richard. Sk'eg was standing in the bow of the skiff with the line coiled and ready. Darrell reached over, took the line, put it through the davit pulley, and that was about the last I was conscious of him for a while even though he was standing in plain view, for in the meantime, Sk'eg had jumped on deck and was running around in circles doing something and my eyes and attention had turned to Frank. He popped the pelican release of the main skiff tow line and headed out of the set between the stern of our boat and the seine. Richard started lowering the boat tow line with the double block, and my work sequence began. I was far too energized for the (relative to my expectations) speed of this particular job sequence. I was like a track runner trying to sprint the mile or two mile.

Everything was going well, if jerkily, all the way through to the end of shutting the clasp on the stern davit pulley. Then things got very strange very quickly. What were those "other things" I was supposed to be doing?

The area I was standing in became cramped with Skipper's arrival on the deck--I was in his spot. So, with minimum grace, I scrambled under the two purse lines, which were now very tight between the winch drums and the davit pulleys, only to bump into Sk'eg who was standing just on the other side. He had hooked the skiff end of the seine with a short line attached to the davit's frame and pulled the Canadian release so that Darrell could purse up. (See Figure 1 and note that the secondary skiff tow line and the purse line are connected with a figure-8 clasp and that the Canadian release is a way to shift tension from the secondary skiff tow line/cork line to the secondary skiff tow line/purse line.) By the time I bumped into him, he was already struggling with the difficult job of handwalking the cork line up to the bow so that it could be tied in that position with a short rope. This is the job that would normally have been much easier, but the five to ten fathom rope that should have been attached to the cork line had been forgotten. I reached down over the gunwale and began helping him. This took about a minute because already there was a lot of tension pulling the seine out away from the boat, but we made it only to have confusion take over.

About this time, I decided to hell with participant observation, I had all I could handle trying to participate. Skipper began yelling succinct, entirely ambiguous orders like, "Pull up that line!!". Sk'eg and I would both jump towards the general direction of Skipper's pointing (the davit/rigging area) and commence fondling lines until one of us happened to hit upon the correct one. Then we would tug

away at it. Just when we were feeling we must be doing a good thing, Skipper would yell out another equally clear instruction and we would let go our line and jump for the second one. This would bring a rapid verbal evaluation of our overall mental capacities from Skipper. Eventually, without discussing it, Sk'eg and I "decided" to divide and conquer--each to his own line--and Skipper's agitation began to dissipate.

In spite of our ignorance, Sk'eg's and mine, the forward breast line and stern breast line were pulled up and secured in place. We did not know why they had been pulled up and tied, we were just happy that we had finally gotten them done. Then the two of us retired to the stern and jointly pulled many fathoms of the cork line up on deck. That was another hard job with no apparent motivating reason except the order to do it.

I was sweating profusely even though it was about 55°F. I had been fully garbed out in my whole rain outfit since about 5:30 AM, and it does not take much physical exercise to make the inside of a rubber suit into a steam chamber. Already I was beginning to understand, or at least see in a new light, why Popeye's forearms are so large.

Skipper announced "Rings!" and there was more bustle. Sk'eg jumped over to do something on Skipper's orders, and I began throwing the corks back into the water. This finished, I got in other people's way trying to help with whatever they were doing. Somehow, the rings got lifted up with the double block and winch and dropped on deck, the boat end of the seine got started over the power block, the purse line was disconnected from the seine so it would not go over the power block, and we

were ready to begin hauling in.

Frank had told me to pile corks in big loops and not just drop them on deck in little piles. I had watched him do it once, but then I had to watch while doing something else myself so I had not really been able to observe closely. The basic problem of piling the cork line is very simple: get five hundred yards of rope with roughly five inch diameter corks onto your one-third of the back deck in such a way that it will play out smoothly the next set. Intellectually, I thoroughly understood my task. All that is required is coiling a long line. But, the doing of this simple job is considerably more difficult than it may sound.

After just a few minutes, I was in the thick of a daytime nightmare. The line I was trying to coil was continuously attached to the webbing, and it was Richard's job to make a nice, flat web pile and not let the web billow about in the air like a sail, so he was having periodic and unexpected tugs-of-war with me. I was having difficulty keeping my balance because as the cork pile grew I had to stand on the wet round corks. I saw that I was going to have a space problem before getting to the end, that is, there were lots of fathoms yet to go and already I had used too much of my space. And lastly, the jellyfish were flying about in the air and burning like hell on my face. Emotionally and mentally, I was fatigued. The corks seemed to rush and rush at me, and I could hardly keep up with them or think ahead far enough to plan where I was going to put each arm's length. For some reason, a stick in the web, Sk'eg losing his lead line, or something, we stopped for a moment. My

right shoulder was aching, I was roasting and dripping with sweat, my fingers hurt, and I looked over to port (I was piling corks on starboard) expecting to see us near the end of the seine. No such luck. We were only about half way through! I was immediately convinced that this was the hardest work I had ever done, the most unpleasant, and I wished mightily that I had gotten in shape before coming to Alaska. Then we started hauling again.

The only things existent in the world were a muscle fatigue pain in my right shoulder, splotches of fire on my cheek, and that damned cork line that just kept on coming. My whole visual attention was focused on imaginary loops of cork line, the ones I saw with my mind about three feet beyond the last place I had just thrown a real one. Finally, we came to the bunt end of the seine and began stopping to remove gilled fish from the web. I could hear the sound of fish flapping and splashing and thrashing in the water and alongside the boat.

Skipper stopped the power block and decided we should "strap" the bag. Richard, Sk'eg, and I came down from our piles to join Skipper and Darrell on mid deck. The three experienced seiners did the work, Sk'eg and I doing assorted assisting jobs, and the fish landed on deck.

We then went back to our piles and finished getting the seine over the power block. The set was over except for setting up for the next one and pitching the fish into the hold. Relief! Out of my rain gear and into the galley for coffee and breakfast! Oh yes, it is about 7:45 AM and we caught about one hundred or two hundred fish, so in an hour or hour and a half I made about $15.60. Skipper, Frank, Annabelle, Darrell,

and Richard are not very impressed with the catch, but now that it is over, I cannot help thinking that I am making similar wages to plumbers, heavy equipment operators, and beginning doctors. Yes, seining is "easy money." But then I start remembering that I owe for groceries and fuel, taxes and social security, and that all the hours I am on the boat should count in figuring my "hourly wage," and I, too, realize that we have not done so well. But, we will make another set in two turns (about an hour) and maybe we will do better then, or the time after, or another day.

So ended my very first set.

The next set was only slightly more interpretable to me and was mixed with similar episodic panics. As the day wore on, I began to do better though I did not forge much in the way of overall plan or understanding. Three major "phases" seemed a natural segmentation of the whole job: getting the skiff off and plunging, everything from the time the skiff came back to beginning of hauling gear, and hauling gear and set-up. This chopping up was of my job track and extended to the whole operation only insofar as my job was part of that whole. I did not yet think of the whole as an operation; that perspective requires too much distancing of oneself from the work and I was very enmeshed in it.

After six sets our first day, I began to conceptualize my job as a string of "jobs." I was still having trouble remembering which "bead" went before another, but I had dredged up the mode of thinking refined in my old mathematical linguistics course to help me. I also had the rudiments of multi-level organization of my job in that the "natural phases" were higher order constructs than the "jobs" on my string. My

thinking was not so abstract as this description sounds, however, but was rather in non-verbalized-to-self clumpings of sequenced "jobs." Proximity in time was the only organizing principle I was using at that point.

The next day we also made six sets. There was gradual improvement along the directions built up the first day, nothing startlingly different. Some further job allocations had developed--"developed" rather than assigned--and these helped the efficiency of the crew as a whole and kept the Skipper from having to yell at Sk'eg and me. They developed without talking about them, however, and I still find this remarkable. For example, without saying a word about it, I became the person "responsible" for plunging. Both Richard and Darrell rotated with me, but I was the first to start plunging, and when the skiff started back in I took over at it. This became an unspoken but nonetheless firm "rule." Similarly, it just happened that I took responsibility for tying the cork line to the bow (just after we have retrieved the skiff's end of the seine), and Sk'eg took responsibility for pulling up the forward breast line. We did not say a thing to each other about "You do that and I'll do this," it just worked out that way. Under Richard's influence, I also had begun searching for the "functions" or "why's" of certain jobs. In addition, I had already begun to build up certain characteristic feelings associated with different phases of my whole job, such as plunging↔relaxation, frivolity; piling corks↔pressure, intensity, etc. These feelings were active <u>during</u> their respective phases and typified their phases <u>post hoc</u>.

Cruising back to Ketchikan the morning of August 10th, I was confident about the summer being a good one, making some money, and eventually turning into a pretty good cork man. I suggested to Sk'eg that we mentally rehearse our jobs, and we practiced making a set in this way. He too felt confused during a set and could not remember what he was supposed to do next in several situations. The Skipper let it be known that he was fairly pleased with the crew's work and thought we would work out fine. Everyone was high on our "team."

At this time, I should like once again to shift writing perspectives. For the last several pages, I have tried to describe one person's (my) baptism-by-fire entrance into the culture complex of salmon purse seining. Hopefully, this has helped the unexperienced reader appreciate seining by personally identifying with me as I retold the story of my initial experiences. This narrative could be continued, but I think its special value is at an end for this chapter. Appendix A provides more specific information concerning happenings my first seining season should one want to read more specifics. Now I would like to take the reader "with me" in a different style, I want to describe an opening from a more abstracted perspective and at a more general level. We shall imagine that it is midway into the seining season and the crew of our boat is working well together, everyone knows what to do when and gets along with the others.

We are in port waiting for the next announcement of a seine opening by the Fish and Game Department. We have just gotten back from an opening, so we have scattered in the town, each amusing himself with whatever.

Plate 3. One of the floats in Thomas Basin (Ketchikan, Alaska) where the salmon boats moor in between "openings" of the season

The boats in the foreground are small "Western Combination" boats, used primarily as salmon trollers. The seine boats are a slightly larger version, and are down toward the far end of this dock.

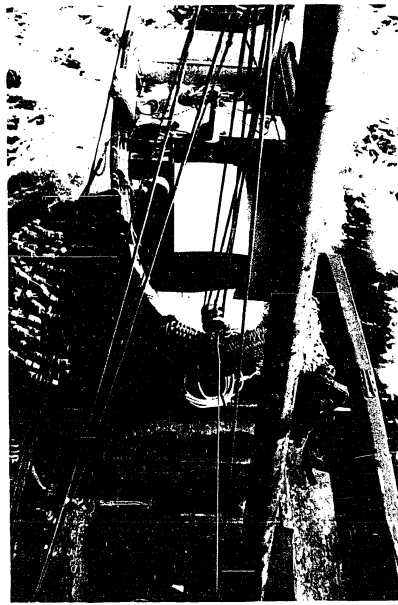

Plate 4. Looking down at the working space of a seine boat from atop its mast

Notice that the skiff is pulled up on the web pile. In addition to making neat piles for aesthetic reasons and so that the seine will play off smoothly the next set, one must be able to haul up the skiff so that the boat can move about at full speed just in case the skipper wants to try seining somewhere else.

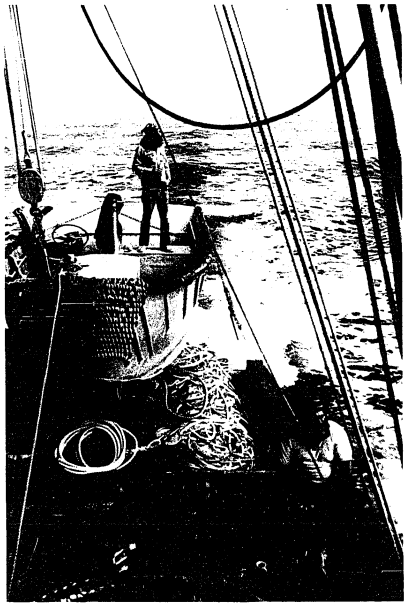

Plate 5. Stern of seine boat as seen from atop its cabin

The two seiners are watching for jumping salmon as the boat cruises around the day before an opening. This is a socially pleasant time because there is no real work to do, everybody is probably tired of being in town and happy to be out fishing again, and there is no polution in the form of jellyfish on the gunwales and deck. In addition, everyone is clean and can feel good about wearing freshly washed work clothes.

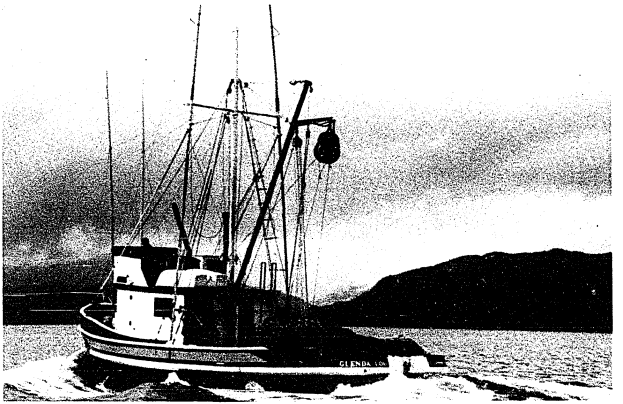

Plate 6. A seine boat preparing to seine

They have lowered their skiff down off the web pile and are about ready to receive it at the stern. Once in the water, the skiff will be tied to the main boat until it is released at the beginning of a set. At that time, the skiff will be tied to the "bunt" end of the seine. Notice the Puretic Power Block suspended on the "gooseneck" of the boom. Also, notice that this particular boat is rigged for salmon trolling as well as for seining (the poles near the bow and parallel to the mast).

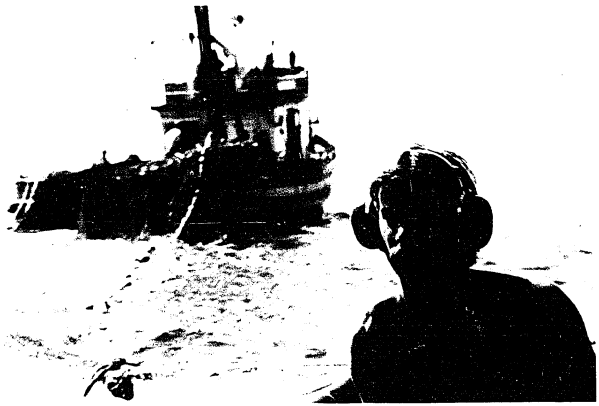

Plate 7. A set just underway

This is about ten seconds after the skiff was released. In that time, the skiff driver has turned the skiff around and begun dragging the seine off the stern of the main boat. The "ear muff" device on the head of the skiff driver is to dampen the noise of the diesel engine. Frequently, skiff men have walkie-talkie headphones, but here they are just noise dampeners. The men on deck are watching to make sure the purse line does not tangle and drag the seine off all at once. Generally, the deck hands watch for any of the assorted problems that could come up at this time.

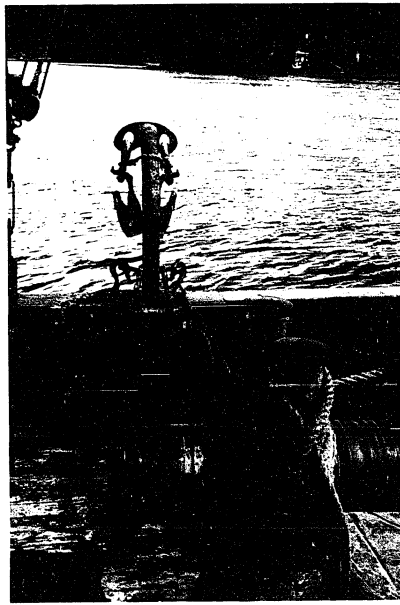

Plate 8. The davit area and the boat tow line tied to the niggerheads on the power winch

While the skiff is near the shore and the set is being held open to catch salmon swimming into it, the deck hands are plunging and preparing for the skiff's return. The davit area is very important. From top to bottom on the davit, the purse blocks (pulleys) are open and ready, the hooks are in position to be fastened to the two ends of the seine, and the chains are handy when it is time to hoist up the rings.

Plate 9. The author "plunging"

In my 1975 job routine, I finally learned not to wear all my rain gear (jellyfish protection) until well into a set. This was what I had to wear at this stage if I were to have sufficient time later to get my coat, wristers, goggles, and hat on without making the rest of the crew wait on me.

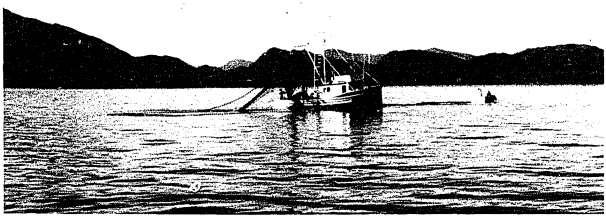

Plate 10. A seine crew "hauling gear"

The set has been to starboard. They have already pursed up the bottom of their seine and hoisted the rings up. Now, they are making their cork, web, and lead piles on the stern. The skiff keeps the main boat out of the seine.

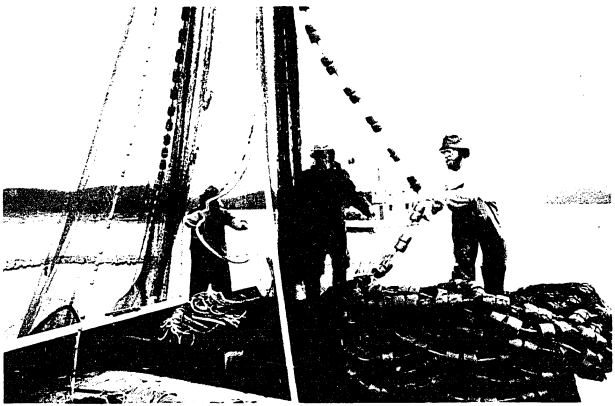

Plate 11. Close-up of a seine crew "hauling gear"

From left to right, they are piling leads, web, and corks. The cork man (author) has finally gotten over an excessive dread of jellyfish and realized that if it is not windy he does not really need to wear a hot rain coat. Jellyfish can be seen coming out of the water with the seine if you look closely.

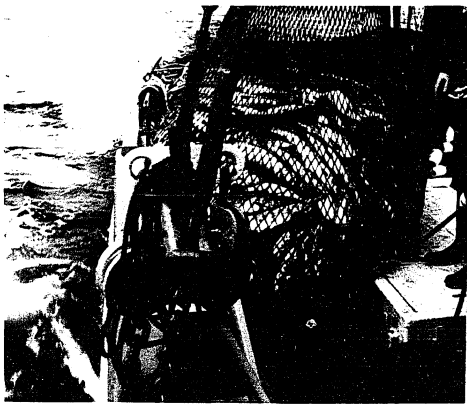

Plate 12. A medium-sized bag of salmon, the result of one "set"

There are approximately three hundred fish here (mainly humpies) worth about $32.70 to each crew member. It is not a bad catch -- no one would grumble about making another set in the same spot -- but in a good year for salmon, the crew would want to do a little better.

Plate 13. What it's all about

There are about four thousand salmon here, the result of one day's work. Each crew member's share of the catch would be roughly $600. The reader can see why the seiner is smiling.

Plate 14. A seiner fondles his nemesis as he helps in the clean up after a set

Plate 15. A seine boat getting ready to unload their "money fish" (non-humpies) at a cold storage plant

Each night during an opening, the boats unload their day's catch of humpies to cannery tender boats and ice their non-humpies. After the opening, the boats have to unload their iced fish at a cold storage plant. This extra effort is worth the time, however, because they get more per pound for iced fish, especially for iced kings, cohos, reds, and dogs (i.e., the non-humpies).

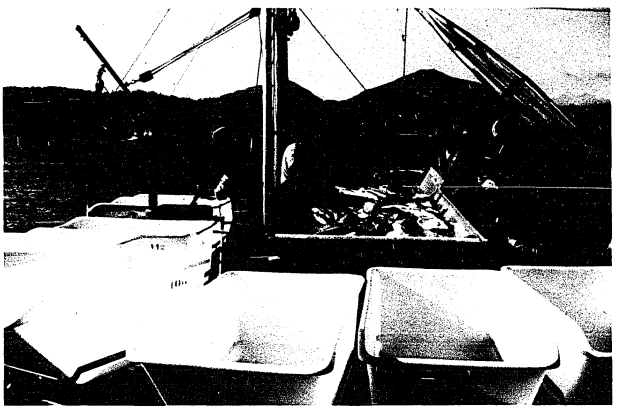

Plate 16. Cold storage "experts" sorting iced salmon by species and maturity (brightness versus darkness)

This would be an excellent field location for an ethnography of sorting procedures. The crew of the seiner whose catch is here being sorted and weighed are down below scrubbing their boat's hold with disinfectant and cleaning the jellyfish pollution from favorite lounging spots on deck. After this, they will run around town and frolic each in his own way until another opening is announced.

Some may have climbed Deer Mountain to get away from people for a while, others are hanging out in bars, still others may have found some romantic interests.

The following day, the crew will rendezvous at the boat to go through the seine and patch holes in the webbing. Maybe the boat needs fuel and water, so everyone will cruise over to the gasoline and diesel float and take care of those preparations. Hopefully, everything that needs to be done to the boat and seine can be completed in one day, then the crew can frolic freely in town until the announcement comes out.

A couple of the crew need money, and so they ask the skipper for a draw. He fills out forms indicating how much money, and they take them to their boat's fish-buying company and get a check for the amount.

A couple of days and hangovers later, the seine opening is announced. These announcements specify the legal fishing areas and times according to type of gear. Let us imagine that it is for three days and has several areas open.

The crew, which has been checking in with the skipper every day, finds out whether they are leaving one or two days before the beginning of the opening. They will have to cruise around looking for jumps as the skipper decides where he wants to fish. In making his decision, the skipper will consider what time in the season it is, his knowledge of the behavior of salmon in the area (where big spawning streams are, how fish get from the outside ocean to the inside waters, etc., etc.), the tides, the predicted weather, where good catches have been made so far this season and when, how many good hook-offs he knows in each area, how

crowded he thinks each area will be, and his total past experiences in each area. Usually, he will have two or three or four spots he would like to check out personally, and these are the ones that will be visited by the boat the day or two days before the opening.

The cook and anyone he/she can recruit will charge the boat's groceries to their cannery account and get them loaded aboard. Each crew member will probably make a last moment run to buy personal goods, mail letters, tell whomever that he is leaving and when to expect him back, etc. Then the boat will leave town. Incidentally, this is a good time to find a job on a boat because some crew members may not show up, for various reasons.

The day or two before an opening are very pleasant for the most part. Usually by this time, the town life has gotten boring or too expensive. Also, most seiners seem to enjoy cruising around on their boats. There is no work to speak of except taking wheel watches (if the skipper lets-makes the crew steer) and watching for fish jumps. During the day, people sit around the galley table playing cribbage, reading, talking, and so forth, or they go outside and soak in the scenery. In the evening, the boat will anchor and if the mood strikes, the crew will fish for halibut, red snapper, and assorted bottom fish with hook and line. During these times, crew members will operate in their "boat personality" and the social world is usually superficial yet intense. Some individuals behave in much the same ways on the boat as they do elsewhere, but some, I think most, change. Typically, "normal" tendencies towards aloofness, coolness, and sophistication seem to dissipate to more open, direct, humor-filled

modes of interacting. Small, completely trivial happenings, mannerisms, sayings, etc. become blown up, magnified, and points of the crew's esprit de corps. "Heavy" conversations, if they happen at all on the boat, would be very incongruous with the ethos dominating the public forum and are restricted to particular contexts in dyadic conversations. "Serious" topics appropriate to the public attention would include such things as where to fish, prices for fish, condition of the seine or some other items of equipment, weather and tides, how to handle the skiff at a particular hook-off, etc. These "serious," not "heavy," conversational topics are likely discussed when the skipper is partaking in the conversation or just present.

Night comes and the crew traipse off to their respective bunks to get as much sleep as they can. The skipper and cook will most likely have the two bunks in the cabin, the rest of the crew sleep in the focsle. This is the last night the crew will enjoy the dubious pleasures of being "clean" and able to feel good about wearing non-work clothes until they get back to town and shower. The boat is relatively safe in terms of jellyfish, so you do not have to remind yourself to wash your hands before relieving yourself.

The next morning (the day of the opening) will begin around 4:30 AM when the engine starts. The roar of the diesel will be a constant companion until late that night. The boat is very likely already near the hook-off the skipper has selected to try first, but the boat has to go on over and get in line, or start the line--according to the informal rules which seem to govern interpretation of which boat is where in line,

an anchored boat is not "in line," even though it may be right in front of the hook-off.

Fishing will begin for different boats at different times, depending on where they are in their respective lines, and continue in rotations as late as each boat wants, usually dark. Choice times to make a set are the tide changes, and choice position is next to the shore. Sometimes in very congested areas, lines form not only for the shoreline set position, but also for "second set out," and (rarely) for "third set out." The skipper is responsible for keeping track of his boat's place in the rotation systems, and it can get pretty confusing when there are ten to fifteen boats within half a mile of each other, each one slowly cruising about trying to decide whether to stay or move on, and other boats newly arriving. In this milieu, the speed with which a crew can make a set can become very important in terms of money. A fast crew can make a set slightly after a slow boat, say in the "second set out" position, beat the slower boat's crew, and thus move ahead of the slow crew in its shoreline turn.[10] This is one of the main reasons speed at making a set is so desirable. It is also desired in totally uncongested situations, because the faster the crew is the more sets they can make, and generally, there is a direct proportion between number of sets and number of fish, though not always. Sometimes it is wiser (post facto) to wait in line for hours at a hook-off for one set than to go to another

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[10]"Half-pursing" is a fairly recent technologically innovative style of seining that generally is faster than the more traditional "full pursing" style. In general, boats rigged for half-pursing can move up in line past full-pursing boats.

area and make one set after another all day, but everyone would agree that speed in making a set is highly desirable.[11]

Skippers (and crews) can get into fairly heated altercations involving breaches in the rotation system. Sometimes the breaches are purposeful and sometimes not. Skippers build reputations for being "tough" or not so tough; a tough skipper would most definitely retaliate for some violation of the system that affected him. This would normally take the form of "corking" the offending boat, which means that the skipper doing the corking would make a set right in front of the offending boat's set, and when done correctly and with balls, the boat that has been "corked" not only will not get any fish that set, but also will have to wait for the other boat to haul in its gear. Other ways to cork a boat are not so blatant and would include making a set in front of some boat that has held its set open for too long (roughly, more than thirty minutes at a crowded hook-off). This action would precipitate a very heated debate--the corking boat arguing that they are right because the other boat violated the etiquette of holding a set, the corked boat screaming about future revenge.

Another way to retaliate for an offense is to drop buckets of sand or rocks into the offending boat's seine. This would only be reasonable, however, if done immediately following the offense, unlike corking which

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[11]The "speed" here referred to is not the simple time from cutting loose the skiff to getting the fish on deck and set up again. The time the set is "held open" should be subtracted from the total time of the set. The "speed" of a crew is measured in terms of how fast they can work from the time the skiff comes back to being ready to go again.

can be saved for the future (hours, days, months, years). A third level of retaliation would be throwing one's garbage into the offending boat's set. And then, of course, the lowest level of force is just to rant and rave at the offending skipper and crew. (It is variable whether and to what extent a boat's crew, as separate from its skipper, gets blamed or does the blaming in these confrontations.)

Of the breaches of the rotation system's rules, taking another boat's turn is the most severe. The offending boat takes quite a risk because once its seine is out in the water it is vulnerable to almost any retaliation. Other offenses do not generally meet with the level of force that taking a turn does. Breaches, however, are not always semi-unintentional encroachments at the fringes of the system's rules. Sometimes, they are purposefully committed to "test" another skipper, thinking that the offended boat will not retaliate and hoping to get a good set. Or, what within the confines of a given here and now may appear to be a blatant, purposeful corking or turn-taking may be a delayed retaliation for some previous offense. By and large, the CB radios are busy as skippers waiting in line "chat" with one another and verbally resolve beforehand most questions of the line's order of rotation. Corkings, especially the classic "cork-for-cork" kind, are rather rare, and when they do happen they get talked about for a long time. But, skippers of boats have to know about all these sorts of things and be prepared to deal with encroachments, and this is in addition to running their operation, deciding where to fish, and doing their own work while making a set. The benefits of fishing near friendly boats should be clear.

During the day, activity on the boat is markedly episodic. In between sets, the crew can take off their rain gear, lounge on the cabin or deck, take photographs, play cards, or whatever. Some boats expect the full crew to assist the skipper in watching for fish, others do not. About the time one gets cooled off and comfortable, it will probably be time to make another set, and so the routine goes.

Weather can make very dramatic differences in a seiner's personal comfort. Few people like to seine in actual rain, but a good thick cloud cover is most desirable. The reason for this curious preference is that "rain" gear is actually "jelly" gear, the rubberized outfit of the Alaskan seiner protects him from the stinging and burning of jellyfish which abound in the waters. Thus, a bright sunny day is terrible weather because the rubber rain gear will still have to be worn as jelly-fish protection, and the heat of the sun beating down on a rubberized suit is stifling. Later, in the early fall, the temperature will be low enough (ca. 40°-45°F) on the average that sunny days can be appreciated, but during the summer, a sunny day is pure misery.

Wind also affects very directly the comfort and ease of seining as a job, and even safety. Although the webbing is largely holes, in strong winds it billows like a sail during the hauling phase, when the seine goes up and over the power block (ca. ten to fifteen feet overhead on the boom). Wind also means waves, which rock the boat. This rocking of the boat is amplified the higher on the mast or boom equipment is attached, so that on a windy day, the double block (attached near the power block and used for several jobs) can become quite dangerous as it

flies around the deck when someone forgets to tie it down or hold onto it. A ten to fifteen knot wind is not too bad to work in, rather it sort of helps one keep cool inside rain gear. But, winds thirty knots and over make the work much, much more difficult, especially for whomever is doing the web pile.

By dusk, unless the fishing is very good, most boats will have stopped making sets and headed for a "tender." A tender is a larger boat or barge owned by some fish-buying company which takes a seiner's catch back to fish-processing plants so that the seiner can stay out in the fishing areas. Each of the fish-processing companies owns several of these tenders, some of which are used in other fisheries during the year but act as tenders during the salmon seining season.

Each night of an opening, the seine boats flock to the various tenders to unload their pink salmon (humpies). The crew of a seiner, when its turn comes, have to jump down into the hold amidst fish, slime, and blood and pitch the fish into large brailers. As they throw the fish into the brailer, the crew counts their catch. Usually each brailer-load will have two hundred or two hundred and fifty salmon. When the brailer gets full (reaches the set number of fish, e.g., 200), then it is hoisted up with the winch, swung over to the deck of the tender, weighed, and dumped. Records are kept on "fish tickets" by a person on the tender, and the skipper of the seiner will check the figures before leaving.[12] When the last brailer has been taken away

[12]These fish tickets are the raw data from which the Fish and Game people build up their statistical records.

and all the fish that the seiner wants to get tender's (cannery) prices for are out of the hold, then the crew scrubs down and cleans out their hold. The fishing boat will find an anchorage for the night, and everybody goes to sleep tired.

Going back to the job of pitching fish to a tender, I want to say a bit more of just what this is like.

If you have had a good day, when you jump into the hold wearing high rubber boots and rain pants, you will be in fish up to you crotch, and you may not be standing on the hull but on fish. Adjustment to the smell and slime will take a few moments, but once you get used to it, it is not so bad. As mentioned, it is most likely dark outside, so the lighting in the hold is poor, provided by a couple of light bulbs and the deck lights on the tender. In this context, you are supposed to pitch only humpies first. This means you will have to diagnose humpies from among dogs, cohos, sockeyes, and kings. There are other boats waiting to unload, and everyone wants to hurry and get finished with this relatively unpleasant job. The lighting is not so good; the fish are all slimy and bloody; and under these "ideal" conditions, you are asked to do an ethnoscience-type task.

If this is July or August, most of the fish in your hold will be humpies. Besides, humpies are the easiest to distinguish because they are the only one of the five kinds of salmon in these waters that have "small scales." Using this single criterion, even a rookie can quickly learn to identify a humpy provided he looks for just this one feature. As soon as he starts trying to identify fish in terms of the terminal

categories king, coho, humpy, dog, or sockeye instead of sticking to humpy versus non-humpy, he will very likely start throwing small kings and cohos into the brailer. But with experienced co-workers and a fellow from the tender watching, most of these errors are caught and remedied.

After all the humpies are pitched, the man from the tender will ask for dogs or cohos or whatever you seem to have the most of still in the hold. This is when sorting becomes difficult and takes a fairly long time to learn. Salmon change appearances toward the end of their life cycles, that is, humpies, dogs, and sockeye--the major species having "runs"--change very dramatically. So, so-called "dark" dogs are easy to identify because they will have purplish wavy streaks along their sides, large teeth, and several other features; very mature sockeyes turn red; and the humpies are already in the tender's hold. The difficult cases are bright dogs versus bright sockeyes versus cohos. A bright dog has a thinner tail than does a coho, and a sockeye looks more streamlined and maybe has a slightly blueish tone around his back and head. A king looks like a coho except for the dark spots on a king's tail and its black mouth. (These differences are the diagnostic traits with which I am personally familiar and can use, but there are other ways to discriminate among species of salmon whose specifics I do not know.)

If the boat has no ice or brine tank, then each day's catch will be pitched to the tender sorted by species, because each species gets a different price per pound. But, if the "money fish" (non-humpies) can be preserved until the end of the opening so that they can be unloaded at a cold storage plant, then the crew will pitch only their humpies to

the tender, because at the end of the opening, the cold storage people will sort species and grades of the non-humpies. Since cold storage prices are always better than cannery (tender) prices, it behooves everyone to take on ice if their boat does not have a brine tank. This is especially so as the seining season progresses because the number of non-humpies increases after August, the height of the humpy run.[13]

As the opening passes, the crew becomes increasingly worn out. Getting up at the crack of dawn and hitting the bunk at midnight means a long work day with too few hours of rejuvenating sleep. The seiners may still be fishing at the spot they began in, or they may have moved considerable distances in hopes of doing better. Each night at a tender they may feel like a "high-liner"[14] or an also-ran as they probe the tender's crew for information about how other boats are doing here and there. Whether catching lots of fish or just a few, each set could be that "big one" that could make the whole season, but usually it matches "reasonable expectations" rather than "wild hopes."

By the end of the opening, everyone will be ready to enjoy town life once more. The cruise back is most enjoyable. The jellyfish stings and burns of the day subside and pass from consciousness. The crew has

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[13] In the last few years, humpies have brought from 28-32¢/lb. Almost all humpies, or pink salmon, go into cans. Dogs, or keta or chum salmon, are generally the next least expensive and sell for roughly 35¢/lb. to canneries and 55¢/lb. to a cold storage. Furthermore, humpies average about 4 lbs. whereas dogs are close to 10 lbs. Hence, the dichotomy (humpy versus money fish) is accentuated if non-humpies are iced.

[14] A "high-liner" is a boat that consistently catches more fish than most all other boats. Some few boats are high-liners year after year.

made some money. People have changed to cleaner work clothes and anticipate how good a shower will feel. There will likely be an intense sense of camaraderie, the sort characterized in Miller and Schlitz beer commercials.

As the town lights appear, the crew will gradually slip back into their normal, town personalities. They plan more concretely what they are going to do that evening--hit the bars, get some sleep, chase the opposite sex, etc.

And so ends another opening. This pattern will continue more or less unaltered until the close of the season when the gear will be put away and the crew disperses for home, school, other jobs, vacations. Several weeks after the season, the crew will receive their "settlements" by mail.

Before ending this chapter with some high level generalities, I want to describe three aspects of seining that stand out as I reflect upon my experiences.

The first concerns the levels of competition felt while making a set. To a large extent, these competitions are a motivating force underlying the whole seining ethos. They are: (1) competing with oneself to do a job better than the last time; (2) trying to keep even or ahead of the rest of the crew (temporally); (3) personally trying to do a specific job better than it is done on other boats; (4) personally trying to impress the skipper and other crew members that you are good at your jobs; (5) the whole crew trying to better its last performance; (6) the

whole crew competing with other crews in total effort; and (7) the whole crew trying to impress the skipper that it is as good as or better than previous crews on the same boat. To me, these are ingredients of being a "good worker." They foster pride in one's work, both personally and as a member of a team. All seven levels of striving are felt in greater or lesser intensities during a set, but overzealous personal striving (especially numbers 2 and 1) can damage the group efficiency and spirit. However, I cannot imagine a strong group spirit without each crew member taking his work personally to some degree. The point is that there is a strong drive for better and better performance, for quality work, measured both by speed and aesthetics. Hauling gear is the job(s) which offers maximal opportunity for quality work and correspondingly is most psychologically intense.

The second aspect is the phenomenon of a "smooth set." It is an experiential phenomenon--one felt by a crew when and after it happens, but very difficult to specify. A fast set is likely to be also a smooth set, but not necessarily. On the other hand, a slower than average set will only very rarely be also a smooth set. Generally, a smooth set is a relatively fast set in which everything went well (as it was supposed to) and everyone managed to stay relatively comfortable. From this description, it is logically possible and expectable that there should be gradations of smoothness, for example, four of the six man crew felt a given set was smooth but two did not. This kind of split-decision does in fact happen, but in the normal course of things, it is remarkable that usually there is a consensus of crew evaluation that either a set was or

was not smooth. This, I do not understand.

The last pre-summary point I want to call attention to is the multiple meanings of "good crew." There are three major meanings in the use of this phrase. The first major meaning derives from focusing on the fact that seining is a way to make a living in our society. In this first sense, a good crew is one which catches a lot of salmon, period. The second meaning derives from focusing on the work efficiency and work quality of a crew. Here a good crew is one which makes sets quickly, does aesthetically pleasing gear piles, can handle problems well (both usual and unusual), and has a relatively high count of smooth sets. The last sense of good crew is based upon congeniality of the personalities. If everyone gets along well and there are only minor, if any, personal conflicts, then such a crew is also a good crew. At the start of a season, a crew hopes that they will be a good crew in all three senses. My first year's crew was good in the sense of two and three, but we did not catch so many fish. My second year, we were not a good crew in any of these senses. My third year's crew was strong in congeniality and basic competency, and we did fairly well in terms of catch, but we were fairly slow at making sets in spite of our skill. A priori, it seems reasonable that if a crew worked well together (the second sense) then it would follow that they would get along well and also catch more fish than a crew that did not work well together. This is to some extent true, but a boat with relatively incompetent deck hands who cannot stand each other personally can catch more fish than a socially harmonious, efficient crew. It all depends where the fish are when the seines are in the water.

2.6 Summary

As a culture complex, salmon purse seining in Southeast Alaska has three noteworthy features. These features are more apparent and appropriate in the context of an "objective" viewing of the complex, rather than the personal vantage point from which I have been writing.

The first feature is that salmon purse seining is only "alive" and actively being practiced for about three months in a whole year. This makes it rather different from everyday life complexes such as the East African Cattle Complex which is outside and around East Africans every morning, day, and evening. In this simple-minded temporal aspect, purse seining more nearly resembles periodic complexes such as harvesting, Naven ceremonies in New Guinea, semester examinations etc.

Another general feature of the complex is the annual change of personnel as numerous rookies learn how to seine afresh and many experienced crew members change boats. Unlike new personnel in the form of babies, new personnel for seining are at least young adults with years and years of living experience, specific pre-existing ideas and modes of interpretation, and well-developed systems of muscular coordination, as well as diverse cultural backgrounds. Thus the human fodder of the complex is non-homogeneous in several respects, yet they will emerge relatively standardized, at least with respect to certain key activities.

And, as is perhaps true of all complexes involving massive and sophisticated technologies, no one person knows enough about all that is involved with the complex to be able to transplant the whole complex to a new location. Old skippers may know incredibly well how to rig and

outfit a boat, how to build the gear, how to run the operation, but may not know about canning fish, making nylon twine, repairing diesel engines, etc., etc. Similarly, other "experts" may thoroughly know certain spheres or portions of the whole, but they could not reproduce the complex de novo.

The first two of these features register also when viewing purse seining from the personal vantage point. The third general feature, however, is peculiarly constructed and is largely an artifact of the "objective" orientation to culture complexes. This orientation embodies several presuppositions which are dealt with in Chapter 4, so I shall leave them until that time. This chapter has been based primarily on the observation that in a sense, everyone who goes seining "knows" the complex, and this knowledge is based on his or her specific experiences and the significance those have for that person.

Using myself as the example, I can attest that the complex of purse seining, as it exists in me after experiencing it for three seasons of actual work on seine boats, is diffuse, rich with connotations, and amorphously unbounded. Only some of that totality has been described in the previous pages for it is too large, too ramified to isolate and analyze properly. In spite of this difficulty whenever I try to apply a logic of inclusion/exclusion to locate its boundaries in me, I feel that there is a coherence to the multitude of aspects, and this volume of coherence, this unity is what I think of as the Purse Seining Complex. The vortex of the nebulous complex stands out sharply in contradistinction to the more nether regions, and this cohesive essence of purse

seining for me is the process of making a set--the actual putting of the net into the water and getting the fish. Because of the privileged standing of this constellation of emotions, activities, and thinking, I shall focus subsequent analyses upon this what-it-is-all-about central operation.

Competing perspectives aside, the general features of salmon purse seining as a culture complex make the fact that it persists from year to year, from decade to decade, quite remarkable. The people who enliven the complex for its three month annual appearance may or may not come back the following year, and if they do, most will have had little to do with seining during the intervening months. Further details specific to this particular culture complex could be adduced, but the more fundamental issue is cultural stability generally. How can any specific clump of human culture (as opposed to <u>all</u> of human culture) be stable through time? In following chapters, answers to this question will be sought, both abstractly and using purse seining specifically as an example.

CHAPTER 3

STANDARD PERSPECTIVES ON "MAKING A SET"

3.1 Introduction

In the previous chapter I tried to paint the landscape, set the stage upon which the phenomenon of salmon purse seining transpires. At the same time I wanted to bring the reader into a more personal relationship with the process of seining, and so I recounted certain highpoints from my own first season.

In this analytics chapter, I shall describe in much greater detail what is involved in the basic task of seining--"making a set." The goal here is again twofold: to describe a certain range of ethnographic information and to frame the basic data in a particular kind of formalism. Although the level of detail is perhaps unusual compared with the standard sort of ethnographic literature and the chosen formalism is not exactly common in anthropological work, the basic mode of thinking about the work of seining is quite usual.

The first section presents the crew organizations from my first two years of fishing. Both of these years, I worked on the Glenda Lou and largely with the same crew composition. The basic mode of presenting the data is in what I call the Toy 1 Series, that is, personnel-to-jobs flow charts. In addition to this mode, I shall discuss some general features of each year's crew in the normal essay format.

The second section presents what I call the Toy 2 Series. It consists of an abstracted diagram of the sequencing of jobs as they should

be performed in the ideal set (one where nothing goes amiss), but without reference to who is responsible for what. Thus, the Toy 2 Series more or less describes the logical ordering of seining in a particular style and therefore is comparable to a competence model in the sense that if a person knew the Toy 2 presented he would "know" how to seine on the Glenda Lou.

Both the Toy 1 and Toy 2 Series are inadequate formalisms in that they fail to account adequately for their data bases. But, as is the case with all simulation exercises, the points where the formalism fails are of at least as much interest as those where they succeed. Therefore, I shall digress occasionally to explore certain seemingly obscure and perhaps trivial fine points.

In the third section, I shall outline the rudiments of an Operations Research type project that could be done using the Toy 1 and Toy 2 Series conjointly. This ancillary project, together with the information compactness of the Series format, was a large part of the motivation to construct the somewhat lengthy flow charts of seining in the first place. I do not intend to carry through with the proposed project at this time, but I think even its outline will suffice to show the potential utility of displaying certain kinds of data after the fashion of the Toy 1 and Toy 2 Series.

The final section is perhaps the most significant in terms of the overall arguments of the dissertation. There I shall examine the nature of the mentality that produces the vision of seining reflected in the previous formalisms. The conclusion is that although such a mode of

understanding is formidable and useful for some problems it does not really capture the empirical essences of the work. It does not really probe how a person who seines does his work.

In general, this chapter's descriptions are from the perspective of an outsider. One could obtain most of the data from detailed observations of a seining crew working and subsequent interviews. Its orientation is to the crew as a whole, without delving into how a crew member subjectively organizes what happens while making a set. In this respect, the Toy 1 and Toy 2 Series are "objective" reports. Oddly, the principal goal of the chapter is to negate itself in the end, and in passing, to provide detailed accounts of the coordinated and intricate basic process of "making a set"--the most significant and focal aspect of the salmon purse seining complex.

As a final prefacing remark, I should perhaps explain why I call my formalisms "toys." In my thinking, any formalism is an aesthetically inspired contrivance which in its creation and its applications gratifies its originator. Hence, it is a "toy," it is fun to play with. This in no way detracts from the power of such toys, however, rather it merely helps to keep one's ego from overinvesting in a creation. As seems to be the case with most everything of human manufacture, there are good, better, and bad "toys." In the dissertation as a whole, there are four such "toys." These first two are not my personal favorites.

3.2 The "Toy 1 Series"

The Toy 1 Series consists of flow charts in which the nodes are "jobs" and the arrows between the nodes represent movement in time.

Hence, Toy 1 Series is a dynamic representation incorporating in its most fundamental conventions the behavior of a system in time. It is a mode of representing who-did-what in the process of making a set, and it shows the temporal ordering of the various activities involved.

A given Toy 1 is very close to actual behavior, but it is by no means identical to it. A given Toy 1 is a "skeletal simulation" (Raser 1969). In principle, a different Toy 1 would have to be constructed for each set that was made on the Glenda Lou each year, for each set is a little different from all others. But representational accuracy at this level of detail, although perhaps required for some studies, is not pertinent here. Thus, I have made three kinds of distortion in abstracting the Toy 1's presented below.

First, I constructed each with the aim of representing the ideal set. That is, each depicts who-did-what in the perhaps non-actualized set in which everything went perfectly, no contingency actions were necessary. For example, a log caught in the seine requires additional "jobs," but these are not included in the diagrams.

Second, each portrays the relations of who-did-what at a point during a season when these relations had not only standardized but had also become relatively stable. The diagrams present the crew organizations that stand out in my mind as being the dominant, typical, stable arrangements characterizing each season, respectively. This is a simplification because crew organization evolves during a season--there are several modifications in the who-did-what relations looking at a season as a whole. For example, in the 1975 season, the crew organization had become

relatively fixed by the beginning of the third opening. From this time onward, although there were subsequent modifications, any change registered subjectively as a change from normalcy.

Last, the Toy 1 Series is a skeletal simulation in that it does not model how to do all the "jobs," rather it merely labels them and thus takes all performance knowledge as axiomatic givens.

Therefore, the specific Toy 1's diagrammed below (Figures 4 and 5) are not based on any particular sets. They are abstractions from all the sets made in their respective seining seasons. For this reason, I refer to Toy 1's as a series. The diagrams presented here are second-order abstractions from the set of all Toy 1's.

Since there are two Toy 1's--one for each year I worked on the Glenda Lou--I shall discuss each separately, for they have slightly different histories and require different auxiliary commentary.

On the diagram, each crew member (skipper included) has a path on which are located his characteristic "jobs." Jobs are designated by a written label.[1] This path (a column)--including both the set of jobs on it and their temporal ordering--constitutes a person's "job routine." The whole diagram--thought of as both the set of job routines and their temporal interrelations--makes-up the "crew organization." Temporality is ordinally represented rather than metrically, the convention being that time moves from top to bottom. As a crude concession to real-time depiction, one may read horizontally across the columns and get a rough

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[1] I shall postpone discussion of how I methodologically identified "jobs" until pp. 159-162 below.

Figure 4. The "Toy 1 Series" representation of 1975 crew organization

TOY 1: 1975

(This season, our cook did no deck work, so her work is not drawn.)

FRANK	SKIPPER	SK'EG	JOHN	DARRELL	RICHARD
					loosens skiff/boat line around drum
	command to release skiff				
					releases skiff by slacking line
turn skiff 180° and head for "hook-off"		catch skiff cable and put in bow of skiff			
			coil skiff/ boat line & stow it	make sure purse line goes through rings okay	
		get walkie-talkie ready			
			make sure cork line plays off stern okay		
	maneuver the boat to put a "hook" in the seine				
					untie double block from mast cleat and wait until the seine is off deck
			walk Canadian release off stern		
					swing double block across deck to John
			fasten double block to boat tow line	help fasten double block to boat tow	

	maneuver the boat to put a "hook" in the seine	cork line plays off stern okay		
				untie double block from mast cleat and wait until the seine is off deck
		walk Canadian release off stern		
				swing double block across deck to John
		fasten double block to boat tow line	help fasten double block to boat tow line	
maneuver skiff as the Skipper directs and as the tide runs; try to keep the bow of the skiff against the shoreline				hoist boat tow line with double block
	watch for fish jumping near shore or ahead of the set & relay any messages from Skipper to Frank	get plunger from top of cabin and begin plunging		
				hose down the deck and throw seaweed overboard
			get the davit area ready for the skiff return (chains, open pulleys, put the hooks in a handy spot)	
			alternate	

		block to boat tow line	help fasten double block to boat tow line	
maneuver skiff as the Skipper directs and as the tide runs; try to keep the bow of the skiff against the shoreline				hoist boat tow line with double block
	watch for fish jumping near shore or ahead of the set & relay any messages from Skipper to Frank	get plunger from top of cabin and begin plunging		
				hose down the deck and throw seaweed overboard
			get the davit area ready for the skiff return (chains, open pulleys, put the hooks in a handy spot)	
		take off ring bar from stern gunwale and get guide line over power block from rigging to stern	alternate plunging with John and Richard	
				alternate plunging with Darrell and John
			clean John's, Richard's, and	

take off ring bar from stern gunwale and get guide line over power block from rigging to stern

alternate plunging with John and Richard

alternate plunging with Darrell and John

clean John's, Richard's, and Darrell's "jelly" goggles

order skiff to return

rev. skiff engine and begin towing seine back to boat

take over plunging from whomever has been doing it

check out everything on deck to make sure it is ready for the skiff return

maneuver boat to meet skiff alongside

untie secondary skiff tow line from the skiff kingpost

take boat out of forward gear and adjust idle to proper

keep skiff

clean John's, Richard's, and Darrell's "jelly" goggles

order skiff to return

rev. skiff engine and begin towing seine back to boat

take over plunging from whomever has been doing it

check out everything on deck to make sure it is ready for the skiff return

maneuver boat to meet skiff alongside

untie secondary skiff tow line from the skiff kingpost

take boat out of forward gear and adjust idle to proper speed for using the power winch

keep skiff alongside boat so Sk'eg can get out

hand Darrell the secondary skiff tow line

put secondary skiff tow line through forward pulley on davit

put plunger away, unfasten double block from boat tow line, and fasten block to deck cleat

lower double block

get skiff out of set between seine and stern of boat

wrap secondary skiff tow line around forward drum of winch and begin

jump aboard boat from the skiff

tie double block line loosely to

		dary skiff tow line from the skiff kingpost				
keep skiff alongside boat so Sk'eg can get out	take boat out of forward gear and adjust idle to proper speed for using the power winch	hand Darrell the secondary skiff tow line		put secondary skiff tow line through forward pulley on davit		
get skiff out of set between seine and stern of boat		jump aboard boat from the skiff	put plunger away, unfasten double block from boat tow line, and fasten block to deck cleat	wrap secondary skiff tow line around forward drum of winch and begin pursing	lower double block tie double block line loosely to mast cleat	
	climb down to the deck from atop the cabin	hook bunt (skiff's) end of seine to boat with the forward davit hook (rope) pop Canadian release on bunt end of seine	put boat tow line through stern pulley on davit and shut the pulley clasp, say "Okay!" to Richard		untie boat tow line from niggerheads and wrap it loosely around stern drum of winch begin pursing	
take skiff to other side of boat and fasten a line to the line			duck under	coil secondary skiff tow line		

keep skiff alongside boat so Sk'eg can get out	gear and adjust idle to proper speed for using the power winch	hand Darrell the secondary skiff tow line		put secondary skiff tow line through forward pulley on davit		
			put plunger away, unfasten double block from boat tow line, and fasten block to deck cleat		lower double block	
get skiff out of set between seine and stern of boat		jump aboard boat from the skiff		wrap secondary skiff tow line around forward drum of winch and begin pursing	tie double block line loosely to mast cleat	
		hook bunt (skiff's) end of seine to boat with the forward davit hook (rope)			untie boat tow line from niggerheads and wrap it loosely around stern drum of winch	
	climb down to the deck from atop the cabin	pop Canadian release on bunt end of seine	put boat tow line through stern pulley on davit and shut the pulley clasp, say "Okay!" to Richard			
take skiff to other side of boat and fasten a line to the line tied to the boat's bow and deck cleats, get the skiff in position to pull boat out of the		pull up the forward breast line	duck under purse lines and get the cork line tied to the bow cleat	coil secondary skiff tow line and put it on hatch	begin pursing	
	hook boat's end of seine to boat with the stern			resume pursing	stop pursing after the seine is close to the davit so that Skipper can hook its end	

		of seine to boat with the forward davit hook (rope)			untie boat tow line from niggerheads and wrap it loosely around stern drum of winch
	climb down to the deck from atop the cabin	pop Canadian release on bunt end of seine	put boat tow line through stern pulley on davit and shut the pulley clasp, say "Okay!" to Richard		
take skiff to other side of boat and fasten a line to the line tied to the boat's bow and deck cleats, get the skiff in position to pull boat out of the seine when the time comes					begin pursing
		pull up the forward breast line	duck under purse lines and get the cork line tied to the bow cleat	coil secondary skiff tow line and put it on hatch	
	hook boat's end of seine to boat with the stern davit hook (rope)			resume pursing	stop pursing after the seine is close to the davit so that Skipper can hook its end
	pop Canadian release on boat's end of seine				resume pursing
	get plunger and plunge in the water beside the davit area				
			check stern breast line and pull it up if its tangled, otherwise pull up several fathoms of cork line on the stern		

pop Canadian release on boat's end of seine				resume pursing
get plunger and plunge in the water beside the davit area		check stern breast line and pull it up if its tangled, otherwise pull up several fathoms of cork line on the stern		
(stand on stern breast line if John has had to pull it up)	help John pull corks on stern			
watch for figure-8's coming up and tell Richard or Darrell when his does	watch for figure-8's coming up and tell Richard or Darrell when his does	watch for figure-8's coming up and tell Richard or Darrell when his does		
			(take a wrap off the drum and purse slowly until figure-8 is through)	
help get figure-8 over drum	pull up stern breast line if it is not already up			(take a warp off the drum and purse slowly until figure-8 is through)
help get figure-8 over drum				

	watch for figure-8's coming up and tell Richard or Darrell when his does	watch for figure-8's coming up and tell Richard or Darrell when his does	watch for figure-8's coming up and tell Richard or Darrell when his does			
				(take a wrap off the drum and purse slowly until figure-8 is through)		
	help get figure-8 over drum	pull up stern breast line if it is not already up			(take a warp off the drum and purse slowly until figure-8 is through)	
	help get figure-8 over drum					
	put plunger on top of cabin					
	watch for rings coming up and tell pursers when they do	watch for rings coming up and tell pursers when they do	watch for rings coming up and tell pursers when they do			
	"Rings!"	"Rings!"	throw corks on stern back into water		tie purse line around niggerheads	
				tie purse line around nigger-heads		
slowly begin to tow boat out of seine but not too fast or hard						
	unhook stern end of seine from boat			put "dog" on winch and turn winch off	put chain on stern lift line	
		get tow lines and purse line ends off hatch				
				put chain on forward lift line		

	rings coming up and tell pursers when they do	rings coming up and tell pursers when they do	rings coming up and tell pursers when they do		
	"Rings!"	"Rings!"	throw corks on stern back into water		tie purse line around niggerheads
				tie purse line around nigger-heads	
slowly begin to tow boat out of seine but not too fast or hard					
	unhook stern end of seine from boat			put "dog" on winch and turn winch off	put chain on stern lift line
		get tow lines and purse line ends off hatch		put chain on forward lift line	
		move the purse boxes, and help keep lines clear of where the rings will come down on deck		pass double block to Richard	hook chains with double block
				untie purse lines from niggerheads and take them off the drums of the winch	
			gather stern end of seine together and take it to stern area, tie guide line over power block around the end	hoist the rings a little by hand, then give Skipper the line	
	hoist the rings out of the water				

untie purse lines from niggerheads and take them off the drums of the winch

gather stern end of seine together and take it to stern area, tie guide line over power block around the end

hoist the rings a little by hand, then give Skipper the line

hoist the rings out of the water with the winch

push ring bridal lines away from gunwale and straighten them

help Richard straighten bridal lines

lower the rings onto the deck

take the chains off the lift line and hand Skipper the double block, put chains back on cabin wall, turn davit in

fasten double block to mast cleat

swing rings and bridal lines across hatch

swing rings and bridal lines across hatch

take guide line over power block from John

put ring bar back on stern gunwale

pull lift line through rings and disconnect figure-8

straighten stern breast line along deckboard

engage power block and get its control,

onto the deck

off the lift line and hand Skipper the double block, put chains back on cabin wall, turn davit in

fasten double block to mast cleat

swing rings and bridal lines across hatch

swing rings and bridal lines across hatch

take guide line over power block from John

put ring bar back on stern gunwale

straighten stern breast line along deckboard

pull lift line through rings and disconnect figure-8

engage power block and get its control, hand Darrell the guide line

get seine started over power block by pulling the guide line

start towing boat out of seine (slowly)

catch the end of seine coming over power block and bring it to stern area

untie guide line and tie it to rigging pin

put boat tow line in purse line box

	hand Darrell the guide line			get seine started over power block by pulling the guide line	
start towing boat out of seine (slowly)			catch the end of seine coming over power block and bring it to stern area	untie guide line and tie it to rigging pin	
				put boat tow line in purse line box	
	operate the power block controls			tie purse line still through rings on deck to niggerheads	
		haul seine (leads)	haul seine (corks)		haul seine (web)
watch Skipper for any special instructions					
				look for any trouble that may be developing and try to take care of it before it makes us stop hauling gear (e.g., get gilled fish out of webbing)	

				put boat tow line in purse line box	
	operate the power block controls			tie purse line still through rings on deck to niggerheads	
		haul seine (leads)	haul seine (corks)		haul seine (web)
watch Skipper for any special instructions					
				look for any trouble that may be developing and try to take care of it before it makes us stop hauling gear (e.g., get gilled fish out of webbing)	
				untie cork line from bow cleat	
	untie breast line from rigging and hold it tight				
				get strapping line and double block	
		straighten bunt webbing	straighten bunt webbing		straighten bunt webbing
				take breast line and tie it to nigger-heads	

				untie cork line from bow cleat	
	untie breast line from rigging and hold it tight				
				get strapping line and double block	
		straighten bunt webbing	straighten bunt webbing		straighten bunt webbing
				take breast line and tie it to nigger-heads	
				strap the bag of fish	
			hook the strap with double block		
	hoist bag of fish out of water and drop it on deck	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch
bring skiff back to boat and unfasten the line connecting skiff and boat, then bring the skiff around to the back of the boat and wait for the crew on deck to finish				unstrap the bag and straighten the breast line	
		finish hauling seine	finish hauling seine	tie guide line around end of seine	finish hauling seine
					arrange Canadian release (bunt end)

			hook the strap with double block	bag of fish	
	hoist bag of fish out of water and drop it on deck	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch
bring skiff back to boat and unfasten the line connecting skiff and boat, then bring the skiff around to the back of the boat and wait for the crew on deck to finish				unstrap the bag and straighten the breast line	
		finish hauling seine	finish hauling seine	tie guide line around end of seine	finish hauling seine
					arrange Canadian release (bunt end) and skiff tow lines on port side of stern
			get rope on cork line ready so that it can be used to fasten cork line to bow cleat next set		
				get skiff/ boat line and hand it to Sk'eg	
	get back on top of cabin and put boat in gear	connect skiff cable to skiff/boat line with pelican release		put "dog" on winch	get purse line threaded through rings on ring bar, connect purse line sections with figure-8,
				pull skiff	

and unfasten the line connecting skiff and boat, then bring the skiff around to the back of the boat and wait for the crew on deck to finish				unstrap the bag and straighten the breast line	
		finish hauling seine	finish hauling seine	tie guide line around end of seine	finish hauling seine
					arrange Canadian release (bunt end) and skiff tow lines on port side of stern
			get rope on cork line ready so that it can be used to fasten cork line to bow cleat next set		
				get skiff/ boat line and hand it to Sk'eg	
	get back on top of cabin and put boat in gear	connect skiff cable to skiff/boat line with pelican release		put "dog" on winch	get purse line threaded through rings on ring bar, connect purse line sections with figure-8, and get the bunt end (skiff) of seine set up
				pull skiff up snug to stern of boat with winch	
connect main skiff tow line to pelican release on kingpost of skiff			help move purse boxes to proper places	turn off winch	

				get skiff/ boat line and hand it to Sk'eg	
	get back on top of cabin and put boat in gear	connect skiff cable to skiff/boat line with pelican release		put "dog" on winch	get purse line threaded through rings on ring bar, connect purse line sections with figure-8, and get the bunt end (skiff) of seine set up
				pull skiff up snug to stern of boat with winch	
connect main skiff tow line to pelican release on kingpost of skiff			help move purse boxes to proper places	turn off winch	
tie secondary skiff tow line around skiff kingpost		open hatch and pitch fish into hold, clean off deck	open hatch and pitch fish into hold, clean off deck	open hatch and pitch fish into hold, clean off deck	open hatch and pitch fish into hold, clean off deck
	maneuver boat back into the line at the "hook-off"				

picture of what "jobs" were being done contemporaneously. This is not completely reliable, however, because some very quickly-done "jobs" take quite a bit of space to describe in English whereas some rather lengthy "jobs" have been labelled in seining jargon. For example, "pursing" is a long-term "job" (about eight to ten minutes), and "take boat tow line to davit and put it in stern davit pulley" takes only about six seconds.

With the above instructions for interpretation in mind, the reader may wish to study Figure 4 for a few minutes and see how incredibly sequenced salmon purse seining is. Quite to the contrary of our modern assembly lines where the product moves along to different workers all doing highly repetitive tasks and is thereby sequentially transformed, in seining, it is the workers who "move" through their jobs in the overall operation of entrapping salmon. This is perhaps the most rewarding aspect of seining from a psychological viewpoint. It has certainly poisoned me from further work in factories as they are usually organized today.

Figure 4 had a rather interesting process of development. It was born about three months after I returned to graduate school from my rookie year fishing and was pieced together from data on taped interviews with Darrell, Richard, Sk'eg, and myself. Even by the end of my first season, I did not really know who-did-what, or for that matter what-was-getting-done, during the whole of a set. I had rough ideas of what everyone was doing at any given moment in my job routine, but I had large gaps in my awareness. These gaps coincided with those time regions in a set when I was working alone on "important" jobs. Only very

late in the season did I become adroit enough to perform some of these and intentionally observe others' work, but even then my "observations" were momentary glances. On the other hand, after only three of our six openings in 1975, I knew exactly where everyone would (should) be during any given time range. In December as I listened to the tapes of the other deck hands verbally recounting their job routines, I was amazed at both how accurate and how ignorant I had been in my impressions of what they did. I was never in error, in the sense of distortion by commission, but in some especially rushed phases of my routine I was rather ignorant of exactly how they accomplished what I knew was their end result. For example, I knew (during the season) that Darrell "put the dog on the winch" and "took the dog off the winch" at certain points, because it made a clattering sound when "on," but I did not have much of an idea why he did it or what the job context was for this--what else he was doing in that time range. As I listened to his very detailed description of his job routine, however, I suddenly realized what he had been doing here and how it related (in various ways, but especially temporally) to my own job routine. Figure 4 thus represents a truly composite description.

The fact that I did not know the information presented in Figure 4 during the 1975 season is indicative of one of the more bewildering phenomena of seining--no one need know the totality of who-does-what relations for the system to function efficiently. The four of us who made tapes (I made mine before asking the other to do so) described our work in varying degrees of detail, with differential knowledge of what

others were doing at any given time, and with differing degrees of so-called comprehension of what seining is all about. Each of us strongly felt that it was necessary only for each crew member to do and know his own job routine and a set could be made. The only recognized advantage to knowing how one's jobs "fit into" the postulated overall scheme of things was that emergencies, unusual happenings, etc. could then be handled faster and with less effort expended. Other than this, "knowing how to seine" was merely icing on the cake, it was desirable but not really essential. A certain amount of curiousity was expected and appreciated by the Skipper and Frank (very experienced seiners, both), but a rookie's quest for knowledge was supposed to be contained to only occasional questions and primarily channelled into "think it out for yourself."

From the data on the taped interviews, I obtained a wealth of knowledge that might have taken several seasons of personal experience to get otherwise. Everyone taped found the question, "What do you do when we make a set?", very natural, and with no exceptions, they really enjoyed recounting their normal job routine. The only methodological difficulty was getting them and myself to describe our respective job routines at comparable levels of detail. For example, Richard and I gave our job routine at about a middle-range level of specificity, whereas Darrell was very detailed, and Sk'eg was very general. I tried to guide them at the outset by asking them to imagine they were telling their jobs to an experienced seiner like ourselves but from some other boat and to imagine that he really wanted to know exactly what they did. It took

from fifteen to thirty minutes for a person to verbally recount his job routine--roughly the same time it takes to actually do the work (if the time spent holding the seine open is not included in this comparison).[2]

I must add that Figure 4 has more information represented than is recorded on the interview tapes. In other words, the tapes served as a ground from which I built up the composite. This was necessary to obtain more comparability in detail among the crews' job routines. The disadvantage of this "fudging" is that Figure 4, as is, is no longer an accurate depiction of each narration. This mode of constructing Toy 1 is in line with the overall objective of describing the crew organization to someone who was not present, but it loses much significance as data for subjective or cognitive study.

Turning from the artifact of Figure 4 itself, I should now like to provide some additional data regarding my 1975 crew. I do not intend to write about the temperaments and personalities of my friends and co-workers in gory detail, but some very brief sketches seem in order if one is to appreciate how the crew worked together and the boat's ethos.

The Skipper was in his middle sixties, Haida, a life-time resident of the area, and had roughly fifty or so years of fishing experience.

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[2]This is an interesting bit of data in itself, and I shall have a bit more to say about it later in Appendix B. Also, it perhaps should be mentioned here that each person interviewed seemed to gradually retreat from social interactive behavior with the interviewer (me) as their story progressed. It seemed as if they were moving in a visual world in their memory. I say this because I underwent this feeling when I made my tape, and others' hand, arm, and eye actions supported the idea that they were mentally "doing" a set as they told me about it.

His grandfather designed the Glenda Lou, and he and his father built it. Skipper is well respected around town and by other local fishermen. I say "respected" because whether people think he is a "good fisherman," just average, or below average, his personal reputation is high, especially among the Indians. From my point of view, he was very knowledgeable about seining, his style was old fashioned or traditional, but most significantly, he was very "easy going." He was soft-spoken, polite, and, if this does not sound too strange, seemed to have a contented life as the constant and immediate goal of his actions and thoughts. He enjoyed the present.

Annabelle was the Skipper's wife and the cook on the boat. She is Haida, about sixty-three years old, and a life-time resident of the area. She has one adult son from her first marriage. She, like Skipper, is soft-spoken and attached to Haida traditions. She married Skipper about five or eight years ago after he divorced his first wife. Since then, she has worked on the boat. She pushed a lot of Indian ways--linquistic phrases, stories, comparisons--into the crew's thinking and usually set Skipper up as the authority on such matters. She did no deck work, but was a strong presence on the boat.

Frank is about fifty-two years old, Tlingkit/Tsimsian, a life-time resident of the Ketchikan area, and has about thirty-five years of fishing experience (twenty or so with Skipper on the Glenda). He was quiet, knowledgeable, occasionally surly, and unmarried. If he liked someone, he was very likeable himself, but he had little or no patience with those whom he disliked, and such is his reputation. I was lucky in that he liked me.

These three formed the core of the crew in the sense that they "belonged" on the boat and set the overall tone. Skipper and Frank were the repositories of experience and knowledge. The three of them troll for salmon in late spring and early summer before the inside waters of Southeast Alaska open to seining. Frank does not live on the boat when in port, whereas Skipper and Annabelle do. In recent years, the "crew" on the Glenda Lou consists of this triumvirate plus a variable assortment of young "Haoles" and Indians. In 1975, the crew was four young Haoles, three from Minnesota and myself.

Darrell was twenty-eight and had worked on the Glenda in 1974. He was a good mechanic, very energetic, and playful. He was also our "deck boss." Richard was about twenty-four and had also worked on the Glenda in 1974. He and Darrell were our "experienced" deck hands, each having one year of experience and that on the Glenda. Sk'eg was twenty-nine and rather into Youth Culture (freaks, drugs, long hair, scams, etc.). All three had come up to Alaska from Minnesota for adventurous seasonal work. Darrell and Sk'eg had travelled up together and were open in terms of their "career" work. Richard had plans to go to school and become a professional photographer. All had at least one year of college. All were excellent workers in the sense of non-laziness and cooperative attitudes.

I was twenty-six years old, over-educated, the "big guy," and a rookie. My "in" on the boat was that my brother had worked on it nine years previous (1966 season) and he had gotten on very well with that crew. My posture on the boat, using the others' comments, was one of a

good natured hard worker. I did not know very much about practical skills such as mechanical work, painting, or hydraulics, but I was pretty good with twine and rope.

With these characterizations in mind, it is hopefully clear that the issues surrounding who-does-what while seining are by no means determined by personality or general ancillary skills. Considering everything related to working on a seine boat, we had different strengths and weaknesses, but these were of no consequence while making a set. The four of us young Haole deck hands were tweedle-dees and tweedle-dums with respect to the jobs involved making a set, the only significant difference being seining experience. We were all reasonably coordinated, agile, and spatially aware, and what differences in physical strength may have existed were not important, for all of us were capable of doing any of the normal tasks. How then did the crew organization depicted in Figure 4 come to be?

The first thing most people seem to think of is that the Skipper, the person in charge, the boss, must have assigned the jobs and created the existing division of labor. This is indeed what happened, but as was discussed in Chapter 2, his assignments were minimal, especially so when compared to Figure 4. It is also the case that he oversaw the operation, telling us when certain jobs needed to be done and generally trying to coordinate our efforts, at least the first couple of openings. But the fact remains that his actual job assignments were but a withered skeleton of what came to pass. The full crew organization thus remains a mystery.

From working on the Glenda two seasons and having been deck boss the second, I can understand why Skipper made the assignments he did. At the time, however, I could only make anthropologically informed guesses--there were mappings going on amongst variables such as seniority on the boat, status in the authority hierarchy, spatial location of major hardware controls, etc. His reasoning, reconstructed by me two years later, probably went something like: (1) Darrell knows most about the whole operation so he should purse on the forward (or inside) drum of the winch; (2) Richard has experience so he will purse with Darrell; (3) With respect to hauling gear, Richard was pretty good at web last year so he should stay with it, John is taller than Sk'eg so corks should be easier for him (longer arms), and since I want Darrell to get gilled fish out of the web and do the other jobs associated with that position on the deck, Sk'eg will do leads; and (4) Sk'eg will be "jump man" in the skiff because Darrell has got to stay on deck, Frank does not particularly like Richard, and Sk'eg is more agile than John.[3]

In the foregoing sketch, the significant discriminations were seniority on the boat, experience at a particular job, estimations of total understanding of seining, social compatibility, and two aspects of physical make-up. The mappings between constellations of these variables and specific jobs may vary from boat to boat. For example, on the Glenda Lou, the deck boss will operate the inside drum of the power winch while

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[3]This hypothetical chain of reasoning is grounded from information I gleaned post facto later in the 1975 season, but mainly from rather lengthy discussions during the 1976 season, when I was deck boss, concerning who would be good at what and who should do what (the two are not the same).

pursing, and all things being normal (each crew member is reasonably acceptable), will also not pile gear while hauling, although there are exceptions to this last point. On the Jean D, the deck boss does not purse at all but is responsible for pulling up the forward breast line, hooking the skiff end of the seine to the davit, and announcing when the figure-8 clasps and rings come up from the water. He will pile gear--which is not determined--because the cook runs the power block and gets gilled fish, etc. The point is that although most skippers appear to use similar considerations in assigning jobs, the relational mapping between "qualifications" and jobs may differ according to the traditions on the boat, the number of working-on-deck crew, and whether or not the crew is the sort that develops standardized job routines or switches jobs around from set to set. Suffice it to say that patterns of job assignment are different owing both to hardware and seining styles and to different notions of what the "status" jobs are. Nonetheless, the Glenda is as good a case as any other, and I was able to go to a different boat and work within its peculiarities and yet feel quite confortable and familiar.

Basically, the Skipper's abbreviated job assignments were sufficient to prevent total chaos the first few sets until we had the chance to work things out amongst ourselves. This was all he was concerned about. Furthermore, his instructions were sufficient to block out most of the operation, if we had all been experienced seiners who had worked on the Glenda before and had already constructed in ourselves a general understanding of seining. But here, "sufficient" would mean that each one of

us would supply vast amounts of our own knowledge to fill in the gaping holes. In 1975, at least two of us on the boat were aghast, intimidated, or freaked by the lack of detailed "job descriptions," I refer to myself and Sk'eg.

Irrespective of how each of us reacted or understood Skipper's allocation of jobs, by the end of the first opening (total of twelve sets), almost all represented in Figure 4 had developed and stabilized. All the holes or discrepencies between Skipper's division of labor and Figure 4 had been remedied and filled in. Most of this fleshing out was done without discussion, although a few job responsibilities were talked about and decided upon consciously. To me, a rookie, the major task was not so much dividing the labor. The more immediate problems were learning exactly what high level glossing phrases like "other things," "pursing," etc. meant and how to do them. By the end of the third opening (forty-two sets), everything in Figure 4 had become fixed and each crew member had become quite adroit in their respective routines. The only modifications in the crew organization after that time were the very small idea of using the ring bar to hold down a line over the power block at a certain point in the set so that it would not swing around in the wind and a switch to a slightly different style of dealing with the purse lines while hauling gear.

Before shifting from my 1975 season to 1976, I should like to make a couple of general comparative remarks concerning my first crew and other crews on other boats.

As mentioned above, we were very much the sort of crew that had incredibly fixed job routines. They developed quickly (I was amazed at the time and still am) and then became incredibly unalterable. We did not rotate jobs as many crews do; we did not exchange whole routines for a set; we had practically no deviations except those caused by varying tidal conditions, weather, drift in the water, etc. This rigidity persisted through the four openings of the summer (humpy) season. But when fall fishing (dog season) began, we changed our style of seining slightly by the Skipper's decision.[4] The first couple of sets the new way were a bit shaky, but then the same process of routinization took over and the alterations were ingested into the various job routines with the same fixedness as had happened before.

Another perhaps hidden quality of my 1975 crew was that we were really good. After the second opening or so, or maybe mid-way through it, we had all learned our routines fairly well and were generating lots of internal pressure to speed up our whole operation. By the third opening, we were running the power block at maximum speed while hauling gear, our total time to retrieve the seine and fish being about thirty minutes to forty depending on contingencies beyond our control. By the end of the season (six openings, for me, total of seventy-four sets), we could purse up in about eight minutes and haul gear in about fifteen. While this is not by any means the best in the fleet (the fastest boat

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[4]We had been putting all the rings on a ring bar, but then switched to running the purse line over the power block along with the seine, so the ring bar was no longer needed. (This is obviously a technical point irrelevant for the general reader.) See Figure 6 (Toy 2) for details.

can retrieve gear in about twenty minutes total), it is much better than average and as good as several "high-liners."[5] Furthermore, we all got along very well and we four Haoles shared an apartment in town. In spite of our relatively poor catch for the year, we all knew that we were a good crew in the other ways that is possible, and Skipper and Frank felt we were perhaps among the best they had worked with (more so Frank than Skipper, but both were thinking mainly of pleasant social life).

The last point regards the whole notion of "job routines." At the beginning of the season, even before we went out for an opening, Sk'eg and I had become acquainted with and indoctrinated into the boat's seining ethos. As discussed in the previous chapter, teamwork is a strong aspect of this complex and pervasive attitude. Given the tenacity with which we remained protectively enveloped each in our own job routine, I find it most fascinating that one of the early ways to express the proper attitude for the crew of the Glenda was, "We don't have 'jobs' on this boat; if you see something that needs to be done, do it." What is even stranger is that in spite of the fact that I have just written at some length about how much we were all locked into routine job tracks, I would at the same time steadfastly maintain that all of us fully subscribed to the quote above and did in fact behave according to it. Psychologically, we were deeply attached to our respective "normal" (both ideal and statistical sense) jobs, but at a moment's notice each of us could easily break our routine and handle some emergency, or less

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[5] If the reader notices twinges of pride here, he is correct.

spectacularly, just "cover" for someone who was behind or having trouble. This 'don't you dare hide behind a job description argument' attitude was also quite active in between sets or in port. The intent of the maxim above was to counter any traces of a factory-fostered orientation to work, but the phrasing is completely at odds with what actually stood out about our crew organization.

Figure 5 represents the crew organization on the Glenda Lou in 1976. It should be read in the same ways as Figure 4--"jobs," "job routines," ordinal-time, etc. The reader may wish to take a few minutes to study the figure and compare it with Figure 4. The level of specificity was intended to match that of Figure 4, but I have not attempted to make them exactly comparable, even so far as making each contain the same "jobs."

Figure 5 was constructed in a manner different from 4. Instead of audio-taping interviews with each deck hand and building up a composite description in that way, I simply described the whole operation into a tape recorder myself and transcribed that into proper Toy 1 Series form. My ability to do this marked a tremendous progress in my own seining competence and awareness. My first season I could not have done it, but after just six sets (the first opening, fifteen hours) the second season, I knew what everyone did and when, with only a few omissions (on the order of four or six small tasks). There are at least three major factors underlying this increased ability on my part: I had already constructed Figures 4 and 6 the previous winter and had benefitted from that effort, I was more able to do my work without having to focus full

Figure 5. The "Toy 1 Series" representation of 1976 crew organization

TOY 1: 1976

(This season, our cook did no deck work, so her work is not drawn.)

FRANK	SKIPPER	SK'EG	JOHN	KID	LOUIE
			loosen skiff/ boat line around drum		
	command to release skiff				
			release skiff by slacking line		
turn skiff 180° and head for "hook-off"		catch skiff cable and put in bow of skiff			
			make sure web does not snag and seine goes off okay	coil skiff/ boat line & stow it	
		get walkie-talkie ready			
	maneuver the boat to put a "hook" in the seine				
				untie double block from mast cleat and wait until the seine is off deck	
					walk Canadian release off stern
			pull deck hose switch in	swing double block across deck to Louie	
			pull deck hose switch in cabin and turn on winch		fasten double block to boat tow line
maneuver skiff as the Skipper directs and				hoist boat tow line with double block	
			get davit area		

maneuver the boat to put a "hook" in the seine

untie double block from mast cleat and wait until the seine is off deck

walk Canadian release off stern

pull deck hose switch in

swing double block across deck to Louie

pull deck hose switch in cabin and turn on winch

fasten double block to boat tow line

maneuver skiff as the Skipper directs and as the tide runs; try to keep the bow of the skiff against the shore

hoist boat tow line with double block

get davit area ready for the skiff return (chains, open pulleys, put the hooks in a handy spot)

watch for fish jumping near shore or ahead of the set & relay any messages from Skipper to Frank

get plunger from top of cabin and begin plunging

hose off the deck and throw seaweed overboard & then shut it off (switch & winch)

go watch for jumps with Skipper

relieve Louie from plunging

clean John's, Louie's, and Kid's "jelly" goggles

order skiff to return

rev. skiff engine and begin towing

take over

the skipper directs and as the tide runs; try to keep the bow of the skiff against the shore		watch for fish jumping near shore or ahead of the set & relay any messages from Skipper to Frank	get davit area ready for the skiff return (chains, open pulleys, put the hooks in a handy spot)	line with double block	
				hose off the deck and throw seaweed over-board & then shut it off (switch & winch)	get plunger from top of cabin and begin plunging
			go watch for jumps with Skipper	relieve Louie from plunging	
			clean John's, Louie's, and Kid's "jelly" goggles		
rev. skiff engine and begin towing seine back to boat	order skiff to return				
			check out every-thing on deck to make sure it is ready for the skiff return		take over plunging from whomever has been doing it
	maneuver boat to meet skiff alongside				
		untie secon-dary skiff tow line from the skiff kingpost			
	take boat out of forward gear and				

			clean John's, Louie's, and Kid's "jelly" goggles		
	order skiff to return				
rev. skiff engine and begin towing seine back to boat					take over plunging from whomever has been doing it
			check out everything on deck to make sure it is ready for the skiff return		
	maneuver boat to meet skiff alongside				
		untie secondary skiff tow line from the skiff kingpost			
	take boat out of forward gear and adjust idle to proper speed for using the power winch				
keep skiff alongside boat so Sk'eg can get out		hand John the secondary skiff tow line	put secondary skiff tow line through forward pulley on davit		
get skiff out of set between seine and stern of boat				lower double block	put plunger away, unfasten double block from boat tow line, and fasten block to deck cleat
		jump aboard boat from the skiff	wrap secondary skiff tow line around forward drum of winch and begin pursing	tie double block line loosely to mast cleat	
		untie boat		hook bunt (skiff's) end	

	take boat out of forward gear and adjust idle to proper speed for using the power winch				
keep skiff alongside boat so Sk'eg can get out		hand John the secondary skiff tow line	put secondary skiff tow line through forward pulley on davit		put plunger away, unfasten double block from boat tow line, and fasten block to deck cleat
get skiff out of set between seine and stern of boat		jump aboard boat from the skiff	wrap secondary skiff tow line around forward drum of winch and begin pursing	lower double block tie double block line loosely to mast cleat	
		untie boat tow line from niggerheads and wrap it loosely around stern drum of winch		hook bunt (skiff's) end of seine to boat with the forward davit hook (rope)	put boat tow line through stern pulley on davit and shut the pulley clasp, say "Okay!" to Sk'eg
	climb down to the deck from atop the cabin			pop Canadian release on bunt end of seine	
take skiff to other side of boat and fasten a line to the line tied to the boat's bow and deck cleats, get the skiff in position to pull boat out of the seine when the time		begin pursing	coil secondary skiff tow line and put it on hatch	get the cork line tied to the bow cleat	go to stern of boat and pull up several fathoms of cork line on the stern
	hook boat's end of seine to boat with the stern davit hook (rope)	stop pursing after seine is close to the davit so that Skipper can hook its end	resume pursing	pull up the forward breast line	

			and begin pursing		
		untie boat tow line from niggerheads and wrap it loosely around stern drum of winch		hook bunt (skiff's) end of seine to boat with the forward davit hook (rope)	put boat tow line through stern pulley on davit and shut the pulley clasp, say "Okay!" to Sk'eg
	climb down to the deck from atop the cabin				
take skiff to other side of boat and fasten a line to the line tied to the boat's bow and deck cleats, get the skiff in position to pull boat out of the seine when the time comes		begin pursing		pop Canadian release on bunt end of seine	
			coil secondary skiff tow line and put it on hatch	get the cork line tied to the bow cleat	go to stern of boat and pull up several fathoms of cork line on the stern
		stop pursing after seine is close to the davit so that Skipper can hook its end	resume pursing		
	hook boat's end of seine to boat with the stern davit hook (rope)			pull up the forward breast line	
	pop Canadian release on boat's end of seine	resume pursing			
	get plunger and plunge in the water beside the davit area				
				check stern breast line and pull it up	

and fasten a line to the line tied to the boat's bow and deck cleats, get the skiff in position to pull boat out of the seine when the time comes			coil secondary skiff tow line and put it on hatch	get the cork line tied to the bow cleat	go to stern of boat and pull up several fathoms of cork line on the stern
	hook boat's end of seine to boat with the stern davit hook (rope)	stop pursing after seine is close to the davit so that Skipper can hook its end	resume pursing	pull up the forward breast line	
	pop Canadian release on boat's end of seine	resume pursing			
	get plunger and plunge in the water beside the davit area			check stern breast line and pull it up	
	stand on stern breast line				
	watch for figure-8's coming up and tell John or Sk'eg when his does			watch for figure-8's coming up and tell John or Sk'eg when his does	

stand on stern breast line

watch for figure-8's coming up and tell John or Sk'eg when his does

help get figure-8 over drum

help get figure-8 over drum

put plunger on top of cabin

watch for rings coming up and tell pursers when they do

"Rings!"

(take a wrap off the drum and purse slowly until figure-8 is through)

(take a wrap off the drum and purse slowly until figure-8 is through)

tie purse line around nigger-heads

tie purse line around nigger-

watch for figure-8's coming up and tell John or Sk'eg when his does

watch for rings coming up and tell pursers when they do

"Rings!"

watch for rings coming up and tell pursers when they do

throw corks on stern back into water

help get figure-8 over drum

put plunger on top of cabin

watch for rings coming up and tell pursers when they do

"Rings!"

tie purse line around nigger-heads

slowly begin to tow boat out of seine but not too fast or hard

unhook stern end of seine from boat

pass double block to John

untie forward

(and pull slowly until figure-8 is through)

tie purse line around nigger-heads

put "dog" on winch and turn winch off

put chains on lift lines

get tow lines and purse line ends off hatch

hook chains with double block

watch for rings coming up and tell pursers when they do

"Rings!"

get guide line over power block to Louie on the stern

watch for rings coming up and tell pursers when they do

throw corks on stern back into water

gather stern end of seine together and take it to stern area, tie guide line over power block around the end

		around nigger-heads		
			tie purse line around nigger-heads	
slowly begin to tow boat out of seine but not too fast or hard	unhook stern end of seine from boat		put "dog" on winch and turn winch off	
			put chains on lift lines	get guide line over power block to Louie on the stern
		get tow lines and purse line ends off hatch		
	pass double block to John			gather stern end of seine together and take it to stern area, tie guide line over power block around the end
			hook chains with double block	
	untie forward purse line from niggerheads and hoist the rings a little by hand			
			get lift lines out of davit pulleys and turn the davit	
	hoist the rings out of the water with the winch			
				push ring bridal lines away from gunwale and straighten them
			help Louie straighten bridal lines	
	lower the rings onto the deck	guide the rings down onto deck	take the chains off the lift line and hand Skipper the double block	help guide rings down onto deck
	fasten double			put chains on cabin wall

purse line from niggerheads and hoist the rings a little by hand		get lift lines out of davit pulleys and turn the davit		
hoist the rings out of the water with the winch				push ring bridal lines away from gunwale and straighten them
		help Louie straighten bridal lines		
lower the rings onto the deck	guide the rings down onto deck	take the chains off the lift line and hand Skipper the double block	help guide rings down onto deck	
fasten double block to mast cleat	swing rings and bridal lines across hatch	swing rings and bridal lines across hatch		put chains on cabin wall
	pull lift line through rings (halfway) and disconnect figure-8	disconnect boat tow line from stern purse line and connect that end of the purse line to the lift line		
take guide line over power block from Louie	thread the purse line end just disconnected through the stern half of the rings (going from the bow to the stern) and connect			engage power block and get its control

					cabin wall
	fasten double block to mast cleat	swing rings and bridal lines across hatch	swing rings and bridal lines across hatch		
		pull lift line through rings (halfway) and disconnect figure-8	disconnect boat tow line from stern purse line and connect that end of the purse line to the lift line		
	take guide line over power block from Louie	thread the purse line end just disconnected through the stern half of the rings (going from the bow to the stern) and connect it to the stern end of seine Canadian release			engage power block and get its control
	get seine started over power block by pulling the guide line				
start towing boat out of seine (slowly)				catch the end of seine coming over power block and bring it to stern area	untie guide line and tie it to rigging pin

		the bow to the stern) and connect it to the stern end of seine Canadian release			engage power block and get its control
	get seine started over power block by pulling the guide line				
start towing boat out of seine (slowly)				catch the end of seine coming over power block and bring it to stern area	untie guide line and tie it to rigging pin
				straighten breast line along deckboard	
	operate the power block controls				
		haul seine (leads, with purse line through rings)	haul seine (corks)	haul seine (web)	
watch Skipper for any special instructions					look for any trouble that may be developing and try to take care of it before it makes us stop hauling

power block controls

haul seine (leads, with purse line through rings)

haul seine (corks)

haul seine (web)

watch Skipper for any special instructions

look for any trouble that may be developing and try to take care of it before it makes us stop hauling gear (e.g., get gilled fish out of webbing)

untie cork line from bow cleat

untie breast line from rigging and hold it tight

get strapping line and double block

straighten bunt webbing

straighten bunt webbing

straighten bunt webbing

take breast line and tie it to niggerheads

strap the bag of fish

hook the strap

					untie cork line from bow cleat
	untie breast line from rigging and hold it tight				
					get strapping line and double block
		straighten bunt webbing	straighten bunt webbing	straighten bunt webbing	
			take breast line and tie it to niggerheads		
			strap the bag of fish		
		hook the strap with double block			
	hoist bag of fish out of water and drop it on deck	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch
bring skiff back to boat and unfasten the line connecting skiff and boat, then bring the skiff around to the back of the boat and wait for the crew on deck to finish		finish hauling seine	finish hauling seine	finish hauling seine	tie guide line around end of seine

			strap the bag of fish		
		hook the strap with double block			
	hoist bag of fish out of water and drop it on deck	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch	pull bag of fish onto hatch
bring skiff back to boat and unfasten the line connecting skiff and boat, then bring the skiff around to the back of the boat and wait for the crew on deck to finish		finish hauling seine	finish hauling seine	finish hauling seine	tie guide line around end of seine
		connect boat tow line to boat end of seine		get bunt end of seine ready for next set	get skiff/boat line to stern and connect pelican to skiff cable
	get back on top of cabin and put boat in gear		pull skiff up snug to stern of boat with winch		
connect main skiff tow line to pelican release on kingpost of skiff			turn off winch		tie boat tow line around niggerheads and coil the excess

bring the skiff around to the back of the boat and wait for the crew on deck to finish		finish hauling seine	finish hauling seine	finish hauling seine	tie guide line around end of seine
		connect boat tow line to boat end of seine		get bunt end of seine ready for next set	get skiff/boat line to stern and connect pelican to skiff cable
	get back on top of cabin and put boat in gear		pull skiff up snug to stern of boat with winch		
connect main skiff tow line to pelican release on kingpost of skiff			turn off winch		tie boat tow line around niggerheads and coil the excess
tie secondary skiff tow line around skiff kingpost		open hatch and pitch fish into hold, clean off deck	open hatch and pitch fish into hold, clean off deck	open hatch and pitch fish into hold, clean off deck	open hatch and pitch fish into hold, clean off deck
	maneuver boat back into the line at the "hook-off"				

attention on it, and being deck boss, I had talked at some length with Skipper about who would do what and had assigned many jobs myself.

It was noted at the beginning of this section that the Toy 1 Series is indeed a series from which a given representation must be constructed. Each set could be described after the fashion of Figures 4 and 5. In constructing the actual artifact of Figure 4, I was guided by the fact that it represents the stable ideal[6] set from the whole season it describes. In 1975, this condensing of the whole season into a single, typical representation seemed reasonable and appropriate to the facts. But in 1976, the crew organization never stabilized to the same extent as that of 1975; there never emerged a stable ideal, rather only a series of modified ideals. Almost every opening we would try something a little different in terms of job allocation-responsibility, but nothing seemed to work as well as Skipper, Frank and I hoped. The reasons for this arose principally from the general lack of skills on the part of one crew member and a poor attitude on the part of another.

The 1976 crew of the Glenda was mostly the same as in 1975 except that instead of Darrell and Richard being the one-yeared experience deck hands, Sk'eg and I were with two new rookies, Louie and Kid. Skipper,

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[6]Each set is at least slightly different from all others owing to small or large contingency actions necessary to cope with fluctuations in environmental parameters. But there is a psychologically felt ideal which underlies each set and thereby makes it possible to see "contingency" actions as such. The point made here is that in 1975 there emerged a stable ideal, giving a sense of continuity in spite of contingency differences, whereas in 1976 there was little stability in this very ideal, in addition to the sort of statistical instability arising from accommodations to varying external parameters.

Frank, and Annabelle were still the ruling triumvirate, but this time, Sk'eg and I were the favored sons.

Louie was about thirty-five years old, originally from Brooklyn but his soul belonged in his recent residence on the big island of Hawaii, and he was one of the strangest persons I have ever met. He weighed around two hundred and fifty pounds, mostly around his middle, but was not very strong in his arms, shoulders, or legs. His avid interests were astrology, health foods, and massage techniques. He was without a doubt one of the most inept rookies seining has ever had to deal with, and he was on my boat.[7] On his good side, he was great to have with respect to fostering "good vibes" on the boat. We all picked up some of his exaggerated mannerisms, and after we learned not to expect much from him and not to take him so seriously, we all liked him in spite of the extra work his presence on the crew created for each of us.

The other rookie, Kid, was twenty or twenty-one, a "biker" (motorcycle freak), and wanted to be called "Calamiti" rather than his name, Tim. Louie started calling him "Kid," and despite his protestations, it stuck. Kid was very impressionable. He swaggered about relating how tough he was and what a good worker he was. Within three days of his hiring, he had successfully alienated everyone else on the crew, though

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[7]All the seiners who met Louie during that summer remembered him and could not understand how he could do the work. His legacy, in the form of peculiar "Louie" stories, will remain with me to my death. As an example of his total incompetence around boats, he seined with us until the end of the season and he never learned to tie a line around a cleat. He would just wrap the line in a figure-8 pattern until he came to the end, and then look around sort of helplessly for someone to re-do the job.

we were still trying to give him the benefit of the doubt. He was good at figuring out his jobs seining, but he did not partake of the ethos. He would stand idle while others were working and not jump in to help.

This 1976 crew was riddled with strife. Louie and Kid did not get along. Louie was socially acceptable to the rest of the crew; he was just a buffoon when it came time to work. Kid was acceptable as a worker (other than his attitude), but no one liked him. The two of them being rookies, however, put each in a position where they had to try to be accepted by their seniors on the boat, and this set up a needless competition between them. Also, there was no love-loss between Sk'eg and Kid; they almost came to blows while hauling gear. Kid was doing the web pile, but was not very good at it (he was rather terrible), and he would jerk Sk'eg's leads out of his hands and generally mess up Sk'eg's work rhythms. Sk'eg and I thus felt very put upon by our fellow deck hands and resented their being on the boat, not to mention that they were getting the same share. It was a bad scene.

Perhaps part of our problems that season can be located in the fact that Louie got his job on the boat via mail (the Skipper took him on the recommendation of a previous and highly favored crew member), and for that reason, he was not in any sense evaluated beforehand. (Seiners from other boats would ask us how Louie was doing because just from his appearance they could not imagine him working well on a seine boat.) He was hired originally to be our web man, but even before we went out for the first opening, that idea was seen to be wishful thinking. So, at the last minute, we hired Kid to do web. Once again, we had no time to

indoctrinate him into the proper ethos and traditions of the boat or to evaluate him. The situation could have been remedied quickly if Skipper had just fired the two of them and gotten replacements. But he was reticent to fire Louie because he had travelled so far for the job (besides, Louie was comical), and Skipper's personality makes it difficult for him to fire anyone. The social tension on the boat mounted each opening as Sk'eg and I got more and more irritated with what we began to realize was an exploitation of our good intentions and generosity. The two of us had to make up all the slack our two rookies left. From my double position as a seiner and social scientist, I could be more tolerant than Sk'eg because at least I was getting some data on the significance of crew compatibility. Skipper was aware of our resentments, and at the end of the season, he apologized to Sk'eg and me for not firing Louie and Kid. On their side, I sincerely doubt if either Louie or Kid will ever go seining again. Louie would like to, but no one who knows about him will hire him; I think Kid has weeded himself out.

If the reader will follow Louie's job routine on Figure 5, he can see that Louie did not exactly do very much and especially not anything "important." Skipper and I intentionally tried to keep him busy, but out of everybody else's way. On any other boat, he would also have had to cook in order to receive a full share of the catch. Kid did a full share's work, and except for being poor at piling web, he was capable of doing all the work, just as Sk'eg and I had been the previous year. Thus, Louie's incredible disabilities were the single largest factor in our efforts to hit upon a good crew organization.[8]

Without going into any more detail concerning the organization of work in my 1976 season, I should like to make some general comments about this crew as compared with others.

First, the 1976 crew was a poor crew. We were not very fast at retrieving our gear, we did not get along very well, and we did not catch very many fish.

Second, we never settled deeply into a stable system of job responsibilities. I have already discussed this aspect, but I would like to add that perhaps a major reason we did not was that I was the deck boss and I had already constructed Figure 6. This was influential on me in my capacity as deck boss because I saw that position as a general responsibility to organize us in the most efficient manner I could think of, and I used my understanding embodied in Figure 6 in this applied effort.[9] Each set, however, was underlain by an ideal allocation of responsibilities; it is just that the ideal was altered every so often in hopes of improving our speed. We never went so far as to change who piled what while hauling or who pursed. The alterations were of a much smaller magnitude. Also, we did not switch whole routines or portions thereof from set to set.

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[8] If the reader is having a hard time believing what I am saying about Louie, that is good because I would not have believed it either had I not been there.

[9] I had a fair amount of difficulty trying to decide which "self" I owed most allegiance to--deck boss or anthropologist-observer. As an anthropologist I felt I should let things develop on their own, but as a deck boss, a member of the crew, I felt I had to use whatever skills I had to improve our work. I decided in favor of the "deck boss." This is perhaps no big point, but I thought I should mention it for honesty's sake.

Last, this crew convinced me, if I needed convincing, that seining requires certain skills and that not everyone can do it. Just being present on the deck of a working seine boat does not mean that one will learn seining. It takes effort.

Figures 4 and 5 depict crew organizations from two consecutive years on the same boat and largely the same crews. Each figure's history of construction has been described (they are different), and the personalities of each year's crew sketched. Also, certain comparative remarks on each have been provided.

At first hearing, the idea to represent "crew organization" in the form of flow charts of who-did-what-in-what-order may sound easy and simple. As has been discussed, however, this mode of depiction has difficulty capturing developments in crew organization and by itself cannot really convey any psychological nuances of the actual work. Even with these deficiencies, I hope the figures have given the reader a reasonably clear notion of what "making a set" involves for the people doing it, and that the auxiliary discussions have fleshed out the compacted data held within the figures.

The Toy 1 Series remains very close to its empirical grounding. It is a very natural question for seiners to be asked who does what on their boat. They may or may not be able to answer fully, owing to activities going on which are out of their attention, but the question will be natural. They will answer the question on the basis of what their organization is at the time they are asked and, of course, at varying

levels of detail. If asked at the end of a season, they will answer on the basis of their personal feelings about what the typical allocation of responsibilities was from the season as a whole. Even on boats where the crew rotates jobs and switches portions often, there is an allocation of job responsibilities to the extent that each person is associated with a job routine which is "his," though only insofar as his name is the mnemonic for that routine. (My third year's crew was like this.) So, Toy 1 is not as simple as I first anticipated, but I still feel it is close to the data.

3.3 The "Toy 2 Series"

The Toy 2 Series is more abstract that the Toy 1 Series. Figure 6 (below) is not only a skeletal representation of an ideal selected from amongst possibly several during a season, but it also ignores the crew who makes a set by focusing entirely on what collectively gets done. Hence from its inception, it is not based upon even what was supposed to happen during sets, for such empirical grounding would have to encompass the who of the doings. Rather it is a representation of the logical ordering in time of "jobs." It is an a priori construction built by applying a certain frame of mine to the duality composed of a given style of seining and the equipment of a given boat.

Yet Toy 2 construction principles allow for a series of Toy 2 representations. Differences in equipment would very likely produce slight differences in the set of "jobs." Similarly, even with identical equipment, different seining styles would produce different "jobs."

Any given Toy 2 could have many different Toy 1's associated with it, that is, a given Toy 2 could underlie several crew organizations. This one-to-many relation holds because a given Toy 2 represents the modifiable but nonetheless basic constraints on possible crew organization. Toy 2's depict what the "jobs" are (from a certain perspective) and what order they must be done in, but they do not dictate particular divisions of labor. A given Toy 2 (for example, Figure 6) is the compounded union of the delimiting constraints set up both by the hardware (material culture, equipment) and the software (skipper's chosen style of seining, a particular thread in the seining tradition) on a boat. This union takes the form of a set of "jobs" with a partial ordering imposed upon it.

"Jobs" in the Toy 2 Series are not the same, however, as "jobs" in the Toy 1 Series. In constructing a Toy 2 Series representation, I identify as a "job" anything a human must do involving movement of hardware, the people themselves not counted as part of that hardware. But, in addition to this first definition, there are some "jobs" which humans do that do not involve movement of hardware. These secondarily identified "jobs" are verbalizations functioning to announce some state of the hardware and thereby coordinate subsequent actions insofar as the mere announcement affects multiple sequential chains. The best example of such a second order "job" is "announce that the rings are almost up." Thus, these "jobs" are not musts, but they are few in number and regular occurrences on seine boats. For these reasons, I have chosen to include them in Toy 2 Series depiction. They could be omitted without really

altering a Toy 2 structuring of the other "jobs," but in such a case there would have to be some sort of monitoring of important states of the hardware, that is, "jobs" (some) would initiate not relative to other jobs but to certain states of the hardware. A good example of this would be taking one wrap off a power winch drum when the figure-8 clasp on that side comes through its davit pulley. In Figure 6, I have diagramed this sequence by incorporating "announce the figure-8" as a "job," but an alternative mode of handling the same situation would be to have another kind of formal object (other than "jobs") which can be part of R_1, i.e., "hardware states" in brackets, for example. "Jobs" of this second kind are indicated by stars next to them, and may be regarded either as a distinctive kind of "job" or as a different formal object altogether (e.g., "hardware states")--the results in terms of R_1 sequencing are the same in either case.

Furthermore, so long as the movement of some hardware is relatively continuous (or regarded as such by subjective criteria[10]), I identify it as a single "job," whether it lasts for .5 seconds or 15 minutes. These considerations can produce "jobs" which seiners may or may not find natural, psychologically salient, unlike Toy 1 Series "jobs" which are identified (optimally) through analysis of naturally produced and observed

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[10] A job such as "pulling up a line" is not precisely continuous measured by minute space-time behaviors of the rope itself. There will be slight discontinuities as the worker reaches down with his spare hand to take a new grip. But the human action of pulling on the line is relatively continuous and the whole is thus a Toy 2 "job" rather than each arm's tugging.

unities in each worker's narration of his routine.[11]

Thus, Toy 1 and Toy 2 Series are not comparable as they have been illustrated and presented, but they could be made so either by making Toy 2 "jobs" meet the criterion of psychological saliency or by expanding and elaborating Toy 1's such that they use the same set of "jobs" as a Toy 2. The fundamental difference between the two modes of seeing in this respect is that Toy 2 takes an analytical pose which reduces work to physical movements of unities (pieces of hardware) in space, incorporating only to a very limited extent the subjective experience or perspectives of the worker (it does use this to identify some "jobs," the relatively continuous ones).

Neither the Toy 1 nor Toy 2 Series attempts to equate "jobs" with the notion of "natural units of behavior" as conceived in ethology, dance, or other naturalistic approaches to behavior. Such approaches would vastly increase the sheer number of "jobs" as well as identify them differently. Thus, the notion of "job" as used in the Toy 1 Series is derived from the data itself; it is a part of the language in terms of which the work is actually conducted. In the Toy 2 Series, "job" is conceptually sort of halfway between Toy 1 usage and the idea of a natural unit of behavior underlain by a unitary mechanism. The principal difference between Toy 2 "jobs" and natural units of behavior is that the latter focuses upon the doer of work, the movements of the living organism,

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[11]This criterion was abridged by the "fudge factor" in Figure 4 and adhered to only in a sense in Figure 5 because I was there the narrator for all of us. Optimally, a Toy 1 Series representation would be based directly on the narrations of all workers, and they would have comparable detailedness.

whereas the former focuses more upon the unity of movement manifest in the object being moved (but caused by natural units of behavior or strings thereof in the worker).

An additional point about Toy 2 Series "jobs" is that they fundamentally incorporate a particular boat's spatiality because movement of hardware necessarily takes place in space, the boat's space. As a result of this, some "jobs" are nothing more than "get x and bring it over to $space_i$."

With these conceptual points in mind, the reader may wish to study Figure 6. The notion of "job routine" does not make sense in the Toy 2 Series. I should like to call attention to the fact that Figure 6 is merely a particular Toy 2 in the Toy 2 Series. It depicts the basic constraints on crew organizations (Toy 1's) for the Glenda Lou during 1975 and 1976. Like the Toy 1 Series, Figure 6 does not include contingency plans, although in principle these could be added. To illustrate this, I have included one area where alternative job sequencing would obtain depending upon the size of the catch (choice of whether to hoist up fish with the power block, strap the bag and hoist with double block, or "brail").

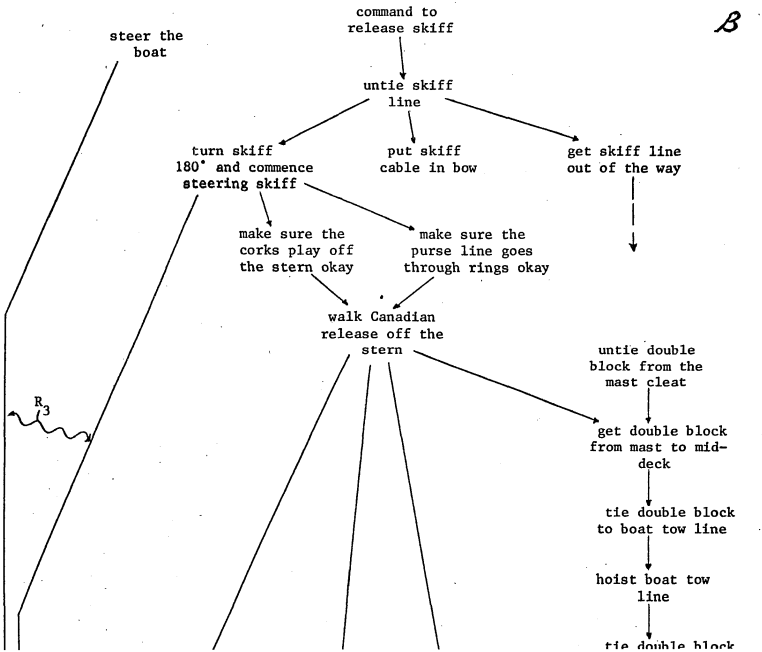
The basic relation underlying Figure 6 is what I call "R_1," which is defined as a condition between jobs such that Job_i must be completed before Job_j can begin. In such cases, an unlabelled arrow is drawn from Job_i to Job_j. In this way, R_1 arrows show direction of ordinal-time, that is, R_1 defines elementary sequencing (not full temporality). By inspection, the relation is transitive, anti-symmetric, non-reflexive,

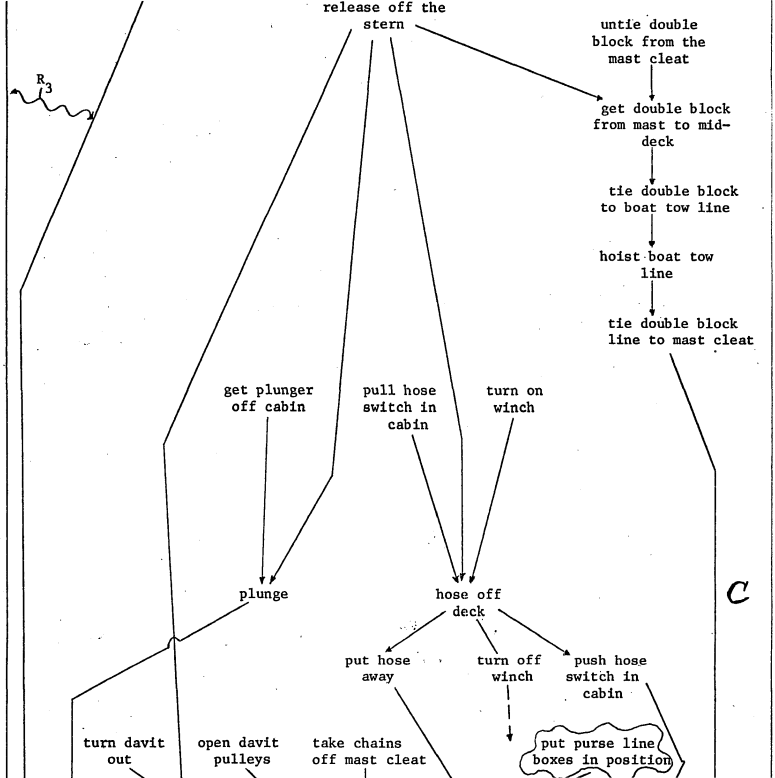
Figure 6. The "Toy 2 Series" representation of a seining style

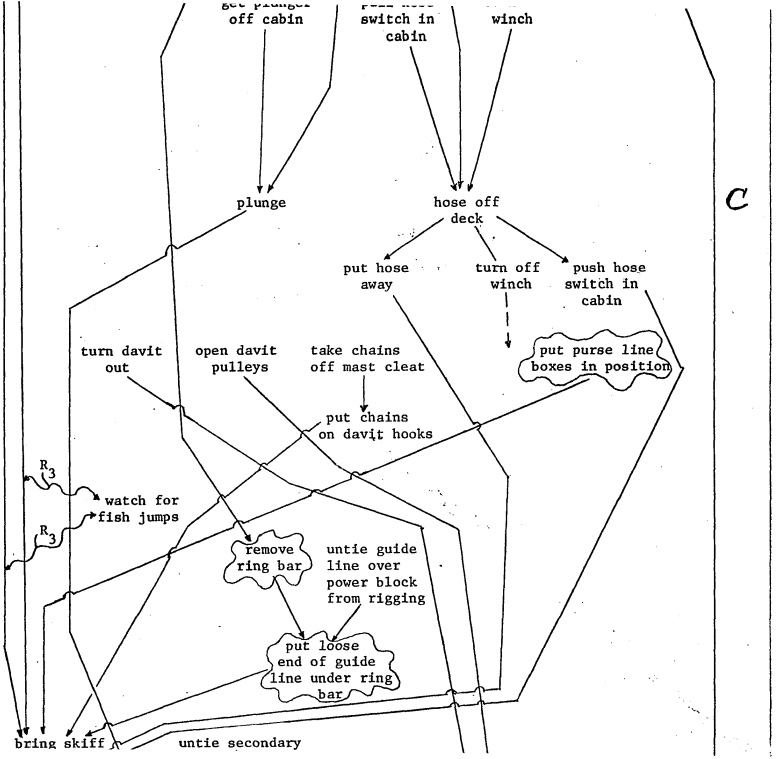
TOY 2

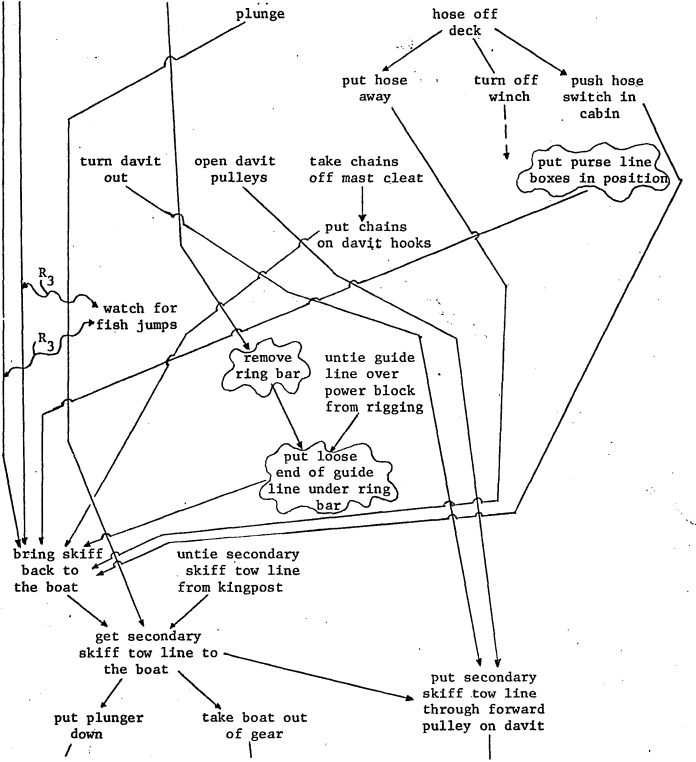
A

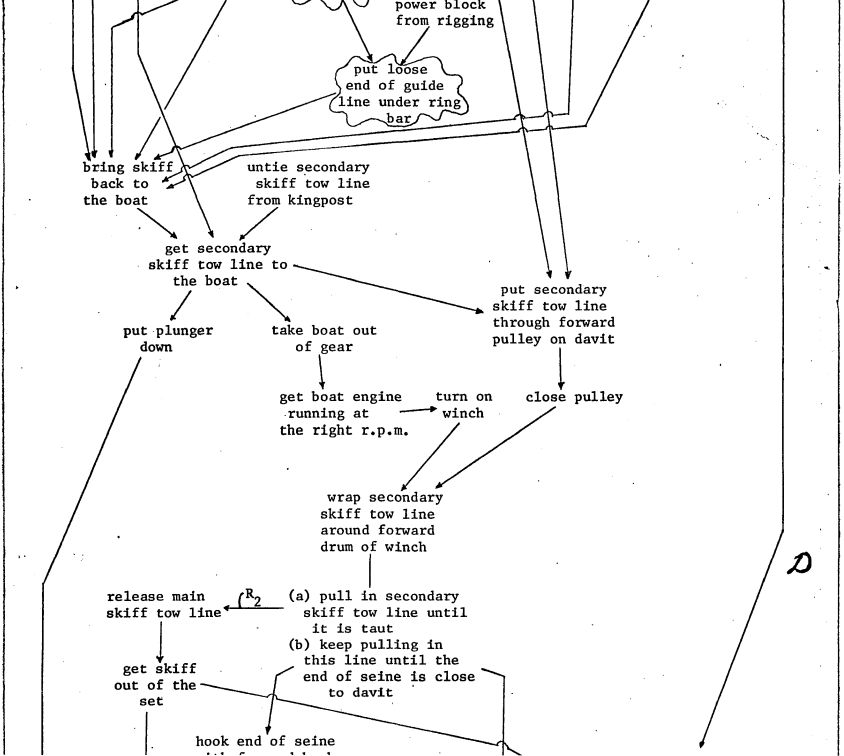
B

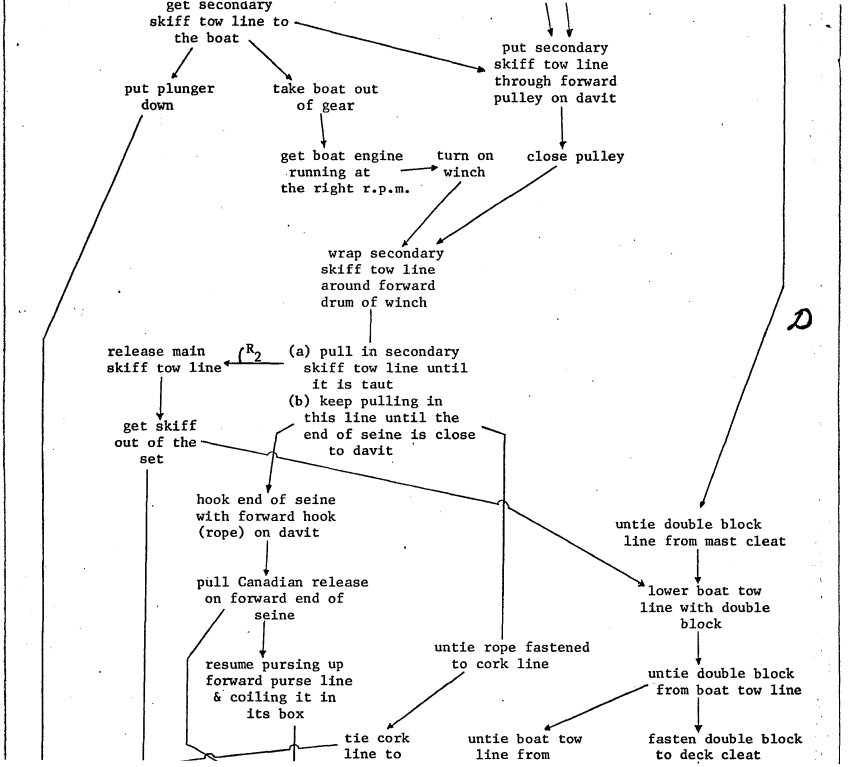


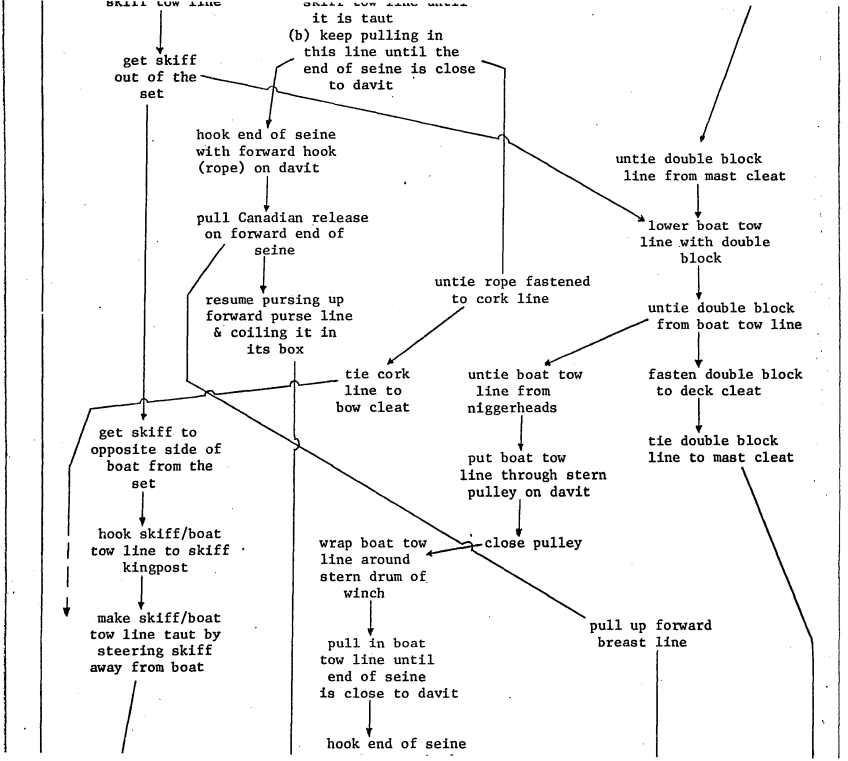


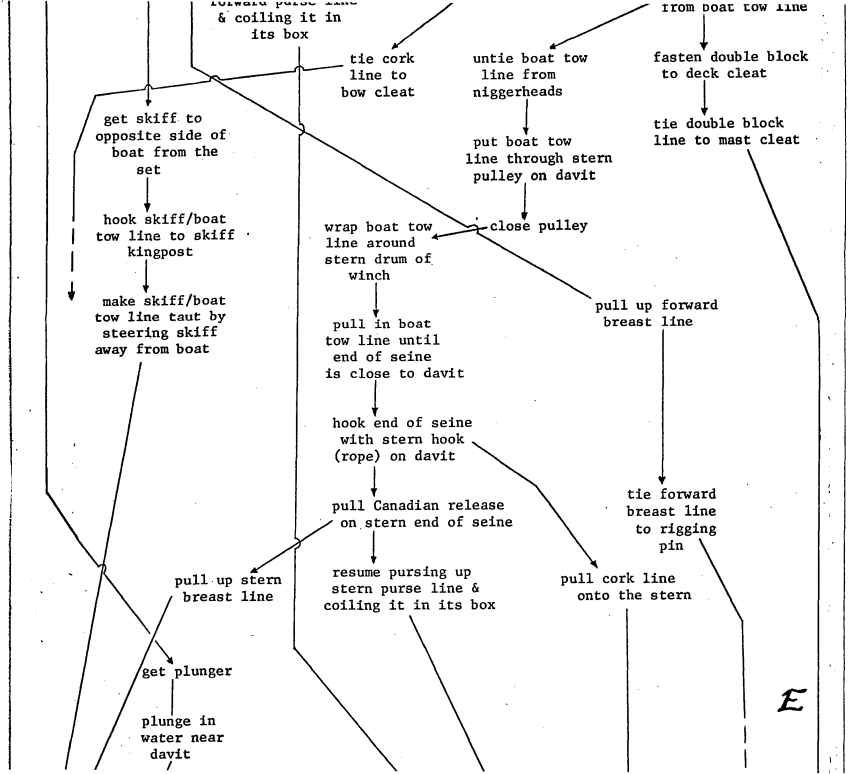


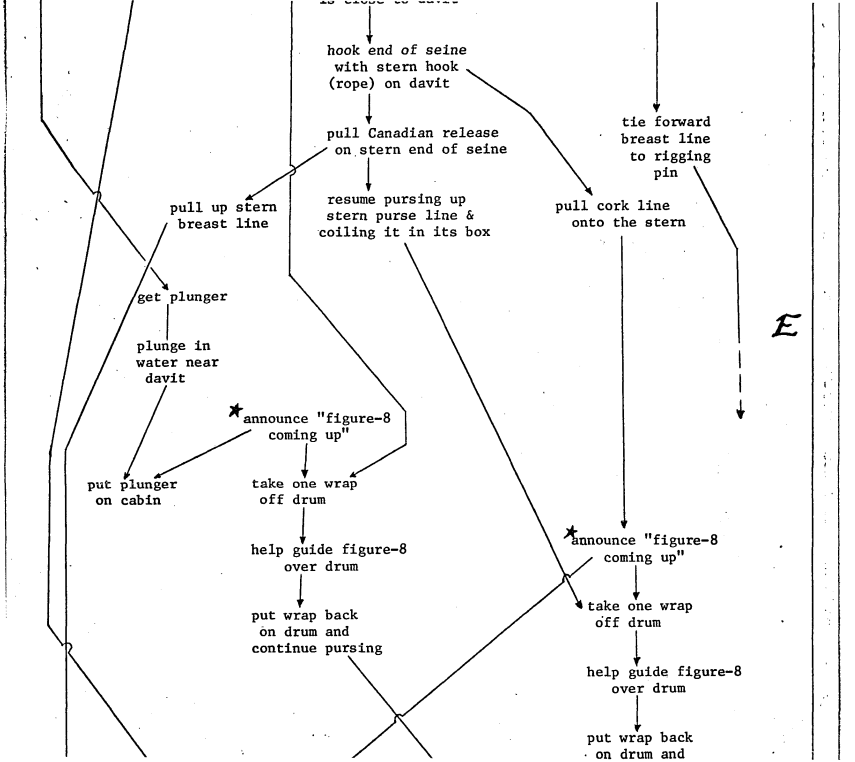


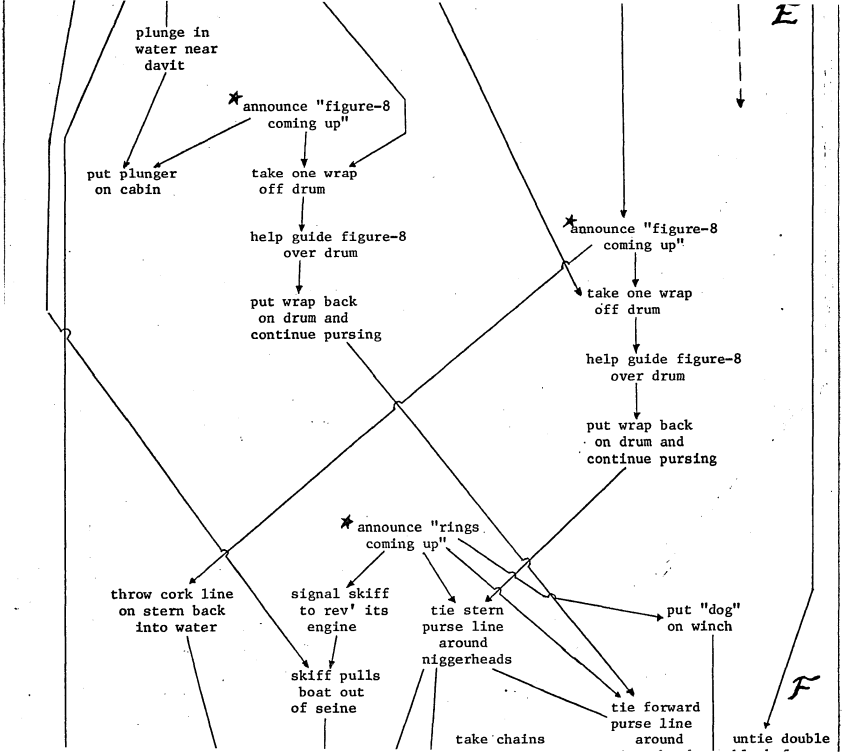


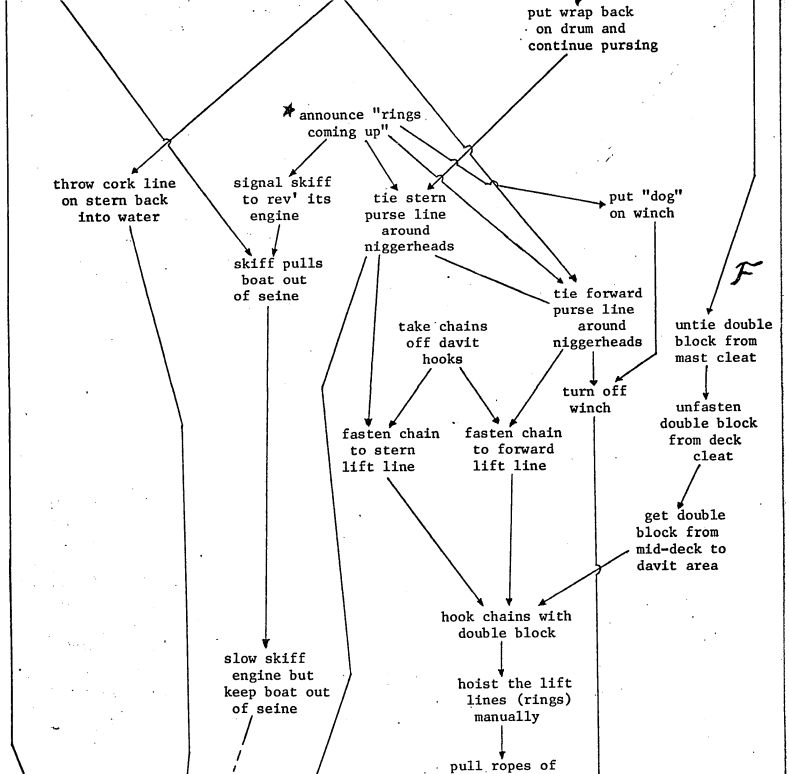


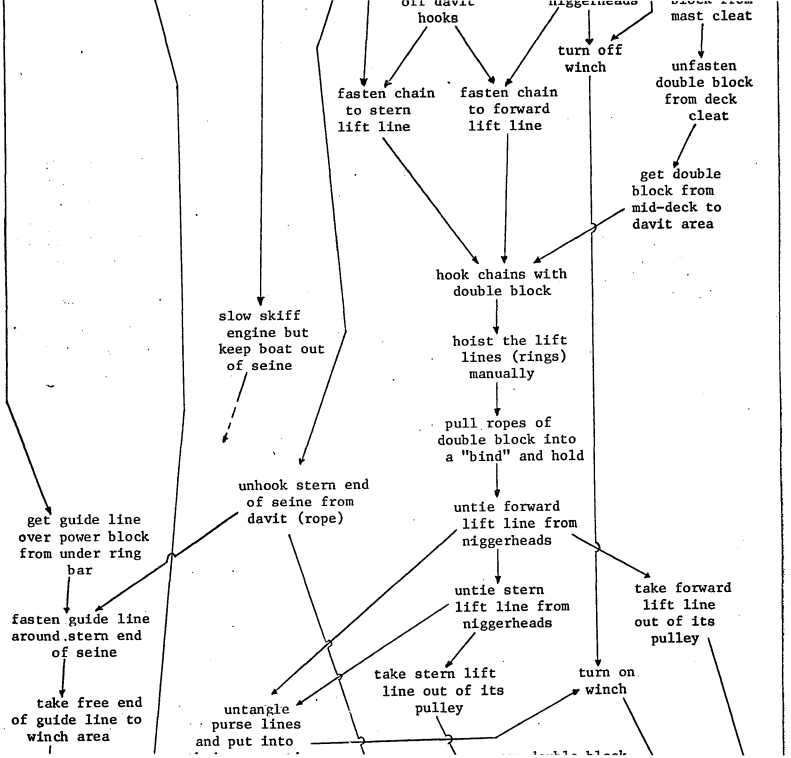


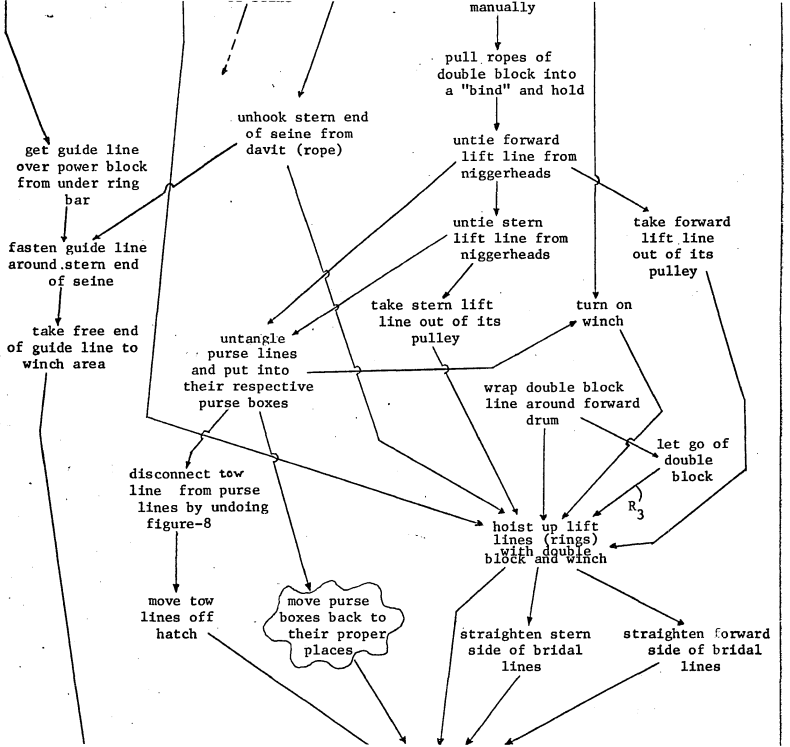


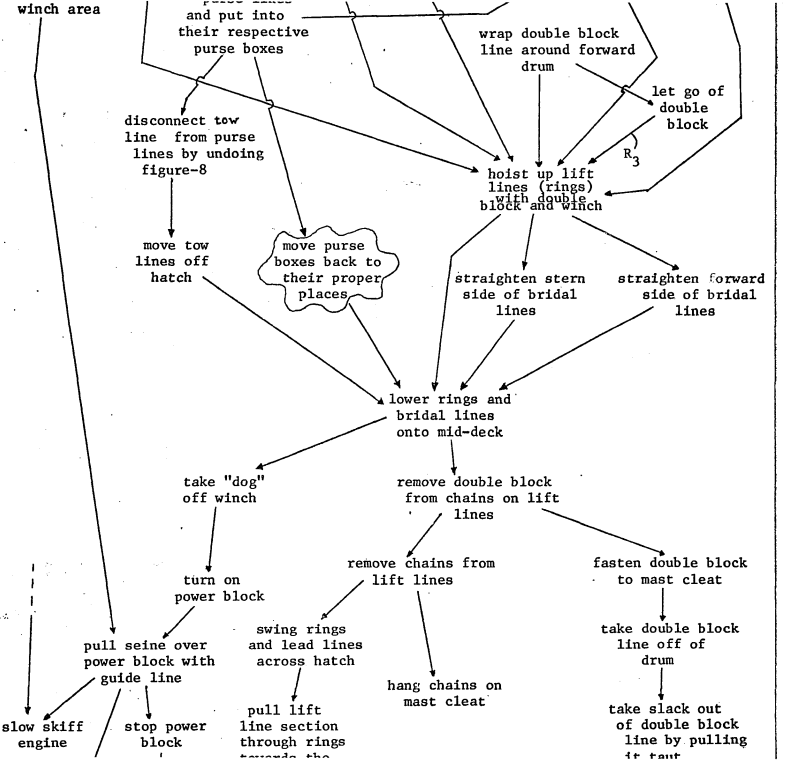


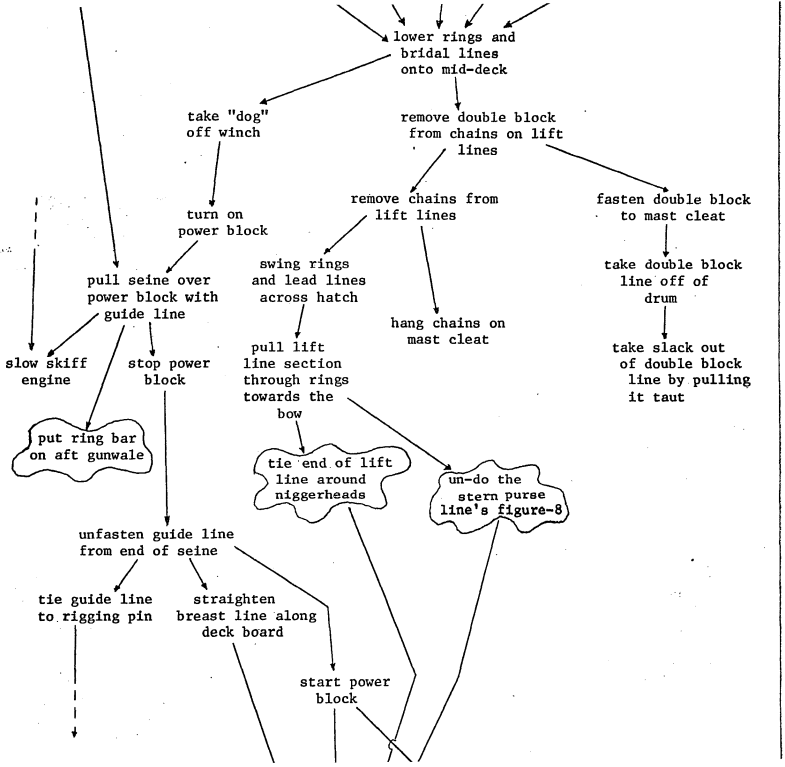


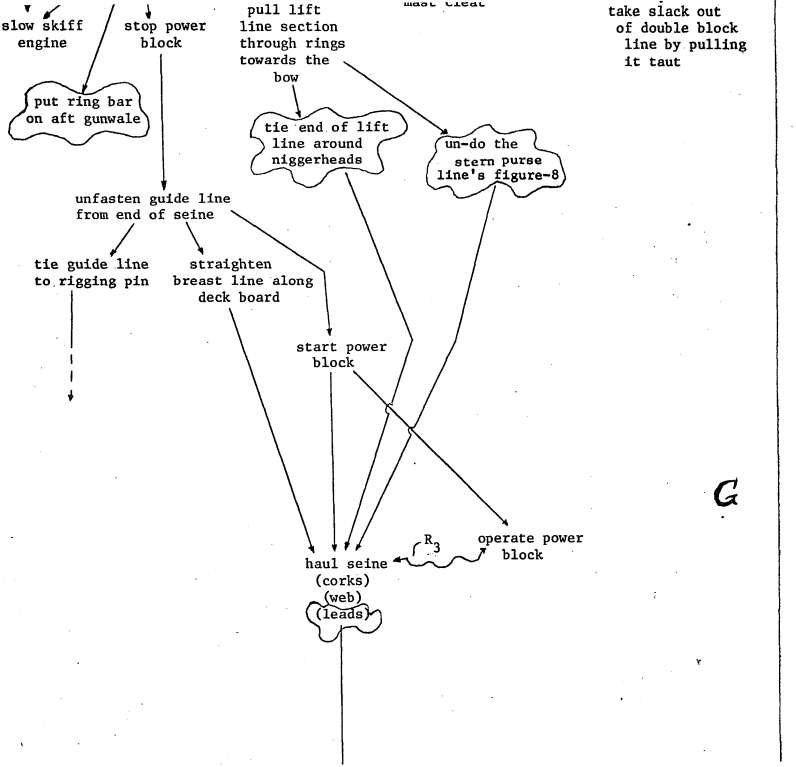


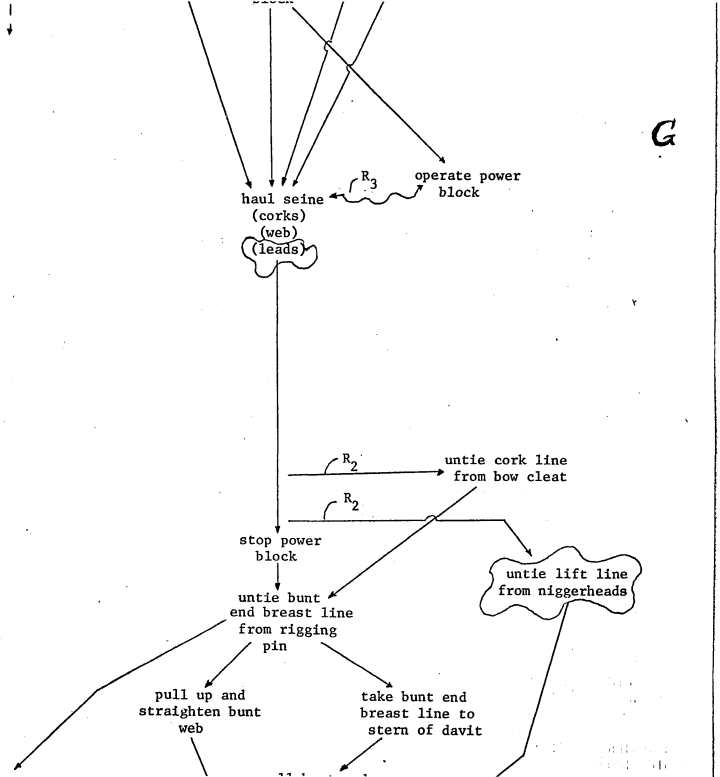


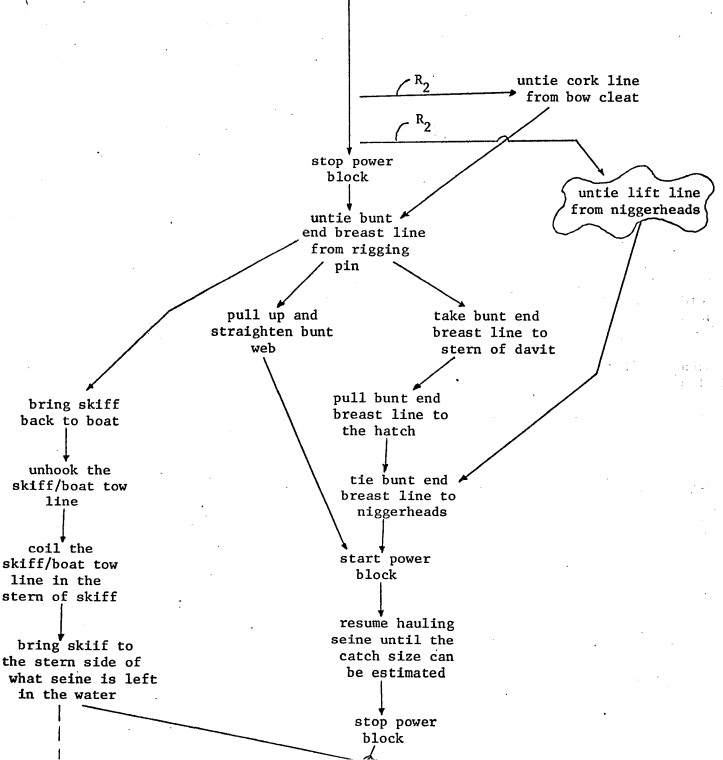


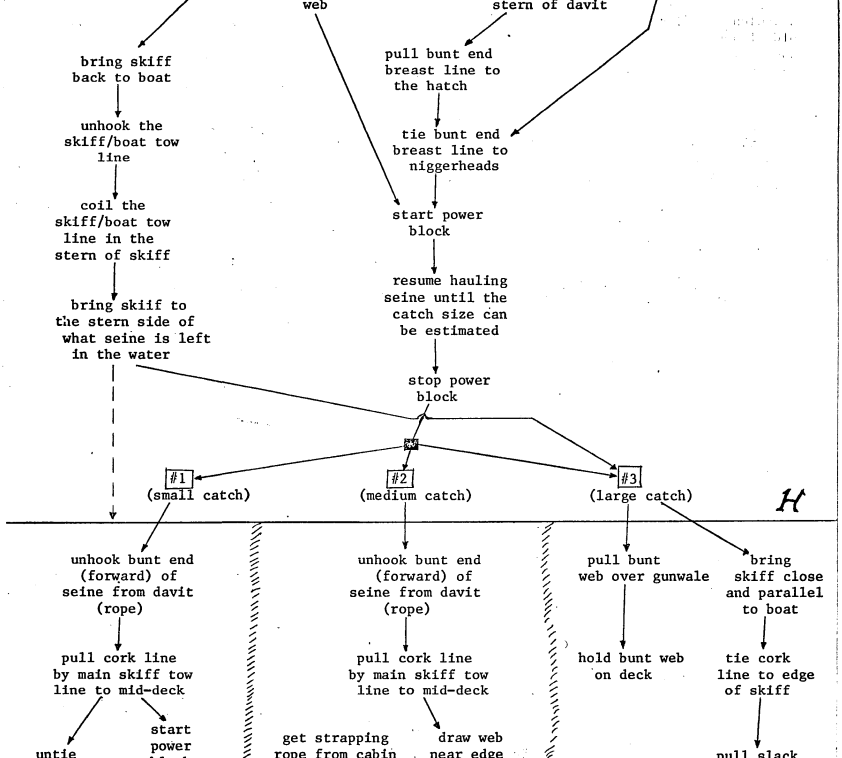


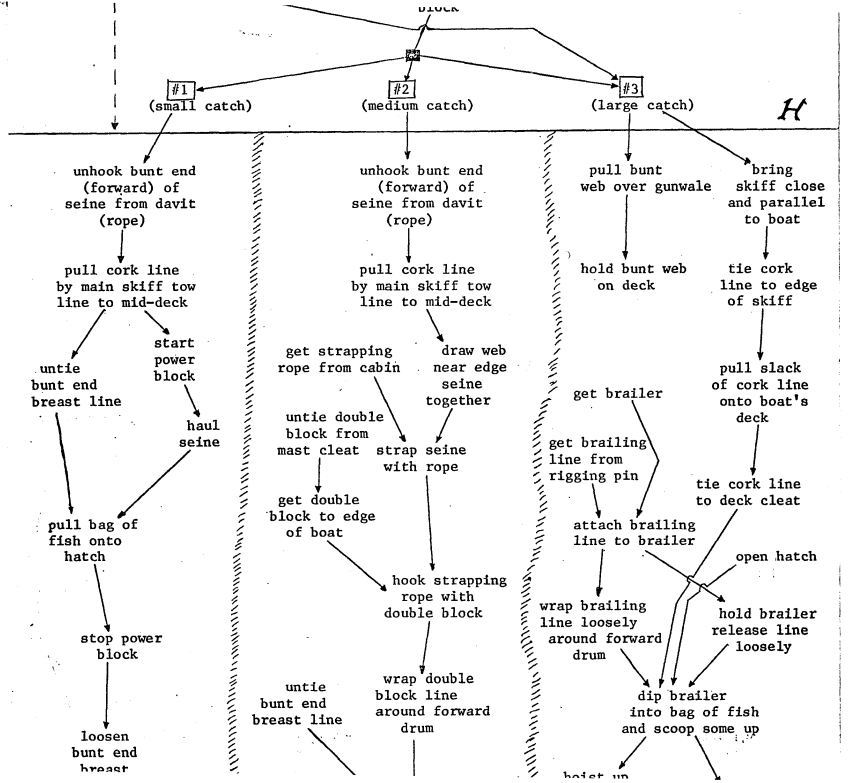


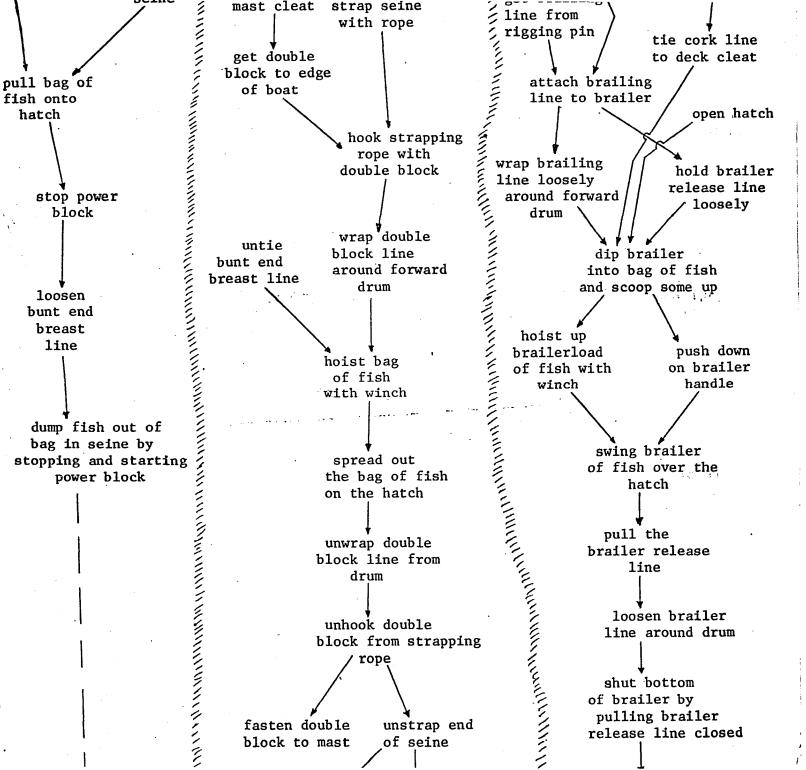


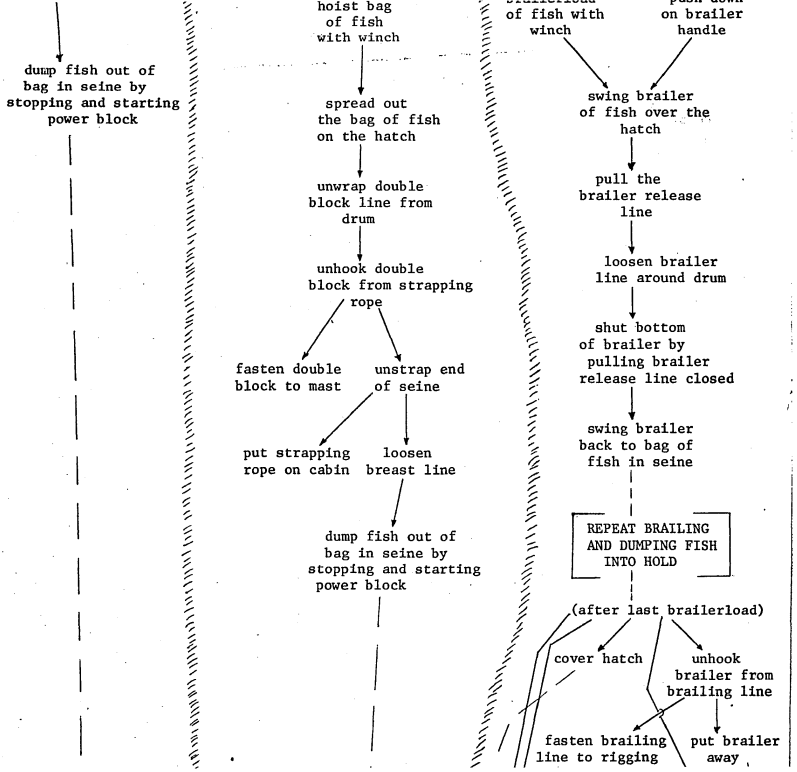


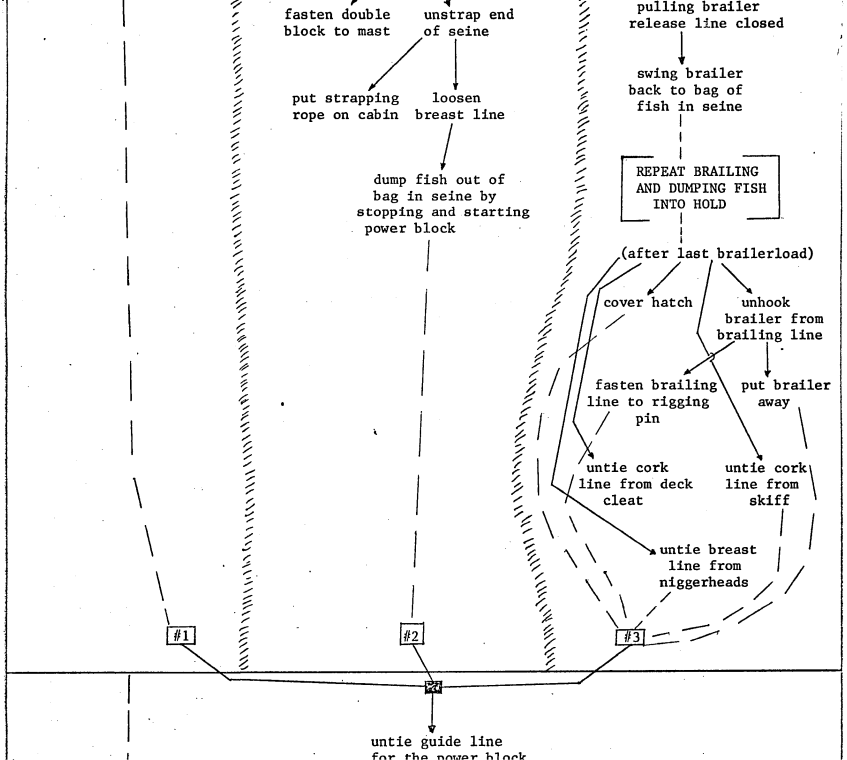


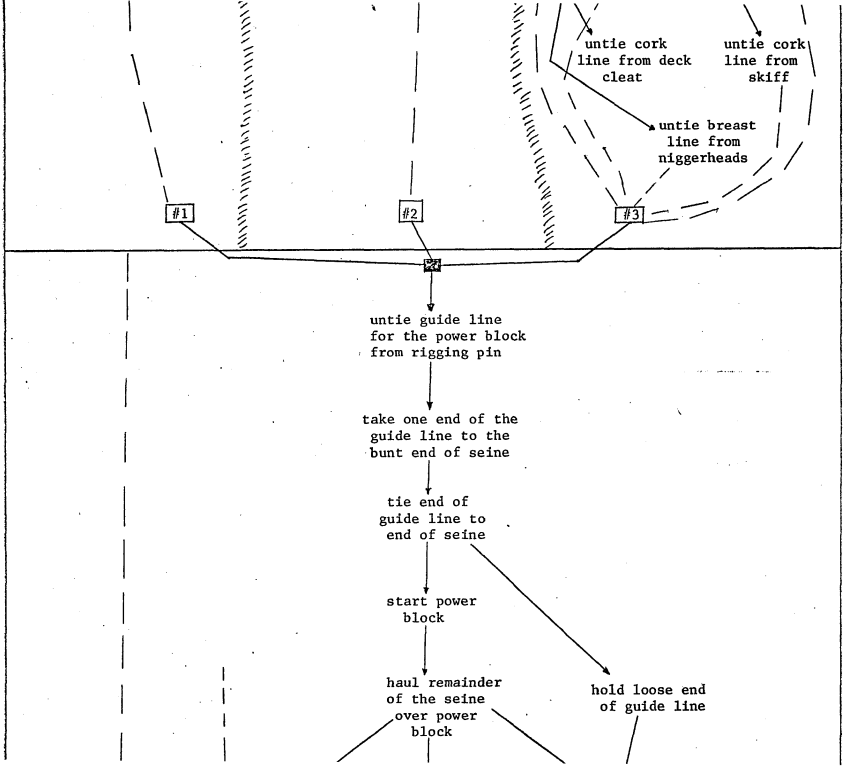


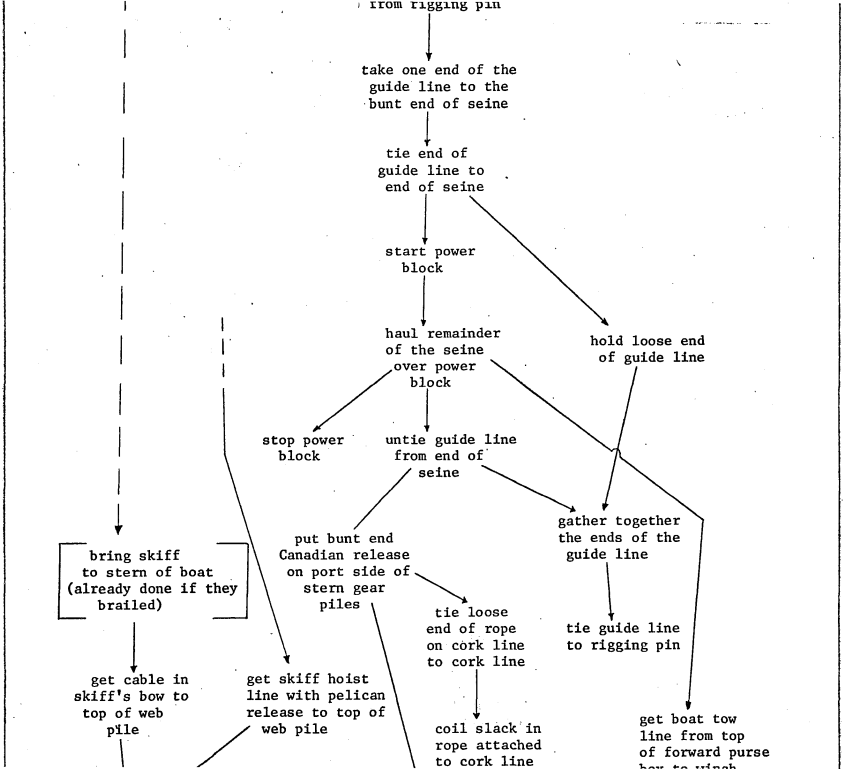


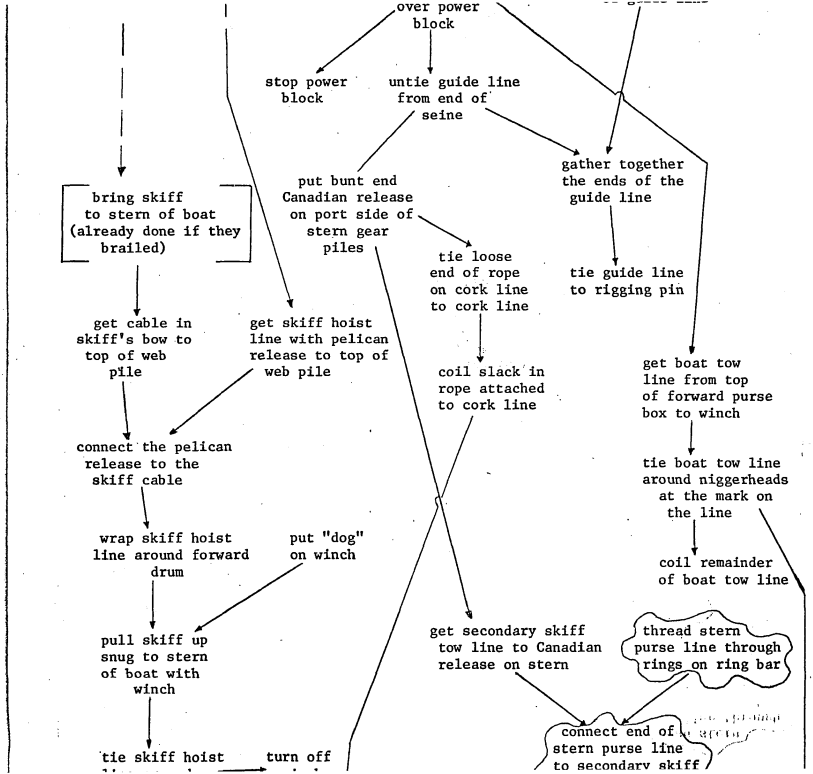


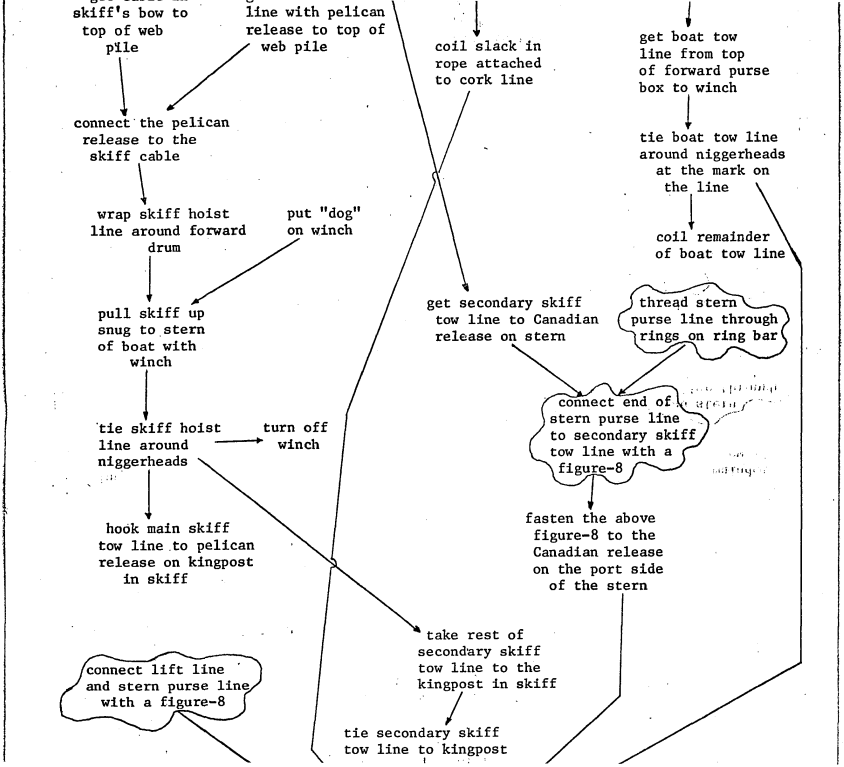


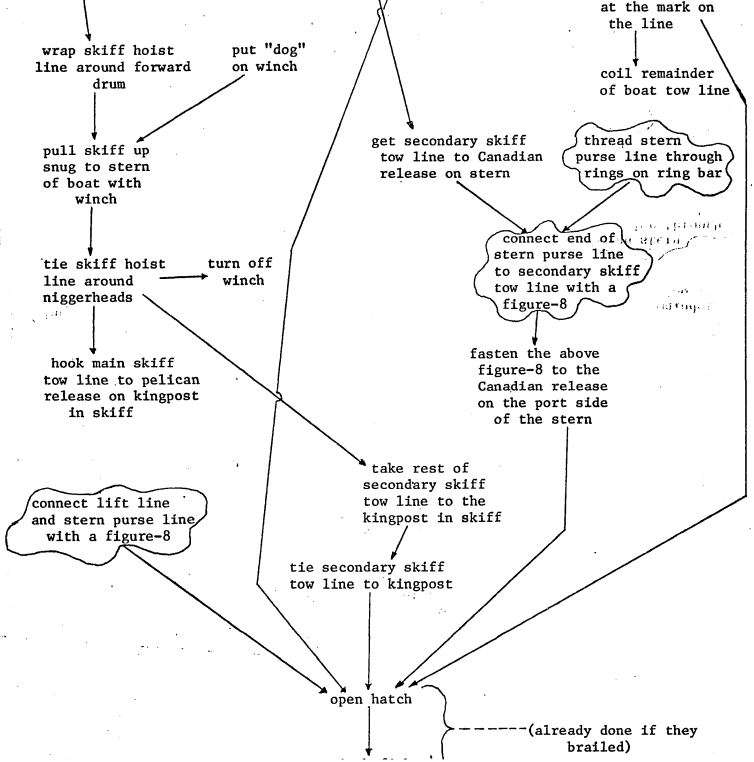


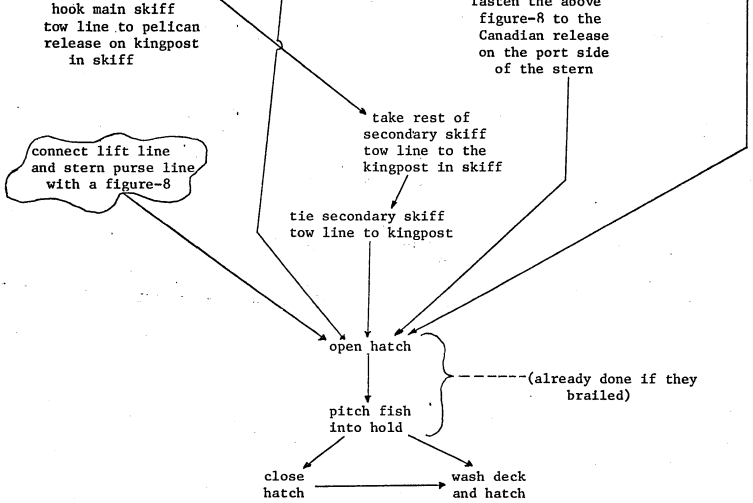












and non-commutative (for definitions, see Wall 1972:110-114).

There are other kinds of information in Figure 6 in addition to the partial ordering of R_1--interesting points where Toy 2 logic is violated, additional relations that could be defined and added to the diagram, and ancillary remarks concerning general categories of "jobs"--but before continuing in this vein, I would call the reader's attention to the following "Log of Emotional States." The log is written from my 1975 perspective in the operation (see Figure 4), and thus it is an entirely different order of description from the "objective" modes found in Figures 4, 5, and 6. Nonetheless, the two perspectives naturally cross-cut each other in the process of making a set, and by conjoining this log with the "objective" accounts already given, the reader may be able to form a clearer picture of my first year's experience.

LOG OF EMOTIONAL STATES WHILE MAKING A SET

(A) This time region begins when I first become aware of an impending set. There is a build up of excitment which plateaus at a nice level of anticipation once everyone is in position and at the ready. This plateau requires that all equipment is "All systems, GO." The anticipation is peculiar in that I do not know what I am expecting, but it is still "anticipation." I suppose that it is similar to getting into a set position in football, but there the time scale is very small. In fishing, this pregnant feeling lasts longer (maybe thirty seconds or so). Is everything ready? Everyone is position? Okay, freeze and wait for the Skipper to overcome that constipated look and give us the signal to cut the skiff loose.

(B) GO! Richard very rhythmically unties the line holding the skiff. POP! The skiff is free. Frank whirls the skiff around and rev's the engine, the net starts playing off the stern, and Darrell starts guiding the purse line through the rings. I can hear the leads, corks, and rings bouncing off the stern. The excitment has been released with the skiff, and I have a sense of controlled speed as I take the skiff line, coil it, and stow it along the galleyway on the opposite side of the boat as the set is being made. We have begun another set; we will go through the whole process now. There is no turning back. Now, I may be

feeling happy or irritated about making this set depending on how hot I have been, how many fish we have been getting, and so forth. The plateau diminishes steadily down to the social, joking time of plunging. But, as I stand smoking a cigarette or cigar and watching my cork pile unwind, I experience an aspect of pride or slight embarrassment depending on how smoothly and evenly my labor investment of the previous time we hauled gear performs as it slides off the stern. If my cork pile the previous set was asthetically excellent, then I feel saddened as I watch it vanish without leaving a trace of its beauty except in my memory and perhaps in the esteem my fellows have of me. On the other hand, if it was not so good, then I am happy that it is finally dying and giving me the chance to do a better one.

When the seine is almost off the stern, I pick up the Canadian release and walk it off the deck (we do not want it to pop open accidentally!). Then, I am always pleased with the way Richard has started the double block swinging across the deck towards me so that when I turn around it is just about to me. The timing is beautiful. Very rapidly, I twirl the chain around the tow line twice and hook it. The speed of this pleases me because it took a little while to learn to do it so fast. As Richard hoists up the tow line with the double block, I invariably sing, "Do the mon-key" (chorus of a Rolling Stones' song) and get the plunger from atop the cabin. The chorus I sing at him is appropriate because the way he pulls the line of the double block makes him look just like he is dancing The Monkey (dance style of the late sixties), and it always makes us smile.

(C) Plunging is usually fun. There is a lot of idle talking and playing. It is also nice exercise for the arms, provided we take turns at it. I look out at other boats, watch the scenery, watch for salmon jumps, and am pleased as the deck gets all cleaned off with the deck hose--it satisfies my sense of orderliness. It is fun to get the davit equipment set up to receive the skiff end of the seine--once again my sense of orderliness gratified. After all the preparations are made, I can take a piss, get a smoke or coffee, or just sit and talk with Richard and Darrell. We may hold comical seminars on plunging styles and their relative merits or play "name that Beatle tune" or just soak in the Alaskan view. Before too long, Darrell will start checking everything just to be sure we have not omitted some preparation for the skiff's return, and then he cleans my jelly goggles for me. I feel fine, it is one of my favorite times. The work is easy yet "work," and I feel calm and sociable. I like the sound of the plunger as it pops the water. Everyone is jubilant when we have cause to yell "Aye-Oh" or "INside" and laughs when there is an "OUTside." One at a time, we might go up on top of the cabin and chat with Skipper. It is a very pleasant period, plunging.

(D) When we notice the skiff coming in, I take over plunging. We are all dressed in our basic raingear by this time, leaving off only as much as we know we will have time to slip on later without slowing everybody else down. Once again, this is the calm before the storm. The

excitment level begins to rise to a controlled plateau. As Sk'eg is about to hand Darrell the secondary skiff tow line, I begin my actions and stop plunging. Darrell almost always plays some kind of joke on Sk'eg at this point, such as acting as if he were going to throw a bucket of water on him.

As Sk'eg actually gives the tow line to Darrell, there is an almost instantaneous bustle of activity. This is accompanied in me by a large rush, both physically and mentally. As soon as I put the plunger aside, I grab the boat tow line and watch and wait for Frank to POP! the main skiff tow line pelican release (after Darrell has the secondary one around the winch drum) and then maneuver the skiff out of the set. So, actually my mental rush begins before I do very much. At any rate, when Frank is clear, it seems that I literally burst into action, all of which is articulated with other folks' work and spatial positionings. I know just where everyone on my paths will be at what times. It always tickles me slightly that I literally run around the boat every set, and it is at this phase that I do this. (Once I figured out this spatial pathway (all the way around the cabin), it helped me to remember my routine.) By the time I get to the stern and begin to pull corks, the sense of "rush" has subsided. During the trek from the bow (where I tied the cork line to the cleat) to the stern, however, I felt like I was doing a dance. I would duck under the purse lines, squeeze by Sk'eg in the galleyway (he would move sideways just a tiny bit without even looking--timing, again), get the cork line secured to the bow cleat, run down the other galleyway saying hello to Frank and Annabelle (through the galley window), jump around Richard (he would be pursing), and end by checking in with Skipper to see if the stern breast line needed attention. Thus, by the time I am pulling corks on the stern, the bustle has subsided and transformed into steady, relatively large time scale jobs.

This bustling, rush period was very marked. It was bounded on both ends by periods of relative calm when I would gander about the soak in observations of everything. It reminds me of the gleam in Frank's and Skipper's eyes when they told me about seining teamwork. During this rush period, I got an incredibly strong feeling of being part of a human clockwork.

(E) As mentioned above (D), once I begin pulling corks up on the stern, I start calming down from the big rush. I watch for signs of fish in the set and tell the others (Darrell and Richard could not look up easily themselves because they were watching their coils of purse line). My mind wanders from the immediacy of what I am doing, and on the whole, it is similar to plunging except that it is harder work, there is no one free to chat with, and I have to pay a bit more attention to what the seine is doing.

I think most of my general understanding of seining occurred to me while I pulled corks, because here I had the chance to watch the mechanics of how we handled the seine, what Frank did to keep the boat clear of the seine, etc. I might joke around with the Skipper if he were in a good mood. Supposing Sk'eg had finished pulling up the forward breast line before the figure-8's on the purse lines had come up, he and I would do a

version of team pulling on the corks that amused Richard and Darrell whenever they saw us doing it. They thought we looked like we were dancing some sort of Latin number.

I could tell fairly accurately how much Richard and Darrell had pursed in from how much of the cork line I had pulled up on the stern. This sort of observation helped me to figure out the action of the seine in the water. But in case I was off schedule, when the figure-8's came through the davit pulleys, I knew it was time to get ready to throw the cork pile back into the water and begin to get ready for the second, minor rush.

(F) When the rings come up, the second rush begins. When I say "rush," it is very appropriate for it is accurate not only in hip, jive-talk meaning, but also in literal, physical meaning. This second one is, however, less strong than the first, when the skiff came in. I throw the corks overboard, put on my jellyfish goggles, rainhat, and wristers, then scurry over to help push the ring lines (bridal lines) out from the gunwhale so that Richard can twist them around straight.

During this busy period, I always feel a little proud of myself because usually I am standing, waiting for either Skipper or Darrell to take the guide line from me (a line used to start the seine over the power block), hence I am ahead of them. The fact that I am almost invariably ahead of the others during this period is, I suppose, one contributing factor as to why I think of this period as only a minor rush, not on a par with the first. I get to watch some of the work the others are doing at this time, but not everything. By now, my mind is thinking ahead to hauling in the seine and getting psyched up for it. Hauling seine is usually on my mind from the moment we let go the skiff until we actually begin hauling, with the possible exceptions of periodic moments plunging and especially during the first rush period. In a way, I dread hauling seine; in a different way, I welcome the opportunity to get better at it.

(G) Sk'eg and I straighten the boat-end's breast line along the deck board just before we start hauling. Richard, our web man, is almost always a bit late getting from mid deck back to the back deck where we pile the seine. As we waited just a moment for him, I try to mentally prepare myself for the highpoint of making a set (in terms of the work)--hauling gear.

I plan a strategy for piling the corks so that it will be both more asthetic and physically easier. I would hope that Richard and I will not pull against each other. I hope the jelly would not be too bad. Then, when Richard joins us, Skipper starts the seine coming over the power block. I always feel that the whole crew on deck sort of stops for moment, takes a breath, and silently says in unison, "Let's go," just as the power block commences pulling the seine up out of the water and dropping it down to us underneath.

The three of us who hauled gear (Sk'eg, Richard, and me) finally managed to train Skipper to begin slowly and build up to speed. Each of us wanted this so that we could get at least a good foundation for our respective piles.

As I laid down the first level of corks, I would try to get my muscles going with my eyes, which were glued to the cork line on deck and just ahead of the actual pile. I would feel driven to get that first layer very smooth and tightly piled, I do not know why. The first layer is done differently than others, at least for most cork men. It is usually laid down in rows, whereas other layers go in various kinds of loops. I knew I would be standing on the first layer for quite a while, and the more even it was, the less trouble I would have keeping my balance. But there was more than this involved in my desire to get it just right.

When I got to slightly differently colored corks, I knew it was time to change piling techniques to my "horseshoe" pattern. No one ever pointed out this change point to me, rather it just came to be my personal marker based on all previous piles I had made. By this time, the power block would be going normal hauling speed (later part of season, this was full speed). I would step up onto the first layer of corks and begin laying down the all important first few horseshoe layers. If these were put down right, I knew that the whole pile would look pretty good even if I screwed up later. By this stage, I would be completely absorbed in my work. In fact, I was unable to look up from the corks until almost the last opening. By the third opening, I had managed to whistle, then sing, and finally talk. But until the third opening I had been so engrossed in the job at hand that I had found it very difficult even to say "STOP!" to the Skipper if I lost my cork line. During the third opening, however, I managed to whistle "Girl from Ipanima" while piling corks, and from that time on, the work was much, much easier. Later that opening, I was babbling various "Sermons from Mt. Cork, by Brother John" to the crew's amusement. So, there was a tremendous jump in my ability as cork man during that third opening.

Richard and I would usually be tugging at the webbing. This would completely tire my arm and shoulder (right if set was to port, left if set was to starboard). Eventually, we got some communications down, and I learned some of the fine points of piling corks from Frank (who had done them for sixteen years). Even then, however, we would occasionally have trouble. I would watch helplessly as one of my loops of cork line would be pulled right back into the middle, or I would have a good plan in mind to throw one large loop on the outside edge of the pile only to find I had no slack, and the loop would land around my feet. So, I began to say, "Going outside," to Richard whenever I wanted more slack. This technique was working reasonably well, but then Frank told me that a <u>good</u> cork man never has to say anything to his web man. Well, that did it! I determined that I was not going to say anything to Richard, and besides, Frank had told me how to get my own slack by grabbing the web a little to the side of the corks rather than the cork line itself.

By the middle of the pile, I would have to change positions. This was always difficult for the corks are very slippery and the webbing catches one's feet easily. I knew I must make this mid-way transition else my pile would look sloppy. I was perhaps over-driven by my asthetic sense because I was under more internally generated pressure to make a good pile than I am in classes or seminars to appear bright. But, the

crew seemed to enjoy my drive and think better of me for it. Besides, there was not much I could do, I just seem to be built that way.

By the middle of the pile, I would be very uncomfortable, both physically and mentally. The physical duress was from overheating inside my raingear, jellyfish on my face or hands, and muscle fatigue in my shoulders. The mental turmoil derived from the strength of my desire to make perfect cork piles in conjunction with my inability to do them. Memory of these conditions contributed to the sense of dread I had of hauling seine. In addition, I could never really remember exactly what hauling seine was like, and I would be a little amazed each time by its peculiarities. On the other hand, I really liked the complete teamwork it required from Sk'eg, Richard, and me. It was our chance to show Skipper, Frank, and the other boats how good a crew we were. It was indeed the emotional highpoint of a set. For example, I was more completely absorbed in the immediacy of my work here than during any other phase. My mind did not wander at all. Even after I realized that I should perhaps know what other people were doing during this phase, I found it very hard to observe anyone else, except maybe Skipper, who was more or less directly in front of me. I never saw Sk'eg piling leads, and I barely saw Richard piling web though he was maybe four feet from me.

As I would make my last few extra-large loops with the last twenty fathoms or so, I felt that everything was downhill and I could think about other things. I would begin to wonder how many fish our efforts had caught and look forward to taking off my rain gear. I also would want to see what my cork pile looked like, whether those couple of loops that dropped down really had loused up the pile.

(H) I was the furthest away from the bag of fish, so I usually guessed how many there were by the amount of noise they made flapping the water and side of the boat. This is a completely beautiful sound. Other indicators were the amount of noise Sk'eg made as he peered down into the money bag from his side of the boat, the breadth of Skipper's smile, how agitated Darrell was, etc.[12] I always felt somewhat strange, however, as the fish came up because we had already put in all the work, and it was the same work whether we caught ten fish or a thousand. So, for me, the actual catch was anticlimactic, even though it is supposed to be the climax. If we had a lot of fish, it was "easy money;" but if there were only a few, oh well.

From the time we had to stop and get the fish on the deck until we finished hauling the last few fathoms of seine over the block, I was always impatient. I could not take off my rain gear until we had gotten the seine piled and the skiff tied up, thus I was in a big hurry and

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[12] My first season, I did not get as involved in the size of the catch as the others. I think this was due to my double interest in the season--money and dissertation. My second and third seasons, however, in spite of their relevance to this dissertation, were much more "normal" in the sense of being very interested in the catch.

agitated with these jobs. I wanted to wipe the jelly off my face, get cooled down, get a smoke, and clean off the mid deck. The event was over and I wanted to get comfortable.

If there were a lot of fish, I would be very happy. There were also pleasant and oft-repeated jokes about the little fish which always seem to splash the most jelly on you as you pitch them into the hold--the expression was "trouble-makers first." Frank would have rejoined us before we began pitching, and in a subtle way, it was always good to have him back.

Before pitching the fish, however, we would get everything set up for the next set. After this and once the fish had been pitched, we would sometimes play "deck hockey" with small jellyfish as we cleaned the deck.

By the time, I was usually curious about how fast we had done the set and would ask Frank how we did (he often timed us as he sat in the skiff). I would also manicure my cork pile, tucking in loose loops and jumping on uneven spots, and this amused everyone and became a standard joke.

Returning to Figure 6 itself, it must be emphasized that it pertains specifically to seining on the Glenda Lou in the 1975 and 1976 seasons of work. During those seasons, there was a slight modification in the style of seining. This change occurred after the fourth opening of 1975, and it involved running the purse line over the power block along with the rest of the seine instead of detaching it and later threading it through the rings on a ring bar. I have drawn Figure 6 primarily as a Toy 2 Series representation of my 1975 season, and from that season, the seining style that stands out in my mind as the "normal" way was that which used a ring bar and did not run the purse line over the power block. But, I have drawn wavy lines around "jobs" and between "jobs" to indicate what aspects of Figure 6 are affected by the change in seining style. Hopefully, these indications will provide information on the order of magnitude of change involved in changing styles of seining. The most dramatic differences would be those obtaining between operations which "full-purse" and use a ring bar (the most "traditional" way according to

seiners) and "half-pursing" operations. In these, the seines are built slightly differently, and the work itself changes; but all in all, they are recognizably "seining" and not different modes of fishing.

There is a benefit in actually constructing formal representations after their fundamental logic has been decided. This benefit results from all the little points where the general logic, which seemed so reasonable in principle, fails to capture the nature of the beast it hopes to describe.

As a first example of this failure of the Toy 2 Series, I should like to examine the basic ambiguity embedded in R_1. As it was defined on page 162 above, it sounds quite simple and straightforward. When trying to apply it to the data, however, I found that there was not such an easy, natural fit as I had a priori imagined. The source of this problem is located in the phrasing, "Job_i must be completed before Job_j can begin." In actuality, it is rather hard to understand when something must be done. The result of this difficulty is that R_1 should be regarded as the relation of elementary sequencing, but the sequencing is based upon a gradation of adverse consequences rather than upon the "simple" principle of must. Thus, R_1 can be broken down into three relations (or as many as one wants) which take different ranges of severity on a bad consequences gradation:

R_1--physically impossible to begin Job_j before Job_i is completed (bad consequences would be upsetting all our ideas of nature and physical laws);

R_1'--failure to wait for Job_i's completion before beginning Job_j will cause a bad foul up, additional work, and a deviation from normal conditions, i.e., an emergency situation;

R_1''--although not absolutely necessary to wait for Job_i's completion before beginning Job_j, it is a much better idea to do so, not because a severe problem will ensue otherwise, but rather because it will be safer, easier, and make more sense.

A second failure of R_1 is that it does not live up to the hopes that it could describe the process of seining well enough to disallow bad foul-ups in its "on paper" simulation. Specifically, R_1, as defined, assumes that the length of time taken between "jobs" is of no consequence with respect to the dynamic system being described. Graphically, this means that the length of R_1 arrows is irrelevant, they are completely elastic and stretch or contract without resistance. So long as the "jobs" get done in their proper sequential chains, the R_1 simulation is supposed to assure no problems. As formal objects, "jobs" and their R_1 relations succeed in modelling this, but if one instantiates behaviors for some "jobs" then there are some points at which major foul-ups are possible within the constraints of Toy 2. For example, in Figure 6, after the purse lines have been pulled in and tied to the niggerheads, the power winch should be stopped as soon as possible else the friction of the turning drums will melt the ropes and damage them. If the R_1 arrow between these "jobs" were really elastic (as they are by their definitions), in time the purse lines would break and the rings would fall back into the water. Therefore, not only must Job_i be completed before Job_j

can begin, but also Job_j must be done soon after Job_i--a condition not addressed in R_1.

While obviously the case that one could point out many and profound ways in which the R_1 relation on the set of "jobs" does not capture significant aspects of the operation of making a set, those simulation deficiencies are of a different order than the business about non-elastic arrows. Problems of duration and general coordination of work are explicitly outside the scope of Toy 2 Series as constituted, but R_1 arrows are definitely supposed to be elastic (in ordinal-time), yet as shown above, at least some of them cannot be and still fulfill the goal of the simulation (to model trouble-free operation in ordinal-time). Of course, this variable of "elasticity" could be captured by proposing a new relation amongst "jobs" and adding it to the existing Toy 2, and in a moment, I shall propose two additional relations, each designed for specific problems, to illustrate ways in which Toy 2 can become richer. The problem of non-elastic arrows is, however, of special interest because it strikes at the very heart of the Toy 2 Series: ordinal-time is too weak a representational mode to consistently describe the dynamics of the data base. This criticism of R_1 simulation is not merely a criticism of omission, but rather of a genuine failure to fit the data. The example has shown that even if one wanted to drag out the operation of making a set to last all day, certain sequential relations cannot be stretched out in real-time else the operation will come to a halt.

Even staying within the bounds of an ordinal-time representation, R_1 is by itself insufficient to accurately model making a set. To patch

up its shortcomings (not distortions), I define two additional relations obtaining amongst "jobs." R_2 is defined as a relation in which Job_i must have proceeded up to some stage short of its completion before Job_j can begin, but Job_i does not terminate or halt before Job_j begins. R_3 is defined as a relation in which Job_x and Job_y must be coordinated in their respective times of performance, such as finishing at the same time or initiating at the same times.

Both of these additional relations amongst "jobs" suffer from the sort of ambiguity which prompted sorting R_1 into three intensities. Again, the difficulty turns on the judgment of what is necessary as opposed to what avoids major problems as opposed to what is a good idea.

In Figure 6, I have illustrated R_2 and R_3 only in select cases rather than exhaustively incorporating them. For some purposes it would indeed be required to compose a Toy 2 which fully diagramed R_1, R_2, and R_3, but here it is sufficient to do R_1 fully and demonstrate the possibility of additions.

R_2 is close to the normal language idea of "monitoring someone else's work until they have gotten to a point where I can begin mine." A good example of the R_2 relation is waiting for the forward purse line to take over the tension of the skiff's end of the seine before popping the pelican release on the main skiff tow line. Releasing the main skiff tow line does not affect the job of pursing in the forward purse line, but if it were done before the purse line were wrapped around the winch and thus holding the tension of the seine, the whole operation would be in jeopardy because the end of the seine would drift away from the boat.

An example of R_3 is the coordination required between steering the skiff back to the boat and steering the boat while the skiff is coming back. Both "jobs" involve attempts to coordinate their enactments with the other's. In general terms, there must be some kind of communication between whomever does the "jobs," not for the general goal of speeding up the whole operation, but because the coordination is required if the operation is to work at all. It should be clear that R_3 can hold only between "jobs" not already in a R_1 or R_2 relationship, for it is a relation between parallel happenings rather than sequential or partially sequential work.

As a last remark on the relations imposed on the set of "jobs," I want to draw attention to the fact that R_1 is not at all the same as "function" or "purpose." There is a superficial similarity in some cases. For example, the purpose of opening the davit pulleys while a set is being held open is so that the tow lines (hence purse lines) can be put through them in order to purse up, and there is a R_1 relation between these jobs. Cases such as this support the illustion of R_1 being similar to "purpose," but it is not. For example, the purpose of pulling corks onto the stern (during the pursing phase) is not so that they can be thrown back in the water later, rather it is done to keep the web from getting tangled on the rudder or propellor of the boat--a fact not encompassed in Figure 6.

I should like to finish this section on the Toy 2 Series with discussions of job categories. In the series, these categorizations are irrelevant, for all "jobs" are regarded as equivalent formal objects with

the single exception of "hardware states" jobs (discussed above). Such a single-tiered view of the work is permissible for many problems or uses, but it is erroneous and misleading if the goal is to understand fully the nature of seining.

The first three categorizations of "jobs" entail subtle and/or obvious violations of Toy 2 Series general logic. The fourth does not.

Some of the "jobs" shown in Figure 6 are of a kind that could be called keeping things in order. A job such as tying a line to the rigging pin after its use is finished is certainly not a must, but the line would perhaps get in the way otherwise. So, by identifying such work as a "job," I acknowledge the general importance of orderliness for the whole of the operation. But such work is not a case of absolutely necessary human-caused movement of hardware.

Another category of "jobs" is holding jobs. Here the nature of the work is to maintain and hold tension on a line. These jobs take two forms: either one ties the line around some hardware, or he holds it with his hands. Whenever there is a lot of tension on the line, it will have to be tied to hardware. This has the benefit of freeing a crew member to do something else while the hardward and knot "holds" that line, but it is in general slower because it takes more time to tie and untie a line than to grab it and let it go. This category of "jobs" violates Toy 2 Series logic in that the human factor enters into consideration two ways: (1) The "job" of tying $line_x$ to a cleat may be necessary only because an average human is not strong enough to hold the line himself, and (2) In some cases, even though a human could hold a line, it will be

tied off so that the person will be freed to do other "jobs." Thus, these "holding" jobs arise not because they are must hardware movements in principle, but because there are a fixed and small number of people to do the work of seining, and they are of limited strength. That is, "holding" jobs derive from conditions imposed by the human component of seining. If there were thirty people on a crew, many holding jobs would modify. But, these factors are supposed to be ignored in constructing Toy 2's.

A third categorization of "jobs" is time-saving jobs. Paradoxically, time-saving jobs are simply extra work. For example, there is no reason to untie the line over the power block (the guide line for the seine) from its rigging pin and use the ring bar to hold it down on the backdeck. All of this is needless work seen from strict Toy 2 Series thinking because it could be left tied to the rigging until needed. But, if one considers that the plunging phase is comparatively non-rushed, and this extra work is no great burden then whereas it is difficult to get to the rigging during the time range the particular line is needed, then the extra work makes good sense because it saves time by not further congesting an already congested time period. Basically, extra, preparatory work is done in less dense[13] time periods so that later, in more dense time periods, equipment will be near at hand and spatial congestion can be averted. The violations of Toy 2 Series logic here are blatant and many. Again, the genesis of such "jobs" is to be found in the size of

[13]"Density" here means the ratio of "jobs" per unit of time.

the crew relative to the number of jobs to be done in a given time range, hence only by considering the facts of crew make-up as this is superimposed on basic mechanical musts are these "time-saving" jobs generated and identified. This violates the very idea of the Toy 2 Series, which was to model making a set without taking into account the people who do the work.

The last categorization of "jobs" distinguishes between dominant and satellite jobs. A dominant job is a major activity around which other things are done. The following are individual dominant jobs: steering the boat, steering the skiff, pursing, and hauling gear. Subsets of the set of "jobs" are collectively dominant in a couple of cases--lifting the rings out of the water and hoisting the fish on deck are such subsets, each having several constituent "jobs." On the other side, there are satellite jobs which are less important than the dominant ones during their respective time periods. Satellite jobs are affected by emergencies in their dominant counterparts, but the reverse is not equally true, that is, satellite jobs can be interrupted or perhaps even left undone and the whole operation can continue. A good example of a satellite job is pulling the corks onto the stern. If anything should go amiss with the pursing or if the breast lines are tangled, the person pulling corks stops and helps rectify the problem affecting the dominant job because pulling the corks is a low priority job and sets can be made without doing it.

The distinction between dominant and satellite merely draws attention to the feeling that some jobs are more important than others, but

not necessarily in the sense of if left undone the operation would halt. Even "little things" can stall or halt further work, that is, break R_1 chains. Dominant jobs are felt to be more important because they are somehow directly responsible for major manipulations of the seine, whereas satellite jobs merely accompany, prepare for, or tactically make possible the enactments of their dominant centers. Thus, some jobs are special much in the way the featured instrument is special in a concerto.

I do not think dominant and satellite partition the set of "jobs." Not every job will be either one or the other. For example, getting the cork line tied to the bow is regarded as a very high priority job, especially in rough weather conditions. Its purpose is to keep the bunt end of the seine from getting fouled and twisted so that the fish will end up in the "money bag" all together. Skippers are very concerned that the "jobs" involved in getting the cork line tied to the bow be done very quickly after receiving the skiff's end of the seine. In these respects, this subset of "jobs" appears to be dominant. But, this small "job" chain has no satellite jobs of its own; it does not relate holistically to a major phase of the operation; the attention of crew members other than the person doing the work is not partially diverted to check its progress; and even though rather important, it could be neglected and not completely stop the operation (i.e., the R_1 arrows emitting from it in Figure 6 are of R_1' or R_1'' intensity). It does not fit easily into either category, and hence illustrates my feeling that "dominant" and "satellite" is not simply a pigeonholing dichotomy. Rather the distinction is a subtle, intuitive feeling about jobs, stronger in some cases

than others. It does not violate Toy 2 Series thinking because it transects the set of "jobs" and provides additional information about them rather than producing-identifying more "jobs." The previous three categorizations of "jobs" all named "jobs" of peculiar genesis, but this dominant/satellite dimension applies to "jobs" or not irrespective of their mode of being "jobs."

The motivation behind Toy 2 Series is twofold. First, it is intended to be a way of describing ethnographic data. Second, it is to be a kind of initial simulation formalism. These two objectives have collided in my efforts to produce Figure 6, where I had imagined they would peacefully co-exist. Strict Toy 2 Series thinking would not describe my experiences on the Glenda Lou as well as I want to do. It would leave out several "jobs" that definitely were part of the work on the boat. Hence, the resultant (Figure 6) is not a "clean" illustration of the Toy 2 Series. I have had to deviate from my original method of identifying "jobs;" there have been instances of flat failure of the formalism (e.g., non-elastic arrows); and additional relations have been necessary. Fundamentally, the deviations in Figure 6 derive from the ethnographic impulse, which, of course, entails certain contaminating considerations of the crew who do the work rather than concentrating purely upon hardward movements. In Section 3.5, I shall return to this subject and discuss more fully and reflectively the mentality of the Toy 2 Series--its uses and shortcomings as well as the frame of mind which sees seining in such a fashion.

Using Figure 6 for what it contains in ethnographic information rather than as a formalism, the reader is once again asked to ponder "making a set." The photographs of fishing boats and their hardward on pages 101-114 may assist in this imaginative and synthesizing effort. The work should appear as a beautifully orchestrated and sensitively controlled group operation within the confines of a very efficiently utilized space. In spite of the complexity, it is yet a "simple" process, once one knows how it all comes together to manipulate the seine in the water. It all boils down to putting the net in the water, trapping some fish by encirclement, completing the entrapment by closing the bottom of the net, and then hauling in the gear and fish. It happens on an average of six to ten times per day per boat while seining.

3.4 Outline of an Operations Research Problem

The fundamental concern of the previous two sections, in addition to being ethnographic, has been what gets done, when, and by whom. Obviously, the techniques, modified to suit specifics, would apply to any kind of sequential behaviors, whether of individuals or groups of individuals. For example, the basic approach could be applied to bartending, blacksmithing, football, marriage ceremonies, and on and on. All that is required of the data is that they consist in easily segmented actions, episodes, or behaviors, that the ordinal-time relations amongst these segments are known, and that the segments can be associated with distinct actors (body parts, whole persons, sub-groups of persons) in some pairwise relation. The formal notions are exceedingly general and

powerful, and for this reason do not have much in the way of explanatory adequacy (Chomsky 1970). But as a condensed mode of conveying complicated and abundant data, as well as for some particular applications, the basic approach has much to recommend it.

In this section, I want to outline one application for the Toy 1 and Toy 2 Series. Used conjointly, these Series provide for the asking of certain questions regarding crew organization--its creation (job assignments) and efficiency. Since the point here is to demonstrate potential utility, I feel no compulsion to follow through with this project. Rather I intend to merely show the application in principle.

There are two somewhat related questions that occurred to me while I was fishing on the Glenda Lou. I slowly began to wonder whether there might not be some better way to divide up the jobs. By "better," I had in mind two senses: an organization that would perhaps shorten our total time to make a set, and an organization that would make the work easier for the crew as a whole by having the people best at certain jobs do them. Essentially, what I wondered about was whether our naturally evolved system of job allocations was better or worse than other possible arrangements. Answering this to my scientific satisfaction requires some sort of numerical evaluation and comparison. If one modified the Toy 1 Series such that its "jobs" were conceived after the fashion of Toy 2 Series "jobs," then these formalisms could be used to calculate just the sort of numerical evaluations of a given crew organization that is needed,

provided one additional kind of information is included. But, before jumping into how this could be done, I would like to set up the problems a bit clearer.

As a very crude first approximation of the nature of these problems (for convenience, call them FAST and EASY, respectively), imagine that all the "jobs" that will have to be done are represented as glass beads with labels attached. These beads could be dumped on a table, then six people (the working crew) would be instructed to put beads on strings with the idea of having strings of about equal length. The result would be that each person would keep putting beads on his string until all the beads were gone and each person's string of beads is about as long as everyone else's. This would be a fine way of assigning "jobs" were it not for two fundamental problems: (1) The "jobs" must be done in proper order (as shown in Figure 6), and (2) Some "jobs" take longer than others.

Bearing in mind the flaws of the first analogy, let us this second time assume that each "job" is again a labelled bead, but this time the size of the bead (length) is directly proportional to the average time the "job" takes to perform. Furthermore, instead of simply dumping the beads onto a table, let us imagine that they are laid out going from one end of the table to the other in accordance with the sequential relationships described in their Toy 2 Series structuring. Now, we give each crew member a needle and thread and once again instruct him to make a string of beads comparable in total length with everyone else's, but this time they should all begin at one end of the table and move to its other end. This second analogy is much better than the first, but it still

allows for the quite unreasonable possibility that the six crew members would merely divide the whole table into six equal areas (in terms of total length of the beads within that area) and then each person take all the beads within one of the areas. In the context of actual fishing rather than the analogy with beads, this would mean that the six man crew would have only one person working at any given time (doing all the work in that time period) and everyone else would be idle until his respective turn came along. This absurd possibility tells us that further constraints are needed with respect to the permissible ways beads can be strung.

Without continuing the bead analogy further, we can turn to the work of seining itself and phrase the overall objectives or goals of any division of labor. As mentioned above, there are two basic goals: to make the entire operation go as rapidly as possible (FAST), and also, to make the work as efficient as possible in terms of maximizing crew competencies (EASY). I see no way to solve these two goals simultaneously, and so I shall deal with FAST first.

As was seen in the second bead analogy, it is necessary to associate each "job" with a duration value. These data could be obtained from very detailed and rich recording techniques (e.g., video-taping) applied to a sample of sets. The duration values for each "job" could then be extracted and the distribution plotted. Given that the form of these distributions will tend to be a unimodal right-hand screwed curve on a frequency/time graph, it immediately becomes problematic as to which of the usual three

central tendency measures should be used. Of the three--mode, median, and mean--I would select the mean for use as a invariant "duration value" to characterize the temporality of a "job."[14] Using the mode would be compatible with the general emphasis upon the "ideal," but one would want to have some accommodation with the less than perfect, and the mean (average) duration seems to catch this inclination a bit better than the other two. This whole business may sound remarkably like hair-splitting, but if one were to actually perform subsequent computations, significant differences would result depending on the method of assigning "duration values." Once a method for establishing duration values has been selected, each "job" can be thought of as a time-chunk, and we may state some general conditions or constraints on the operation.

The first condition is that there should never be a time during which no "job" or "jobs" are being done. Following from this, we can calculate that the worst possible crew organization is one which has but one "job" being done at any given time.

The second general condition is that we assume (rather accurately) that any given crew member can do only one "job" at any given time. This does not say that he must work all the time, rather that when he is working, he can only do one thing at a time.

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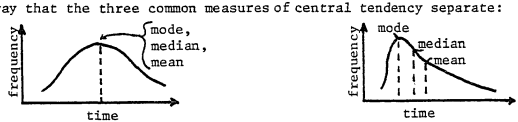
[14] In a unimodal, symmetrical curve, the mode, median, and mean coincide. But in most kinds of "jobs," foul-ups skew this sort of curve in such a way that the three common measures of central tendency separate:



The third condition is that whomever begins a given "job" will see it through to completion. This means that one person cannot start doing a "job," and then sometime in the middle quit and let someone else take over that work. Thus, "jobs" are indivisible unities with respect to job allocation.

The fourth and last ground rule is that each job routine should be as equal in work-time as possible to the other job routines. This matches the normal sense of "fairness," which is based on the logic that since each crew member receives the same pay (remember the share system), he should do as much work as everyone else. Obviously, there are exceptions to this general rule if one takes into account the variable of energy expenditure per unit of time relative to specific "jobs." It is much more difficult work to pile gear than it is to run the power block, for example, but for the FAST problem, it is not pertinent that some "jobs" are more demanding than others. All that counts in this problem is the duration values. (The EASY problem will focus on these complicated non-temporal dimensions of the "jobs.")

The nature of the FAST problem can now be described. The problem is to take all the work that must be done and create as many "job routines" as there are crew members, and do so such that the various "jobs" get done in their proper sequential order. Furthermore, the total average time an organization takes to make a set (T_i) should be as small as it can be. It is assumed that any crew member is capable of doing any "job" and that he can do it in that "job's" mean duration time. Assuming a crew of six, the problem here is simply making six "job tracks"

out of Figure 6. The numerical value of T_i is the means of evaluating a given crew organization$_i$.

T_i could be treated as an independent variable whose value would be established by simply taking the mean from a sample of empirical observations of a given crew organization making sets. This short-cut to a solution has its appeal, but there would be no guarantee that conditions two or three (above) would be met by a working seining crew. Hence, T_i would not arise in the formalism which provides the standards of evaluation (coming up below), and thus it is not really meaningful if treated as an independent variable. The power of this whole mode of evaluating crew organization (using Toy 1 and Toy 2 Series) lies precisely in its analytical, deductive control--it can circumvent the contamination of externally caused contingencies (delays), which are part and parcel of real seining, and focus directly upon crew organization in principle. Therefore, T_i must be generated deductively within the formalism; it must be a dependent variable.

From what has been said above, we can state the procedure for evaluating any given crew organization. To begin, let us define some symbols.

J : the set of all "jobs" in a given Toy 2 Series example ($j_i \varepsilon J$)

D : the set of mean duration values ($d_i \varepsilon D$)

$J \leftrightarrow D : \{(j_1,d_1),(j_2,d_2),\ldots,(j_n,d_n)\} = JD$

R_1 : the relation of elementary sequencing in (JD) x (JD)

R_2 : the relation of partial sequencing in (JD) x (JD)[15]

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[15]Partial sequencing really screws up everything that follows, and since for temporal computations the longest times to get something done are most significant, I shall regard all instances of partial sequencing as being elementary sequencing.

P : the set of all paths in R x R ($p_i \varepsilon P$)

C : the set of crew members on a given boat ($c_i \varepsilon C$)

The longest time to make a set (staying in agreement with the three general conditions) can be calculated according to the following formula

$$(1) \qquad \max.\ T = \sum_{i=1}^{n} d_i = MAX, \text{ where } (j_i, d_i) \varepsilon JD$$

Any crew organization ($C.O._i$) whose T_i equals max. T is definitely a very bad arrangement, in fact, it is the worst it can be. So, MAX is the upper limit for time to make a set and could be achieved only if each "job" were done one at a time (this is what simply summing the duration values for all the "jobs" implies).

Calculating the converse of the worst T is much more intricate. It would be easy to calculate if one could ignore sequencing and have as many crew members as there are "jobs." Then, the minimum value of would be simply the duration of the longest "job."

$$(2) \qquad \min.\ T = \max.\ d_i, \text{ where}$$
$$(c_n, (j_n, d_n)) \varepsilon C \times (JD).$$

But, there is very definite sequencing, and there are only a small number of crew members (let us say six), therefore finding the min. T is much more difficult.

The real minimum value of T (i.e., MIN) will be equal to the sum of the durations of those "jobs" which are in the critical path of the Toy 2 Series diagram. These "jobs" and their associated duration values constitute the path through the diagram which has zero slack time.

Getting away from the Operations Research jargon, this means that an operation's flow cannot go any faster than its longest sequential chain, and in that chain, there is no time during which one of the constituent "jobs" is not being done. One identifies the critical path by inspection of the data (already in time-chunk flow charts); there is no algorithm. The formula for the minimum value of T is:

$$(3) \qquad \min.\ T = \max. \sum_{i=1}^{n} d_{x_i} = MIN, \text{ where}$$

$$p^* = (j_{x_1}, d_{x_1}), (j_{x_2}, d_{x_2}), \ldots, (j_{x_n}, d_{x_n})$$

and

p^* is the critical path.

The better the crew organization, the closer its T_i will approximate MIN. In principle, it may be possible that more than one organization could have a T-value equally close or even equal to MIN. In general, the following is true:

$$(4) \qquad T_q - MIN = a$$

$$T_r - MIN = b$$

if $a < b$, then $C.O._q$ is better than $C.O._r$, but

if $b < a$, then $C.O._r$ is better than $C.O._q$.

Thus far, we have been able to use the Toy 2 Series to formulate a mode of evaluating respective crew organizations provided that the crew organizations are constrained by the same Toy 2 Series. At this point, however, values for $T_i \ldots T_k$ could be instantiated in the evaluation procedure regardless of whether they be empirically or analytically

derived. That is, Toy 2 Series has been used to formulate MIN (critical path analysis of a given Toy 2 example with duration data would provide actual numerical value for a given MIN), but T-values have no specified mode of generation. As argued above (page 187), a way of computing T-values within the formalism itself must be found if we are to make the whole method internally resonant and powerful.

One might erroneously propose that comparison of the longest "job routine" with MIN constitutes a good evaluation of crew organization. As defined for Toy 1 Series usage, "job routine" is of necessity equal in duration value with the T_i of the crew organization of which it is a part. Such tautological conceptualization does not bring us closer to internally generating T-values, however. If "job routine" is given a Toy 2 Series interpretation, then it begins to be pertinent.

Let us define a set, W, having the same cardinality as the set C. This new set is the set of "work tracks," and there is one of them for each crew member. In each w_i it is possible to compute the sum of duration values of all the "jobs" in it. This sum is the work-time of that "job routine." Here, it may occur to the reader that the w_i with the greatest work-time value equals the desired T_i, after the fashion of a critical path. Hence, we could go from calculation of the work-time of each w_i to a w* and the following formula for crew evaluation:

(5) $|MIN - w^*| = a$

and the closer a is to 0, the better the C.O.

In a peculiar way, this idea of having one work track's total work-time equal MIN points toward a possible solution of FAST. That is, if

one crew member were assigned as his "job routine" (his w_i) the critical path of the Toy 2 Series diagram for his boat, then indeed such an organization could solve the FAST problem, provided his is also the longest work track (w*). This is so because in such a case, the w* would equal MIN, and w* would be identical with p*, which is already known to be the longest path through the whole sequencing of "jobs."

Unfortunately, this solution violates general condition four (p. 186 above) in a fairly obvious way. This poor guy would be busy for the whole of the set whereas everyone else would have periods of slack. Also, this kind of organization would be physically impossible since it would require that this special crew member be able to teleport himself in space without time loss and other completely miraculous doings. Staying within the confines of the formalism, however, the pertinent objection is that the so-called solution violates general condition four.

It is clear then that despite its initial appeal, the set W cannot be used by itself to compute T-values. The sorts of calculations upon it are significant for ensuring that the overall "solution" agrees with condition four, and this is important, but the set does not take us far towards internally establishing T_i. General condition four tells us that everyone should do roughly the same time-quantity of work, but in itself says nothing about whether these equal stints should be done sequentially or simultaneously. Condition four is summarized in (6):

(6) $$w_i \approx \frac{\text{MAX}}{\text{crew size}} < \text{MIN}$$

and

$w^* < MIN$, where $w^* = \max. \sum_{i=1}^{n} d_{x_1}$,

$$w_x = (j_{x_1}, d_{x_1}), (j_{x_2}, d_{x_2}), \ldots, (j_{x_n}, d_{x_n}).$$

The basic problem of FAST can be phrased as finding a way (or ways) to make T_i remain equal to MIN but at the same time equalizing $w_1 \ldots w_n$. Since in the case of seining, MIN is greater than $\frac{\text{MAX}}{\text{crew size}}$, the general solution strategy should be to parcel out amongst the crew the various constituent critical path "jobs," but to do so without causing any elongations in this path. That is, the overall objective of a crew organization is to keep the Toy 2 Series critical path flowing smoothly without delays caused by intrusions of the other "jobs," and at the same time, let the various crew members carry the ball at different times so that their total work-times will be comparable. This goal of all crew organizations is also a method for generating T_i insofar as it holds T_i equal to MIN and then demands an equitable parcelling out of the "jobs."

Figure 7 illustrates a portion of a hypothetical crew organization which is successfully managing this general goal. In terms of the equity issue, the greatest difference is between w_2 and w_6, but this may balance out looking at the whole set rather than just an isolated portion. With respect to the issue of keeping p* flowing without intrusions, the hypothetical organization shown in Figure 7 is perfect, there is no time lag between the end of $(j_i, d_i) \varepsilon p^*$ and the beginning of $(j_{i+1}, d_{i+1}) \varepsilon p^*$.

Figure 8 illustrates a portion of a hypothetical crew organization which unnecessarily has elongated p* by poor assignment of "jobs." The

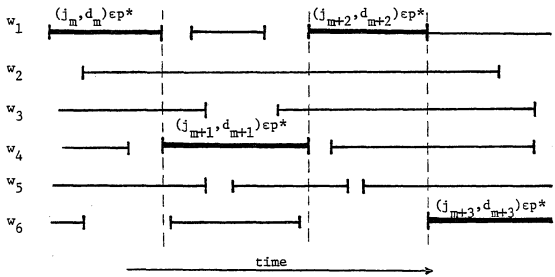

Figure 7. Segment of a (hypothetical) perfect crew organization

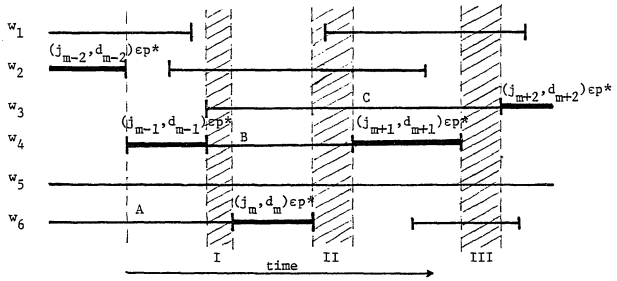

Figure 8. Segment of a (hypothetical) poor crew organization

shaded areas are breaks in p* (the critical path) caused by poor allocation of the critical path's "jobs." For example, both I and II could have been averted if $(j_m,d_m)\epsilon p^*$ had been done by whomever does w_4 and letting job B be done by someone else. Thus, the crew organization partially represented in Figure 8 has unnecessarily elongated, or increased, p*, and its T_i will therefore be greater than MIN.

In Figure 8, shaded area III may arouse suspicion in the reader. Why should it be there? Could not job C merely "slide" backwards and thereby let no time elapse between (j_{m+1},d_{m+1}) and (j_{m+2},d_{m+2})? This is indeed possible and what should happen provided that there is no R_1 relation obtaining between (j_{m+1},d_{m+1}) and job C. But, I have drawn Figure 8 under the assumption that there is an R_1 relation between these two "jobs," and in such a case, job C cannot initiate before the termination of (j_{m+1},d_{m+1}). Therefore, shaded area III represents slack in the performance of the critical path, another way in which non-critical path "jobs" may intrude and cause elongations or higher values of T_i.

Whichever way the critical path becomes elongated (after the fashion of I and II or of III), the cause is poor organization of the work rather than the work itself. And, following this method of focusing upon the critical path, the general formula for computing T-values is:

$$T_i = \text{MIN} + \text{C.O.}_i p^* \text{ "slack"} \qquad (7)$$

That is, the total time for a given crew organization to make a set is the sum of MIN and all those durations when the critical path is made discontinuous owing to the intrusions of other, non-critical path "jobs."

General condition one together with R_1 ensures that these organization-caused slack times can be computed, although perhaps very arduously.

It should be clear that analytically arriving at T-values for crew organizations is an awkward, strenuous, tedious, post facto, inspection method, but nonetheless possible feat. Equations (4) and (6), taken together stepwise, provide the evaluative criteria for the FAST problem. They stand irrespective of whether one wants to arrive at T-values analytically or just let nature produce them in empirical cases. To reiterate, the "best" solution to the FAST problem is a crew organization whose T_i equals MIN and also satisfies $w_i = \frac{MAX}{\text{crew size}}$. There may be several "best" solutions.

The EASY problem is almost orthogonal to the FAST problem, but not completely. This being so, they should be "solved" at the same time with their respective "solutions" being tried out against the constraints of the other problem in an iterative manner. But, it is much simpler to phrase the method for solving EASY if one can assume several solutions to FAST have already been found.

Basically, EASY asks to find the crew organization(s) which maximizes fit amongst personnel and "jobs." Fit is a compound notion, consisting of as many factors as the analyst (or skipper) cares to consider. Normal sorts of variables to consider in evaluating fit on a seine boat are seniority on the boat, amount of authority in the formal social structure of the boat, special skills, proficiency at specific jobs, and so forth.

If EASY is to have numerical evaluations, it will first be necessary to build a formalism which can easily describe fit. Let us set up an n-dimensional joint value space where each dimension is a relevant factor in determining who should do what "jobs." Each crew member $(c_1,c_2,\ldots,c_6)$ would be measured along each variable (or dimension) of this n-tuple with the result being that his "qualifications" would be encoded as follows:

(8) $$c_1(a_{11},a_{12},a_{13},\ldots,a_{1n})$$

or, generally,

$$c_x(a_{x1},a_{x2},a_{x3},\ldots,a_{xn})$$

In a similar fashion, each "job" would be evaluated in terms of the minimum requisites for performance:

(9) $$j_y(b_{y1},b_{y2},b_{y3},\ldots,b_{yn})$$

Since each "job's" n-tuple values are the minimum requisites,[16] it follows that a crew member who does not have at least the minimum value in each dimension could not do that "job." Therefore, the minimum acceptable fit between a given crew member and a given "job" can be determined. Using the notations in equations (8) and (9), the following expressions hold:

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[16] For the moment, we are assuming that all the variables in the n-tuple are ratio scale. This will not likely be the case with the empirically relevant variables, but such considerations pose operational, not in principle problems.

(10.1) $a_{xi} - b_{yi} = 0$ for all i = 1 ... n, then

c_x is minimally acceptable to do j_y;

(10.2) $a_{xi} - b_{yi} < 0$ for any i = 1 ... n, then

c_x is not qualified to do j_y; and

(10.3) $a_{xi} - b_{yi} > 0$ for any or all i = 1 ... n, then

c_x will be able to do j_y easily, how much so depending on how much greater than 0.

The "best" solution to EASY is clearly the crew organization which assigns crew members to "jobs" for which they are more than qualified, does this most often, and, optionally, does this most equitably. That is, the "best" crew organization is one which assigns its crew to "jobs" so that the sum total of personnel-to-"jobs" comparisons (numerous run-throughs of equation (10)) is as much greater than zero as is possible, and furthermore (optionally), the various crew members all have roughly the same contributions to this total. Should this optional condition be desired, it will be necessary to modify the subscripting notation as follows:

(11.1) $c_x \leftrightarrow w_x$

(11.2) $w_x = j_{x_1}, j_{x_2}, \ldots, j_{x_n}$

(11.3) $\forall j_{x_i}$ in w_x, $j_{x_i}(b_{x_i 1}, b_{x_i 2}, \ldots, b_{x_i n})$.

Given the notational modifications in formulation (11), the following lengthy equations prescribe a computational method for arriving at what I shall call "EASY values," or "FIT values."

$$(12.1)\qquad FIT_{c_x/w_x} = (((a_{x1} - b_{x_1 1}) + (a_{x2} - b_{x_1 2}) + \ldots + (a_{xn} - b_{x_1 n})) + ((a_{x1} - b_{x_2 1}) + (a_{x2} - b_{x_2 2}) + \ldots + (a_{xn} - b_{x_2 n})) + \vdots + ((a_{x1} - b_{x_n 1}) + (a_{x2} - b_{x_n 2}) + \ldots + (a_{xn} - b_{x_n n})))$$

$$(12.2)\qquad FIT_{C.O.} = FIT_{c_1/w_1} + FIT_{c_2/w_2} + \ldots + FIT_{c_n/w_n}$$

Equation (12.1) "compares" the fit of a single crew member, c_x, with his whole "job routine," w_x. Should any one of the terms of this long polynomial be less than zero, then that crew member is not "qualified" to do his routine, and the whole operation would not go to completion (in terms of its formal representations, that is). If, however, the crew member is at least minimally acceptable for his routine at each ith dimension comparison, then his FIT will be greater than or equal to zero. The greater his FIT is, the easier his routine will be for him.

Equation (12.2) computes the fit of the whole crew organization in terms of EASY. The larger this value is (it must be greater than zero else there are cases of people doing "jobs" they are not qualified for), the better "solution" of EASY that crew organization is. The optional condition mentioned earlier--that each crew member find his respective "job routine" comparably easy--is formulated in equation (13) below.

$$(13)\qquad FIT_{c_x/w_x} \approx \frac{FIT_{C.O.}}{\text{crew size}}$$

Before summarizing the accomplishments of this section as a whole, I should like to make some comments concerning the assumptions underlying my proposed way of numerically handling EASY.

The motivation behind EASY, or what it is all about, is to bring within the scope of analytical attention the points that not all crew members are equal and that not all "jobs" require the same skills, knowledge, or experience. This dual system of differences was ignored in dealing with the FAST problem, but it is very much part of actual seining. The basic way of describing such differences was to "score" both "jobs" and personnel in an n-dimensional space. That is, each "job" would receive a number, say from zero to one, on each of the n-dimensions of the space, and similarly for each person. Thus, all the a's and b's in formulas (8-12) would be constants, some particular numbers between zero and one. But, since the "jobs" are scored according to minimum requisites whereas the personnel are scored according to their average ith dimensional qualifications, respectively, one would normally expect that the personnel's qualifications would exceed the requirements.[17] This difference is an index of "over-competency" (hence, how easy the work will be). This was the basic logic of my proposed way of finding a numerical evaluation of crew organizations with respect to EASY.

The problems with the method are immediate and twofold. First, one may well wonder how to operationalize the necessary "scoring" of both "jobs" and people along the various dimensions. How does one decide

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[17] Louie's, from my second year seining, did not.

that Person A should receive a value of "5/8" on variable number thirteen? Why not "$\sqrt{2/3}$"? This problem was probably obvious to the reader several pages ago, but allow me to elaborate certain issues that would come up in the course of trying to find a clever operationalization or scheme of measurements.

One way to make all the variables ratio scale would be to make comparisons amongst the personnel on the boat and amongst the "jobs," respectively. This would dispense with the very improbable idea that "measurements" could be made with some sort of absolute yardstick. In such a case, the most competent crew member in the ith dimension would receive a numerical value of "1" in that dimension and an imaginary buffoon could take "0." The rest of the crew would then receive various fractions indicating how they stack up vis-à-vis these two established points, and so forth. But, such a procedure would invalidate comparisons amongst crews, and everyone seining knows that crews, as wholes, vary with respect to overall competencies. Further attempts could be made to devise a good scoring procedure, but I think the energy expediture would be wasted.

The second fundamental problem with my method for solving EASY is that it ignores the plain fact that people are trainable; they learn and get better. That is, there was the implicit assumption that a crew member could be evaluated, or scored (never mind how), once and for all. There was no discussion of re-scoring each crew member every so often (like after each opening) and then re-calculating the $FIT_{C.O._i}$. In a more direct way, this sort of on-going evaluation is necessary else any crew with a rookie would have trouble assigning him any "jobs."

Aside from these two rather major objections, we may say that the method captures much of what happens in the aspect of the general assignment problem I have been calling EASY. And, the method does, in principle, produce a way to evaluate numerically amongst various crew organizations, if not generate the single "best" one. Thus, a given crew organization's $FIT_{C.O._i}$ together with its T_i fairly well describe most of what I used to ponder about with respect to crew organization as I seined on the Glenda Lou. Together they establish the basic parameters for evaluating the assignment problem inherent in the division of labor on a seine boat.

In summary, this section has devised and outlined procedures for assessing relative merits of possible and alternative divisions of labor on seine boats. The procedures fall into the general bag of tricks associated with Operations Research, although I have made no citations to such literature.

A given Toy 2 Series diagram has been an assumed prerequisite. Also, it would be necessary to make each "job" into a labelled time-chunk or known duration. With such a diagram in hand, it would then be possible to numerically evaluate proposed crew organizations.

The FAST problem of crew assignment assumes that each crew member could do any "job." Crew $organization_i$ is then displayed in Toy 1 Series fashion and checked to see if and how much that organization elongates the critical path of its Toy 2 Series basic constraint. The less elongation, the better that crew organization is with respect to FAST. Amongst

equally fast crew organizations, the one(s) which most equitably divides the "jobs" amongst crew members is the better.

The EASY component of crew assignment uses the FAST problem's outputs as its inputs. The solution strategy is based on some sort of scoring both of personnel and "jobs," and then seeing which amongst several equally fast crew organizations makes maximal use of the crew's various competencies. Amongst equally easy crew organizations, the one(s) which most equitably eases the work is the better.

It is very unlikely that the proposed assessing procedures could ever actually be implemented, but in principle, T_i and $FIT_{C.O._i}$ adequately describe the most significant aspects pertinent for evaluating a given crew organization.

3.5 The Mentality Underlying Toy 1 and Toy 2 Series

This critical section revolves around the seemingly innocent question of whether in the previous portions of this chapter I have described the nature of seining. Without a doubt, the work of seining has been the dominant topic of discourse, but have I described its nature?

As a heuristic, I shall segregate my comments into three partially overlapping categories. The first focus of commentary considers the efforts of this chapter as a simulation exercise. Here I shall point to a few areas where the model (Toy 1 and Toy 2 Series) could be expanded and made richer and also to a couple of problems which seem beyond its bounds. But, in general, these comments do not question the very fundaments of the model, rather they only call attention to how it should be expanded.

The second body of critical comments makes comparisons between the model's logic of construction and the phenomenon of seining as it actually occurs. In a way, the question here is the saliency of the model, or how empirically rich it is. That is, these comments examine the model as an empirical account rather than merely a simulation exercise (though the two are obviously related).

The third and last category of remarks leaps headlong into what appears to be a philosophical red herring concerning what a description should be. The fundamental issue is the range of empirical incorporation. I realize this is unclear at the moment, but I think it will be clarified below with examples.

As a simulation exercise, the first problem area that comes to my mind is the method for identifying both "jobs" and R_1 relations in Toy 2 Series. Apart from the minor issues of phrasing the procedures in natural language, I can attest to the strangeness of the mental state as one tries to construct a concrete Toy 2 Series representation. There are several ingredients to this strangeness, but the one I want to draw attention to here is the almost uncontrolable vacillation from model construction to particular facts and back again. At numerous points while making the flow chart, I found that I had to resort to the way things actually happened on the Glenda Lou in order to continue developing the flow chart. Now this was supposed to happen to the extent that a Toy 2 Series representation is operation specific (empirical with respect to a given boat and crew in some time range), but it should not have happened in order to

decide if something was necessary or not. The principles of model construction should generate that, not the facts themselves. As a result of this mental vacillation, it was nearly impossible to eliminate considations of the crew from Figure 6 although I was trying very hard to do so. Basically, I think the problem is rooted in the fact that nothing in seining has to be done except as dictated by intentions, but these intentions take into account crew size, human strength, cost of better equipment, current repetoire of seining styles, and on and on. The results of this fact in terms of Figure 6 were the gradation in R_1 and the various categories of "jobs" which were discussed as violations of the general logic. I do not see how this problem can be overcome. As a result, the ability to construct Toy 2's is an art which requires training rather than a cut-and-dried, clean method. The final model is tied to the facts directly rather than independently developed, hence it is tautological.

The second simulation problem of the Series is that coordination of the work is not captured. As discussed in Section 3.4, this could be partially remedied and yet stay within the overall goals of the model by including duration values for each "job." However, modelling real-time coordination of the work would have to either assume an ideal coordination or select one particular set as the object for simulation. Given that one of the motivations for building the model in the first place was to evaluate alternative crew organizations, I felt putting general coordination of the work as a simulation goal would have made the constraints too rigid to allow for much latitude with respect to the job assignment problem dealt with in Section 3.4. Furthermore, it was

difficult enough to determine the components of an ideal set in ordinal-time. Nonetheless, both Series focus on sequential aspects of the work rather than parallel processes, but these should be addressed, even if only to the extent of adding duration values to the nodes (as in the modified Toy 2 required for computing T_i). And, as they stand, neither Toy 1 nor Toy 2 Series has any simulation mechanisms for initiating or completing "jobs."

The third major simulation problem of the Series was mentioned earlier: all performance knowledge underlying each "job" has been taken as given. Hence, a natural way to enrich the simulation would be to expand each "node" ("job") to incorporate representations of all the capabilities subsumed under each labelled node.

The last problem area I shall mention here is that both Series are "static" representations compared with the whole seining seasons which they attempt to describe. During a season, the operation evolves, but this is not captured in the Series. If, however, a Toy 1 and Toy 2 representation were made of each set over the course of a season, then such evolution and development would be recorded in a standard time-slices, diachronic form.

If all of these problem areas were solved and the solutions incorporated into the Series, then they would be much more powerful simulations of making a set. The general impulse of the simulations would not alter very much, however. The work would still be seen in terms of discrete "jobs;" a person's "job routine" would still be thought of as a simple linear string of "jobs;" and the fundamental constraints of the

system would still be regarded as relatively simple sequencing based upon the mechanics of manipulating the seine. All of these plus the general guidelines for identifying "jobs" and necessary sequencing would stay the same. But, how accurate are these views of seining?

As an empirical account of seining, the Toy 1 and Toy 2 Series convey much of what it is like to make a set. There are many jobs. There is a partial ordering according to which jobs should be done. And there are only a small group of people who divide the labor and do all the work. But, beyond these there are several aspects of the work which are incompatible with or hard to fit into the Series descriptions.

The basic logic which generates Toy 2 is alien to the bulk of people who do the work it describes. To get into the proper mental state to build a Toy 2 representation, one fixates on two concepts: "tension" and "movable part." Once these have become dominant in your mind, you then scrutinize seining in terms of them. Given as the general plan that a large, heavy seine is to be dragged around in the water and then recovered and put on deck, where is the physical "tension" at what times and what "moveable parts" will handle and/or transfer that tension? As a ground for such contemplations, a particular boat's hardware and a particular style of manipulating the seine is assumed. One in this analytical frame of mind ignores a considerable number of things which a worker normally thinks about and, conversely, sometimes makes much of what is to a worker only a fleating semi-consciousness.

But, criticism at this level is easy for one is comparing a post hoc, reflective discussion about seining with one's attentions and thoughts while seining, and the two are naturally quite different. More significantly, a comparison between Toy 2 logic and a seiner's typical reflective discussion about seining reveals basic differences, however. As first evidence of this, one may compare Toy 1 "jobs" with Toy 2 "jobs." In addition to these differences, the "natural" discussion about seining concentrates not upon manditory sequencing, but rather upon the relations between jobs and the general plan of making a set, that is, the "functions" of jobs. So, Toy 2 logic (as a mnemonic I call it "mechanical thinking") is alien to seiners both because its mode of identifying "jobs" is peculiar and because it fixates on sequencing rather than purpose. An innovative skipper may conjure this state of "mechanical thinking." After he has imagined a new way of manipulating the seine, he may conjure something similar to Toy 2's logic in order to work out the mechanics of how to effect his new style. But other than this rather rare situation, I do not think seiners ever contemplate their work in this fashion, neither while seining nor while reflecting upon it. Thus, Toy 2 logic is not a salient, or cognitively empirical, mode of seeing the work of seining, rather it is an analytical mode.

A general aspect of seining knowledge that is not captured in either Toy 1 or Toy 2 Series is that seiners do not see their job routines as simple strings of jobs. Owing to the format demands of narrating one's job routine, a seiner's story will give the impression that he does one job after another after another until his work is finished, and this is

displayed in Toy 1 Series representations. But, if that same person is questioned and probed more deeply about how he regards his work, he quickly evidences much more than a simple "string of beads" organization. A cluster of jobs will hang together as some sort of bundle; sometimes a bunch of jobs are part of a larger job (sometimes named, sometimes not); and furthermore, the jobs are placed in temporal chunks which relate holistically to the perceived phases or segments of the entire operation. In other words, seiners hierarchically organize their work. A particular organization is specific to an individual, however, and thus it would be inappropriate to use any given one as a basis for Toy 1 or Toy 2 representations--what would it mean to do so?

The last problem area I shall discuss here is that the personal experience of seining is very different from the format of either Series. Essentially, this is what the two previous objections have said more specifically. Although weakly related, there are at least two ways the experience of seining can be conjured from memory. The first of these happens when a seiner mentally relives his work while making a set. I can sit in silence and imagine I am making a set. While doing this, I am aware of my thoughts about my work, what I attended to when, what muscles were used, and many other personal things. This activates, though perhaps with distortions, my actual performance knowledge. On the other hand, I can also sit, either alone or in discussion with someone, and remember my seining experience, but this time I think <u>about</u> my work rather than reliving it. This duality is, of course, not peculiar to seiners; so far as I know, everyone is capable of activating either mode.

It would probably be unfair to compare the Series with my performance knowledge for they are of different modes entirely, but my point here is that the Series do not resemble even my reflective thinking about my experiences. The Series merely describe how's; they do not encompass the why's. Perhaps underlying this whole problem to a large degree is the fact that the Series are one-level analyses, but the work itself is felt to be naturally multi-level, ranging from brief synopses of what happens to the seine all the way down to muscle patterns.

In general, these three empirical criticisms are alike in that they argue against the Series, regarded as an empirical account, on the grounds that the Series' formats distort the nature of the work as it exists at the cognitive or subjective level. Jobs at the subjective level are different from "jobs" in either Series, and the exclusive concern of the Series with sequencing misrepresents the nature of the relations amongst jobs. The subjective level has much more fluidity than the Series representations allow. As evidenced in the Log of Emotional States in Section 3.3, jobs frequently have an anticipatory build-up as well as an afterglow decay period, and in many other ways, they are different from "jobs." Empirical aspects such as these, arising in the subjective or cognitive description of jobs, are incompatible with the "objective" characterization of "jobs" in the Series. But, does this incompatibility mean that Toy 1 and Toy 2 Series have not described the nature of seining?

Polanyi (1968:49-50) provides a beautifully simple analogy of the problem I have been discussing. The question is what principle operates by which a cyclist keeps his balance while riding his bicycle.

> The rule observed by the cyclist is this. When he starts falling to the right he turns the handlebars to the right, so that the course of the bicycle is deflected along a curve towards the right. This results in a centrifugal force pushing the cyclist to the left and offsets the gravitational force dragging him down to the right. This maneuvre presently throws the cyclist out of balance to the left, which he counteracts by turning the handlebars to the left; and so he continues to keep himself in balance by winding along a series of appropriate curvatures. A simple analysis shows that for a given angle of unbalance the curvature of each winding is inversely proportional to the square of the speed at which the cyclist is proceeding.
>
> But does this tell us exactly how to ride a bicycle? No. You obviously cannot adjust the curvature of your bicycle's path in proportion to the ratio of your unbalance over the square of your speed; and if you could you would fall off the machine, for there are a number of other factors to be taken into account in practice which are left out in the formulation of this rule.

The Toy 1 and Toy 2 Series are comparable to the "rule" for keeping one's balance on a bicycle: they describe how to seine, but do so in a manner which is different from the knowledge seiners have of their work. Does the "rule" for how to ride a bicycle describe the nature of bicycle riding? Do the Toy 1 and Toy 2 Series describe the nature of seining?

To answer these questions, one must first understand that there are levels of empirical inquiry and ranges of empirical incorporation. Basically, these notions are grounded in the observation that there are many levels of detail or specificity at which one may describe a given phenomenon.

One of the first clear expositions discussing levels of reality in anthropological literature is Kroeber's (1952d) layer-cake characterization of scientific studies. He saw four distinct, emergent levels of reality (the empirical world): the physical-chemical, the biological,

the psychological, and the social or cultural. His primary concern, of course, was to justify the independence of the "superorganic" realm, and was foreshadowed in this effort by Durkheim (1964) and others. Kroeber's formulation is interesting, however, because it attempted to be comprehensive.

The general point of speaking of levels of the empirical world is to legitimize scientific inquiry within one level. That is, it is used to argue against a totally reductionist view. In recent years, the arguments against the reductionist stance have gained wide support from diverse fields of inquiry. Arising primarily from studies of complex organizations, the more recent proponents of empirical levels couch their views in terms of phrases such as "the whole is greater than the sum of its parts." Others, for example Gluckman, ed. (1964), argue that if for no other reason than the rapidly growing corpus of scientific knowledge itself, which a single human can hardly be expected to know, our view of the world will be multi-leveled. Generally, however, most scientists appear to hold as a maxim that the burden of proof falls upon the one who postulates a new level rather than upon those who do not. This is a healthy attitude for many levels of reality are conceptually feasible, the point is to adduce how many and which levels do in fact seem to be.

> The world is a large matrix of interactions in which most of the entries are very close to zero, and in which, by ordering those entries according to their orders of magnitude, a distinct hierarchic structure can be discerned. (Simon 1973:23, emphasis mine)

Bringing this grand scheme of the world back to the problem at hand, my question is whether or not the discrepancies between the Toy 1 and Toy 2 Series and the subjective experiences of seining in fact argue against the validity of the Series or merely indicate differences in level of detail in the descriptions. Chomsky's (1969 Chapter 6, 1970:18-47) discussions of adequacy--both "descriptive" and "explanatory"--are pertinent here.

As previously noted, the Series do not have explanatory adequacy. They are built up from exceedingly general formalisms which pose very few "internal conditions" with the result that not even the domain of "Work" is theoretically circumscribed much less to say "Seining." Deciding whether they have descriptive adequacy is awkward because even though "external conditions" have been found which would contradict the Series (the differences evident between "jobs" and jobs in the subjective experience, for example) it remains at issue whether these discrepancies are pertinent objections. A defender of the Series could merely claim that how jobs register subjectively is beyond the scope (at a different level) of the Series and therefore of no consequence with respect to the description of seining embodied in the Series' level. With this, I disagree.

The reader familiar with Harris (1964) may have already noted the similarity between his "actonic" mode of analysis and my Toy 2 Series. Both try to identify units of human behavior from an objective posture, specifically by referring to intersubjectively observable "environmental effects" of human actions. Thus, while Harris might discard my Toy 2

Series as a proper description of seining on the grounds that its "jobs" are not actones ("jobs" are not minimally identified units of human action), he would, I think, approve of the overall impulse because it derives from a line of thinking congruent with his expressed objective empiricism. And, on behalf of this overlap, Harris would most likely object to my insistence that the Toy 2 Series (and to a lesser extent, the Toy 1 Series) is not empirical. Thus, in arguing against my own creation (the Series), I must also argue against Harris's position.

While I must admit that the knowledge I have of purse seining is not itself readily observable to others, it is "observable" to me through introspection. I know how my feelings feel, whether or not anyone else does. To the extent that I pass as a genuine, bona fide seine fisherman (and I do) in the estimation of other seine fishermen, my subjective feelings concerning seining are relevant to a description of that phenomenon. If I were to generalize my specific comprehension of seining to all seiners, then I would most certainly be wrong. But, I am criticizing the Toy 1 and 2 Series not in terms of whether it matches my specific cognitive constructs of specific jobs. Rather, I am charging that those formalisms misrepresent the general properties of my subjective experience with the phenomenon. And, I insist that how the people, whose actions and behaviors constitute the phenomenon in question, feel about the work they do is indeed pertinent and essential to a description of that phenomenon. Furthermore, though individual seiners cognitively organize their work differently, they appear to be like me with respect to the general features I have used to argue against the Toy 2 Series.

Basically, Harris seems to disclaim any responsibility whatsoever for incorporating the inner life of human actors into his analytical scheme. His reasoning appears to be predicated upon the operational problem of how this inner life can be made intersubjectively observable. In a sense, I admire his attempt at consistency, but I feel very strongly that he has thrown the baby out with the bathwater. The logical extension of his reasoning is that I cannot "know" my own feelings because no one else possibly can. While this may be true from within the confines of a staunchly empirical epistemology, it strikes me as the height of absurdity. It is only through my own feelings, thoughts, emotions, interpretations, and perceptions that I "know" there <u>are</u> other people, and similarly, it is only through these subjective-based notions that I believe those other people agree with me and observe the same things I do. Thus, when introspection reveals general properties of my thinking which are subsequently confirmed in others who have experienced similar situations, then I feel justified in regarding those general properties as very much part of the empirical world, part of nature. Therefore, I am very opposed to Harris's over-zealous empiricism.

In Figure 9, the infinite number of levels of descriptive detail are represented as a directed line ranging from the most general description possible[18] (on the left) to infinitely detailed descriptions (on the right). Such a representation may be regarded as applying to the whole

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[18] I thank Prof. F. K. Lehman for noting that there is a maximally general description, the null description.

of reality. Most human awareness, astro- and sub-nuclear physics not withstanding, operate within only a relatively small portion of this spectrum of possibilities, but then, the figure is not drawn to scale.

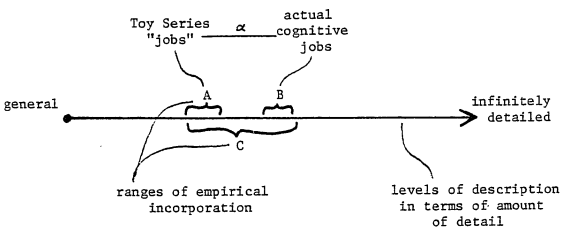

Figure 9. Range of empirical incorporation and descriptive detail

Almost by definition, information will be lost as descriptions of a phenomenon move from right (detailed) to left (general). But, "information loss" should not involve actual distortion except in cases where "near-decomposability" (Simon 1973) occurs owing to the empirical existence of emergent phenomena. Since, as I have commented, moving from B to A (making the comparison α) reveals distortions, and not simply

information loss, either the Series description must be surplanted by one which does not do violence to level B, or I would have to proclaim that level A (as described in the Series) is an emergent level of reality. Given the already suspect qualities of the Series description (its nearly complete lack of principled sensitivity to data even from level A), I think it far more prudent and appropriate to simply disclaim the Series as a description of the nature of seining and to make a better one.

There is yet a third possibility. Following the logic of Kaplan (1965) and Manners and Kaplan (1969), the discrepancies existing between the Series description and my brief cognitive description would cause no problems. Since they are motivated by different questions, they would be equally valid, provided, of course, that each accurately describes its relevant data. The consequences of such a semantic-based philosophy of science is fragmentation in the extreme. While opposed to reductionism, hence similar to Simon (1973), Rapaport and Horvath (1974), Rapaport (1974), Boulding (1975), and others, it differs markedly from these in that there is no grounding empirically through the notion of "emergent." I cannot argue conclusively against such a view, but I do not find it acceptable. I prefer to imagine science and the world in holistic modes, but this is just my preference, not a conclusion.

Having decided to disclaim the Series as a description of the nature of seining, I must ensure that its replacement will not only describe seining at level A but also jibe with level B. This new description should have C as its range of empirical incorporation; it will encompass,

or incorporate, a broader range of descriptive levels, hence it will be empirically richer. This will be undertaken to a large extent in Appendix B.

3.6 Summary

This chapter has done three things. First, it has provided a moderately detailed description of the basic task of salmon purse seining--"making a set." This central operation is the congealing essence of the culture complex of which it is but a part. I hope the reader is now sufficiently familiar with this bundle of activities so that if he were dropped on the deck of a working seine boat, he would not be completely bewildered by what is going on all around him.

Second, the data has been cast in an initial formalism. Using this formal representational mode, I then developed ways to evaluate alternative crew organizations, provided that one first establish duration values for each "job." This kind of effort is in line with a generally functionalist paradigm. It should be apparent how one could go from efficiency calculations to explanations of crew organization stability or instability. Although most anthropologists do not describe their data at the level of detail found in Toy 1 and 2 Series, the fundamental understanding is quite common. For this reason, I called this chapter, "Standard Perspectives. . .". I would suggest that the method of dealing with sequential behavior, here applied to purse seining, could also be employed in the description of all human actions. Unfortunately I feel the same maladies would result.

And third, some insight into the basic nature of seining has been achieved. These insights resulted as I faulted my own descriptions, that is, as I criticized the Toy 1 and 2 Series for their inadequacies. Basically, there are two major observations to be made: (1) an operationally explicit method of identifying "jobs" does not necessarily yield psychologically real work-segments; and (2) the initial assumptions, that seining could be appropriately described in terms of a single-leveled collection of discrete elements related in ordinal-time, are too weak, too simple-minded to capture the real nature of the beast.

If one ponders Toy 1 and Toy 2 with respect to their representational accuracy, it becomes clear that the root of their failings may be attributed to the fact that they are both ordinal-time simulation attempts, as opposed to real-time formalizations. Ordinal-time requires rigidly discrete unities; each element must be sharply and unambiguously bounded. Another way of saying this is that ordinal-time depictions rely heavily on the concept of an "event." But, if not all aspects of one's work experience are simple, black-or-white, chunky, easily demarcated episodes, then graph theoretic formalisms are inappropriate. Rather, one requires more continuous modelling in which the "elements" can be described and characterized by gradations and intensities. Appendix B illustrates a mode of modelling more in line with these general considerations, especially its Toy 3 Series. I would recommend, however, that the reader examine Appendix B only after reading Chapters 4 and 5, because they are necessary to understand what the appendix is all about.

At this point, I shall leave off further discussion of salmon seining. There are numerous areas which are conspicuously missing from the foregoing ethnographic accounting. For example, I have omitted the attribution of "luck" as an explanation for catching a lot or only a few fish; I have not discussed how fishermen regard the Fish and Game people; I have not really developed the role of fisherman vis-à-vis the community; and on and on.

What is important for the remainder of this dissertation is that salmon purse seining in Southeast Alaska is a very good example of a culture complex. The reader has some familiarity with this complex, owing to Chapters 2 and 3. And, I imagine that by now the title of this dissertation is an anomaly. So far, I have written only about fishing and hardly mentioned cultural stability or memory. Nor have I made any attempt to demonstrate how all three subjects are interrelated. Also, I have noticeably vacillated amongst several perspectives--from personal to impersonal, from historical narrative to structural-functional thinking. The remaining chapters will initially continue in this flirtation with perspectives, but the courtship ends in a marriage.

In what follows, it matters little that my research was on salmon fishing. I could have studied high school football, pool hustling, pipe smoking, birdwatching, the kula ring, or any other culture complex. Chapters 4 and 5 are theoretical in nature and are neither specific to nor derived from salmon seining. On the other hand, my personal involvement with this particular culture complex has served as an evaluative basis from which I judge the various theoretical stances that would claim

to account for cultural stability. Thus, by experiencing this culture complex as well as having tried to study it, I see very clearly the dilemma of trying to decide whether there really is a culture complex "out there" or whether the culture complex is only something I constructed in me. Where is it? What holds it together through time? These are the sorts of questions I shall be addressing for the rest of this dissertation. And, although seining is not mentioned prominently again, it has nonetheless been in my thoughts as I have tried to comprehend the more general issue of how it is possible that portions of human culture change at different rates.

CHAPTER 4

AN OUTLINE FOR A GENERAL CONCEPTUALIZATION OF CULTURE COMPLEXES

4.1 Introduction

In the preceding chapters I described the "purse seining complex." These descriptions were compilations of certain facts that come under a common heading as I contemplate my fieldwork experience and some readings. But in writing that way, I implicitly exhibit a commonplace but nonetheless remarkable aspect of my thinking. I feel that all the facts, incidents, happenings, and interpretations are related insofar as they all pertain to the same thing--the purse seining complex. What sort of a thing is this "purse seining complex?" What makes it appear to me as a unity? What gives it coherency? What underlies its stability? Obviously, these questions ask about more than just purse seining; they beg an answer to how it is that my cultural knowledge is organized in "clumpings." The general question is thus, how is it that culture complexes cohere as entities?

The phenomenon which I am seeking here to understand may be generalized as cultural partibility. I examine my own thinking and find that my ideas, feelings, emotions, etc. do not randomly intermix with one another, rather they clump together in innumerable but usually familiar ways. Thus, introspection reveals a non-homogeneous, non-atomistic mental make-up; my knowledge is partible but not rigidly so--it is divisible into parts, but not partitioned in the mathematical sense. Thus, cultural partibility is

a fact of my thinking. This is the subjective experience.[1]

There is an argument for cultural partibility which will have more force for those who are disinclined toward subjectivity. It is an a priori, deductive argument, and begins with the question, "is human culture distributed in space and time?" Should one answer this question in the negative, then he will be led to develop seriously either a notion of group mind or a kind of solipsism. (In this case, the phenomenon of cultural partibility will still be recognized, however, owing to the subjective experience.) Should one answer the question in the affirmative as most everyone does, then he has made at least one assumption and posed cultural partibility as a major problem for conceptualization. The assumption is that humans everywhere and through time produce or support "culture" equally, that is, "culture" is a legitimate class of phenomena. If "culture" is distributed in space and time, then one must envision the units of its distribution--how is culture divisible into parts so that it can have distribution and what is the nature of such parts.

There are two basic ways of construing cultural partibility once one places culture in space and time: (1) culture is manifest in cultures; and (2) culture forms in complexes. The first is derivable from the second, but not conversely, therefore I believe an understanding of "culture complex" as a concept is a completely fundamental though neglected task of Anthropology. This, then, will be the aim of this chapter--to

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[1] See Buchler (1966) for an excellent philosophical discussion of "complexes" as the basis for a metaphysical system.

review a selection of pertinent literature and formulate what I feel is the most profitable mode of conceptualizing "culture complex."

There are at least two issues to be considered in this attempt. The first regards the ontological status of complexes--where and in what sense do they exist. The second concerns how to account for a complex's coherency and stability--what sorts of forces are involved in the "hanging together" of the clumpings of culture that we call culture complexes.

I shall conclude that "culture" complexes have their existence in the cognitive make-up of the "natives," that they hang together as unities owing to the way they are constructed in the process of learning, and that at least much of their stability derives from the nature of human memory.

4.2 Review of Selected Literature Bearing on the Idea of Culture Complex

It is difficult to jump into an intellectual current, but one has to begin somewhere. I shall take up the subject of culture complexes with the somewhat tangential article by Tylor (1889), in which he introduced the notion of "adhesions."

Tylor was interested in making Anthropology scientific, as he understood the adjective. To this end, he proposed a method for discovering "adhesions" through inspection of the co-occurrence of customs. He phrased the procedure as follows:

> The particular rules [of marriage and descent] have been scheduled out into tables, so as to ascertain what may be called "adhesions" of each custom, showing which peoples have the same custom, and what other customs accompany it

> or lie apart from it. From the recurrence or absence of these customs it will be our business to infer their dependence on causes acting over the whole range of mankind. (ibid.:246)

For example, Tylor categorized the "avoidance-custom" into three types: avoidance between husband and his wife's relatives, between wife and husband's relatives, and mutual avoidance. Inspecting the data of roughly three hundred and fifty peoples, Tylor found that there were forty-five cases of avoidance between husband and wife's family, eight cases of mutual avoidance, and thirteen cases of avoidance between wife and husband's family. On the basis of two ethnographic examples which seemed to evidence a relationship between avoidance and postmarital residence, Tylor undertook to see if avoidance and residence were associated in some systematic way. Thus, he tabulated the frequencies of what today would be called virilocality, delayed virilocality, and uxorilocality. The sample of three hundred and fifty peoples had one hundred and forty-one instances of virilocal residence, seventy-six of delayed virilocal, and sixty-five cases of uxorilocal residence. If the custom of avoidance between husband and wife's family and the custom of uxorilocal residence were independent of one another, then one would expect that there would be roughly nine cases in the sample of three hundred and fifty which would have both ($\frac{45}{350} \times 65 = 8.357$). However, the data had fourteen cases rather than the expected nine. Similarly, if avoidance between husband and wife's relatives were independent with respect to virilocal residence, then one would expect roughly eighteen cases ($\frac{45}{350} \times 141 = 18.129$); but instead of the expected eighteen cases,

Tylor found only nine such instances of co-occurrence. He then performed the same kind of two-way test for avoidance between wife and husband's family and virilocal/uxorilocal residences and found a similar sort of correlation.

> Thus there is a well marked preponderance indicating that ceremonial avoidance by the husband of the wife's family is in some way connected with his living with them; and vice versa as to the wife and the husband's family. (ibid.:247)

This, then, was the first demonstrated "adhesion." Tylor went on to adduce a reasoning (after the fashion of a univerally applicable cultural law) to account for the correlation.

The method of adhesion (correlation) analysis reviewed above seems straightforward enough, but it has serious problems. As Galton pointed out in discussion of Tylor's presentation, the method works only if it is legitimate to assume that the peoples comprising the ethnographic sample are independent of one another (ibid.:270). Actually, Galton's problem does not concern the method of finding correlations, but rather only how one goes about accounting for the observed correlations, whether invoking the operation of pan-cultural factors or some process of historical replication of the same case (cf. Naroll 1970 for methods of filtering historical connections from the ethnographic sample). In addition to this, which is called Galton's problem, Professor Flower's concern over making sure that the ethnographic cases are of "equivalent value" poses a major methodological problem. Flower was essentially asking whether it is legitimate to count an atoll population of several hundred persons an equivalent case to that of China: does not number of people involved

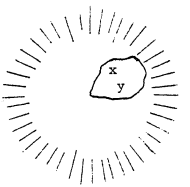
in the sociocultural system make a significant difference in the meaning and interpretation of "frequency?". These are the usually recognized implications of Tylor's article.

From the viewpoint of examining Tylor's method for a statement on the nature of culture complexes, neither Galton's nor Flower's comments are directly pertinent, but one may ask whether "adhesion" is somehow related to "complex."

At first view, it may occur to one that because an adhesion is a demonstrated "hanging together" of component parts (customs), it therefore fits the fundamental criterion for a complex. Upon further reflection, however, it becomes clear that an adhesion does not specify the nature of the co-occurrence of customs, rather it recognizes and labels only an unexpected (i.e., non-randomly distributed) frequency of co-occurrence. And, "co-occurrence" refers only to co-existence of customs within the same culture. Thus, it does not necessarily imply that such co-existent customs are intimately related; they may or may not be so related. If one advocates a strict structural-functional position, of course, then co-occurrence does imply intimate relatedness. But, if one admits to the evidence of history, that cultures can and do adopt customs, traits, etc. from other cultures, then co-occurrence of customs in a culture, by itself, does not imply close relationship of those customs (see Figure 10). Thus, it is apparent that although complexes may underlie unexpectedly high frequency of co-occurrence among customs (either through the action of a pan-cultural law, convergence or historical replication of a given complex), such a high frequency of co-presence

of customs does not necessarily evidence the existence of a complex. It may or may not; that is to be determined by some other means.

Culture A



Culture B

Figure 10. Co-occurrence of traits in a culture does not necessarily imply a culture complex

In Culture A, traits x and y are both present, but they are not themselves closely related. In Culture B, x and y are again both present, but here they are intimately articulated. The two cases of association are very different. Only in the case of Culture B should one speak of traits x and y being in the same culture complex.

Although at first strange, the conclusion to be drawn from an examination of Tylor's "adhesions" is that not they, but rather "customs" possess the unity characteristic of complexes. Only a version of structural-functionalism proclaiming that all traits in a culture are equally related can make frequency of co-occurrence definitive of complexes. Should one choose to adopt such a strong version, which I cannot accept as other than an analyst's dream of cultural reality, then it follows that complexes have only a statistical unity. Furthermore, such a view

will have to devise ingenious means of accounting for the residue of the central tendency. "Customs," on the other hand, are treated as if they have a recognizable sameness (characteristics of unitary entities) irrespective of the particular cultural milieu in which they occur. Furthermore, customs exist without reference to frequency. Tylor has not, however, provided a rationale for the unity of customs or told us what sort of empirical referents justify designation as a custom. Supposedly, a custom is the same thing wherever it may be found.

A major mode of thinking about culture complexes is to be found in the Kulturkreis school of ethnology. The idea of a Kulturkomplex was an essential part of their thinking because a Kulturkreis was "conceived as the area, wherein there prevails a given culture-complex,--where all the cultural elements of this complex are to be found in the most, approximately, pure form" (Herskovits 1926:231). Once these Kreise had been determined, the present distribution of culture around the world was to be explained in terms of varying combinations of cultural strata resulting from the diffusion of the Kreise. Unlike some of their contemporary English diffusionists, the German diffusionists allowed for multiple centers of cultural development, and the concomitant concept of cultural strata (indicative of different sources of origin) was one of the principal distinguishing features of the Kulturkreislehre when compared with other historical or diffusionist schools (Lowie 1937:178).

With respect to the actual determination of cultural similarities (a necessary initial procedure before plotting distributions), they firmly eschewed psychological criteria and used only the criteria of form and of

quantity (Boas 1966b:300; Kluckhohn 1936:162-163; Lowie 1912:25). Furthermore, in accounting for the spread of cultural similarities, it was maintained that diffusion acted on whole complexes:

> "A migration of cultural elements, also of tales, over wide distances, without the spread of other cultural possessions at the same time, may be designated without hesitation as a 'kulturgeschichtliches' nonsense." (Boas 1966b:301, translating a passage from Graebner's Methode der Ethnologie, 1911:116)

Briefly summarizing the Kulturkreislehre school, the essential ideas were:

1. Similarities are to be defined in terms of the criterion of form (morphology) and secondarily in terms of the quantity of formal correspondences;
2. All similarities are due to historical unity;
3. Culture elements spread in complexes;
4. Complexes originally developed in Kreise, or culture circles-centers; and
5. Local culture is to be explained in terms of the specific history of the locality with respect to its overlaying culture strata (a stratum being one diffused complex).

As Kluckhohn's sensitive review of the German diffusionists shows, the above summary is perhaps too black and white, too clear-cut a picture. Within the school there were varying degrees of emphasis on one or another methodological principle. For the purposes at hand, however, being completely fair in evaluating each and every point is quite tangential to the reason for examining the German diffusionists. I am more concerned

with how they thought about the coherency and stability of their Kulturkomplex, and on this point, the critics seem to agree.

> The Kreise are before us as ultimate axioms and by ingenious shuffling of their constitutents the whole of civilization is explained. But what lead to the definition of the complexes? (Lowie 1937:180)

> . . . based on the supposed co-existence of a number of diverse and mutually independent cultural traits which reappear in the same combinations in distinct parts of the world. (Boas 1966c:282)

> It is the very simplicity of the units of the Kulturkomplex, and the capricious distribution of each unit, as compared with that of any other, which makes the Graebnerian assumption of unity of origin so unconvincing. (Herskovits 1926:234)

> One searches in vain for a clear and lucid explanation of how the ethnologist may know precisely which traits form a particular Kreis. (Kluckhohn 1936:168)

> . . . the central tenet of the Kulturkreislehre: that the association of culture elements rests solely upon an historical connection and not upon an inner relation of the elements themselves.[16]

> [16] . . .Actually one suspects that there is need for some clarification here, for while it is stated that the persistence of the combination of traits rests only upon historical genetics, yet in the discussion of the concrete Kreise it often seems implied that there is an internal relation of some elements which makes for the stability of the union . . . The whole subject of coherence in the organic world would seem a fertile but vast and unexplored field. (ibid.:166)

For example, the Moiety complex in Oceania (one of six layers defined for the area by Graebner) is defined in terms of such diverse elements as yam cultivation, plank boats, gable roofs, fire saw rather than fire drill, and heavy war clubs (Lowie 1937:181). From points of view seeing a complex as a cluster of related elements, such ad hoc delimitation of a Kulturkomplex seems quite strange and unmotivated. The reasoning behind

such hodgepodge definition of a complex, however, seems to be located in the general orientation of the Kulturkreis school to explain all similarities only in historical terms and reject all attempts to explain in terms of independent development (whether by convergence or parallelism). For, should such a strange amalgamation of elements actually be found to travel together, the case for diffusion as opposed to independent development is greatly enhanced.

Thus, it can be seen that the unity of a Kulturcomplex rests only on historical unity, of formation within the same Kulturkreis. Only the commonality of original homeland underlies a Kulturkomplex's coherency, and the stability of the complex once formed is assumed, not accounted for (Boas 1966c:282; Herskovits 1926:246, Kluckhohn 1936:166; Lowie 1937: 180). The conclusion is, then, that the Kulturkreis sees the unity of a complex as merely an historic unity derived from common development of the constituent elements within the same geographical area. This view may be supplemented with some version of original internal relatedness, but this seems not to have been explicitly described. However, in restricting the determination of cultural similarities only to formal, morphological features, rather than incorporating psychological criteria as well, the Kulturkomplex concept says absolutely nothing about stability other than to subsume it under some implicit assumption regarding the basic uninventiveness of humans (cf. Kluckhohn 1936:165).

Early in the development of "American" Anthropology, Boas set down views of culture and culture change that separated it from both hard-core diffusionism and parallelism (unilinear evolutionism). The fundamental

distinction of his "historical method," when opposed to the "comparative method" (including both diffusionism and parallelism), was the acknowledgement that similar results can be reached through different histories or causal sequences (Boas 1966a:273 and 280, 1965:169, 1966c:282, 1966d:258, 1938:4). This view of culture and cultural development allowed for the principle of convergence as a permissible explanation for cultural similarities. He did not, however, make this principle into a theoretical dictum, but rather argued its plausibility along with diffusion and parallelism. The point was to see which explanatory framework was evidenced in each empirical case rather than to proclaim one or another principle as the ultimate dogma.

> The theory of convergence claims that similar ways [of confronting the same problem] may (not must) be found. This would be a truism, if there existed only one way of solving the problem . . . Nobody claims that convergence means an absolute identity of phenomena derived from heterogeneous sources; but we think we have ample proof to show that the most diverse ethnic phenomena, when subject to similar psychical conditions, or when referring to similar activities, will give similar results (not equal results) which we group naturally under the same category when viewed not from an historical standpoint, but from that of psychology, technology or other similar standpoint. (Boas 1966b:299)

Although not the first to introduce the principle of convergence in application to culture (Ehrenreich 1903, appears to merit this distinction[2]), Boas, and his students and followers, coupled the principle with a particular method of data collection (cf. Boas 1966a:276). This combination has come to be known as "historical particularism," and it has

[2]Radin (1933) attributes it to someone else, but Lowie and Goldenweiser go with Ehrenreich. It is sufficient here that Ehrenreich's article stimulated Graebner, Boas, Lowie, and Goldenweiser, who appear to be the major authors involved with this issue.

had varying evaluations depending upon numerous factors (cf. Harris 1968: 250-289) which do not really concern us here. Lowie's glowing commentary of Boas's contributions to Anthropology brings us more to the point of immediate interest.

> The intellectual liberation due to Boas is the same that we gain whenever science substitutes a sound phenomenalism for a arid conceptual realism based on premature classification. (1937:142)

One of the main ways in which Boas contributed to the development of the concept of a culture complex was in his criteria for discerning cultural similarities. Over and over he reiterated his objection to Graebnerian-like insistence on morphology as the essential criterion of similarity. For example:

> The concepts of comparability and homogeneity, as I understand them, have to deal not only with historical relationship, but to a much higher degree with psychological similarity, for only as elements of the mental make-up of society do ideas or actions become potent and determining elements of further development. (1966b:300)

> A serious objection to the reasoning of those who try to establish lines of evolution of cultures lies in the frequent lack of comparability of the data with which we are dealing. Attention is directed essentially to the similarity of ethnic phenomena, while the individual variations are disregarded. As soon as we turn our attention to these we notice that the sameness of ethnic phenomena is more superficial than essential, more apparent than real. (1965: 171)

> . . . concepts, like war, the idea of immortality, marriage regulations, have been considered as units and general conclusions have been derived from their forms and distributions. It should be recognized that the subordination of all such forms, under a category with which we are familiar on account of our own cultural experience, does not prove the historical or sociological unity of the phenomenon. (1966d:258)

Thus, Boas was interested in more than mere morphological resemblances; he was insistent that the "psychological" or "sociological" qualities of

culture be included in the calculation of similarities.

From the foregoing, however, there is no indication of what underlies the psychological or sociological qualities of culture. Boas seems willing to account for morphological features of cultural elements in terms of their history, but the psychological or sociological concomitants are, as yet, left dangling with no sources and underpinnings. He seems reticent to accept unqualified some principle of psychical parallelism (in the sense of unilinear psychic evolution):

> In order to interpret correctly these similarities of form, it is necessary to investigate their historical development; and only when the historical development in different areas is the same, will it be admissible to consider the phenomena in question as equivalent. (1965:173)

And, he rejects both environmental and economic determinism, which some feel could account for similarities--morphological, psychological, and sociological (ibid.:173-177, 1966d:256).

The key to the psychological dimension of culture, he concludes, thus lies in the psychic unity of mankind, in the sense of psychic unity through time as well as space.

> There is no fundamental difference in the ways of thinking of primitive and civilized man. (1965:17)

> The dynamic forces that mould social life are the same now as those that moulded life thousands of years ago. (ibid.:178)

In other words, culture interacts with a constant--the common psychic substratum inherent in all humanity.

The combination of this conceptualization of psychic unity and the recognition of historical processes produced a vision of culture as more than just a heap of elements.

> The mere enumeration of these various aspects of life, however, does not constitute culture. It is more, for its elements are not independent, they have a structure. (ibid.:149)
>
> Culture is integrated. It is true that the degree of integration is not always the same. . . . Integration is not often so complete that all contradictory elements are eliminated. (ibid.)

Furthermore, this combination led Boas to a fluid, dynamic image of culture with the source for development located in the interaction between individuals and their culture.

> As soon as these methods are applied [the "historical method"], primitive society loses the appearance of absolute stability which is conveyed to the student who sees a certain people only at a certain given time. All cultural forms rather appear in a constant state of flux and subject to fundamental modifications. (1966c:284)
>
> . . . developments due to inner forces . . . [may] be observed in every phenomenon of acculturation in which foreign elements are remodeled according to the patterns prevalent in their new environment, and they may be found in the peculiar local development of widely separated ideas and activities. (ibid.)
>
> The causal conditions of cultural happenings lie always in the interaction between individual and society, and no classification of societies will solve this problem. (1966d:257)
>
> For these inquires [into the dynamics of change] we have to understand the interrelations between individual and society . . . (1938:4)

Summing up Boas's thinking relative to culture complexes, the first and foremost point is that he incorporated the psychological dimension into the criteria for similarities: two culture traits were not the "same" unless, in addition to morphological features, they were psychologically similar. This means that culture traits (complexes, etc.) must have psychological unity. Second, his method for studying culture directs

one to immerse oneself in a specific "living society" but also to keep one's eye on distributional data and remember that cultures do not develop in vacuo. Only in such fashion can one gather "psychological" data in addition to the more objective data found in formal features of culture. Lastly, Boas reminds us that culture cannot be understood if viewed as merely an "additive" phenomenon (Rapoport and Horvath 1974), rather it has structure, patterning, non-independence of parts.

With respect to the sources of coherency and stability of complexes, it would seem that Boas placed great emphasis on the human psychic substratum and upon the processes of history rather than arguing only in terms of logico-functional interrelatedness. Boas had a fine sense of history.

Wissler is the earliest writer I have come across who presented a direct discussion of the idea of a culture complex. It is not that he was the first to use the concept in doing research, but he seems to be one of the first to address the concept explicitly rather than using it with implicit meanings.

He began by defining a "culture trait": a trait is very simply a "unit of tribal culture" (1923:50). It may be applied "to mannerisms and to concepts of whatever kind," in addition to thoughts and deeds (ibid.). As a first approximation, it thus followed that

> . . . a tribal culture is characterized by the enumeration of its observable traits and that the culture of one tribe is distinguished from that of another by differences in these traits. (ibid.)

This first approximation of the culture trait idea gives the impression of the trait as an atomistic place-holding concept. That is, it may be

applied to almost any level of culture content, yet once it is applied, the delimited contents have no specified internal organization.

Recognizing the extreme atomism inherent in his initial definition of a culture trait and the consequences of such thinking, Wissler developed the notion of a "trait-complex" (under the section heading of "The Culture Complex"). If one thinks of culture in the manner sketched in his first approximation, then one might well imagine that culture is like a bushel of potatoes, each potato (trait) being "a complete and independent unit in itself. Yet, when one carefully scrutinizes a trait, he finds not a clear-cut unit, but a kind of complex" (ibid.:51). Thus, in the second statement regarding the nature of a culture trait, Wissler attempted to overcome atomism through the notion of a "complex," or "trait-complex." But, he went further than this (which really has not said much of anything with respect to the sources of the trait's unity). What, for Wissler, holds the sub-elements of a trait-complex together?

Using the example of the maize complex of aboriginal North America, Wissler claimed that the elements of this large trait-complex "all are dependent one upon the other. They thus form a kind of logical sequence, the ultimate aim of which is to provide a specific type of food" (1917: 384). This view of the unity of trait-complexes is slightly modified in a later work:

> In our discussion of the trait-complex, we found its components associated because they had logical and functional relations, necessary or assumed by the tribe, as the case may be. (1923:63, emphasis mine)

Thus, it is clear that Wissler envisioned the coherency aspect of unity of trait-complexes as due to logico-functional interrelatedness, but it

is not clear whether this interrelatedness is from the observer's or the natives' point of view. From the last quote above, it would seem that it is sufficient to take the insiders' point of view on functional interrelatedness. But, Wissler added "that while some traits in the complex are indispensable, others are not" (ibid.:64). Here, the recognition that each person in a tribe, or each tribe in a culture area, may have variations on a trait-complex has been taken account of, but it has clouded the relatively simple vision of a culture complex's unity being based on logico-functional relations. As soon as one begins looking at the various manifestations of a complex in its distribution, one is faced with the problem of multiple "insiders'" points of view: will the real trait-complex please stand up! Wissler's solution to this objective-subjective problem is to treat trait-complexes as variables which derive from common cores, or norms (ibid.:53).

Granting the wrinkles in the picture caused by variation, Wissler's mode of thinking about trait-complexes is fairly straightforward. A trait is actually a complex whose components are held together to create the unity through logico-functional interrelatedness, more or less. The "atoms" of culture thus congeal into at least trait-complexes. The next question is, naturally, to wonder about associations between and among trait-complexes, such as maize and pottery.

> Now it appears that there is a functional association of some kind between the unit processes within one of these complexes--they are at least knit into a logical whole. . . . Yet it is not apparent that one of these is necessary to the other. . . . The coincidence, then, between the distribution of the maize-complex and the pottery-complex can scarcely be due to any objective functional relation between the two complexes themselves. (1917: 385)

> . . . we are left with a presumption that there are no direct functional relations between the several trait-complexes constituting a culture. The safest assumption seems to be that they are found together because historical causes operated to bring them into juxtaposition within the life of the group. (ibid.:386)

> So it is the time element that contributes to the association of trait-complexes and accounts for their coincidence. . . . this is what anthropologists mean by historical explanations for culture. (1923:66)

Thus, Wissler conceives of complexes as being held together by internal relatedness of components (functional), but they need not have any functional or logical relations with other complexes in the culture, although they may. The bushel of potatoes image of culture has been modified by introducing the notion of complexes, but the major modification of this image is rooted in the idea of "pattern."

The pattern concept is usually important whenever one examines distributional data and discerns the almost inevitable variations.

> The intensive analytic study of single trait-complexes has developed the pattern theory. The conception is that in certain phases of culture each social unit develops a style, or pattern, for its traits and that borrowed traits will be worked over to make them conform to this pattern. (1917:377)

"Pattern," for Wissler, meant the "universal pattern." This universal pattern was essentially an arbitrary outline of nine major headings with sub-headings. Its strength lay in the fact that any culture could be described in terms of the outline, that is, every culture had speech, material traits, art, property, etc. Wissler did not feel, however, that the "universal pattern" was arbitrary, rather he thought of it as a reflection of the psychic nature of mankind. This point is interestingly made in a comment on the power of the historical method, which, remember,

is the presumed mode of accounting for associations of trait-complexes in a given culture.

> So while the historical method can account for the concrete content of tribal cultures, it fails completely when confronted with the culture pattern. . . . so we must reckon with the possibility that the pattern for culture is just as deeply buried in the germ plasma of man as the bee pattern for the bee, and may assume, as a working hypothesis, that a human being comes into the world with a set, or bias, to socialization, according to a definite pattern, and that this bias is firmly rooted in the germ plasma by reason of which man is a human being and not a termite, a bee, nor even a monkey. (1923:264)

Wissler saw the universal pattern acting on culture content (trait-complexes) in two distinct ways (although he never pointed out this duality himself). The first imagery employed is that of a blueprint as opposed to the actual building:

> If we should liken trait-complexes to building materials, then the plan of the house to be built of them would correspond to the pattern of a culture. (ibid.:73)

This general conceptualization is again expressed in calling the pattern "the skeleton of culture" (ibid.:77).

The second image of the universal pattern's action on culture content evokes not a basic structure to which "flesh" is added, but rather a molding of content into structure.

> . . . culture comprises trait-complexes, and these meshed, or felted, under culture complexes, which is the name we have given to these divisions of the pattern scheme [universal pattern]. (ibid.:80, emphasis mine)

Arising out of this imagery is Wissler's second usage of the term "complex." In spite of the fact that the term "trait-complex" was introduced in the section of his book entitled "The Culture Complex," Wissler reserves the phrase "culture complex" for the headings of the pattern:

> We may properly speak of the pattern as made up of culture complexes: speech, art, religion, etc. (ibid.:78)

In Wissler's view of the nature of culture, the principal source of unification, of interrelation among elements, was the substratum of all culture: the universal pattern. This substratum was furthermore conceived to be rooted not in culture per se, but rather in the human "germ plasma." This extrinsic ordering source is distinct from later notions of pattern (e.g., Benedict and Kroeber) in that it does not take a relativistic point of view whereby one may speak of each culture having its own peculiar pattern. The universal pattern is supposedly manifested and operating in all cultures, stemming as it does from the general psychic nature of all humanity.

Before summarizing Wissler's contributions to the development of the idea of a culture complex, there is one last aspect of his thinking on the subject to be considered.

We have seen that Wissler considered a trait-complex to be held together through functional and/or logical interrelation of components. But, this in itself does not illuminate the origins or the stability of trait-complexes. His views on this are perhaps clearest in his article, "Aboriginal Maize Culture as a Typical Culture-Complex." Here, one finds Wissler thinking along lines that today would be subsumed under "cultural ecology."

> In other words, when a culture-complex once develops as an adjustment to a locality and works fairly well, it tends to persist in that locality and may prevail over complete changes in blood and language. . . . . the original elements in a type of culture began as adjustments to the locality in which the participating social group lived. When these adjustments worked, they were perpetuated. (1916:660)

> It is the food, shelter, and transportation complexes of material culture that the intruding group will take over bodily. Then the chances are that one by one the associated ceremonies always found intimately connected with food production will be taken over to displace those now made useless and ultimately drag in their social counterparts. (ibid.:661)
>
> . . . the environment is not a formative factor but only a perpetuator. The formative factors are to be sought in the initial social group. (ibid.)

It should be clear by now that Wissler shared many of Boas's thoughts regarding the nature of culture. He, like Boas, saw diffusionism, parallelism, and convergence as delimitations of the logical possibilities whereby cultural similarities may come about (1922:375-377). He acknowledged, implicitly, the role of psychology in determining cultural similarities. This is not an argued point in his writings, but it is nonetheless quite clear from the way he uses ethnographic examples. For example, he describes the wild rice complex of the Ojibway in terms of what they feel are related elements of the whole (1923:52). He was keenly aware that a particular culture was more than a mere aggregate of independent elements. And, he saw this patterning in culture as a product of psychic factors operating in humans everywhere. What then did Wissler write beyond Boas, with respect to understanding culture complexes?

First, and not so trivial as it may first appear, he provided a reasonably systematic terminology for talking about complexes and higher-order compounds of culture content derived from them: trait, trait-complex, culture type, culture area, culture complex, and universal pattern. Not all of these have been discussed in the preceding pages

because they are not directly pertinent to my interests, but Wissler systematized them in such a way that he could define the notion of a culture in terms of the interaction of a group of people (the tribe)--possessing by virtue of their being humans the universal pattern--and some amount of human culture (organized in terms of the trait-complexes).

Second, in presenting the idea of a trait-complex, Wissler described what he felt to be the nature of its coherency: logico-functional interrelation of the component elements. In doing this, he addressed directly a subject which Boas did not. It is well and good to say that trait-complexes are "units of culture," but it is necessary to do as Wissler did and propose some way of comprehending such unity. Whether one agrees or not with Wissler's thinking about the nature of a trait-complex's unity, at least it is fairly clear what one is dealing with.

Third, Wissler sharpened and detailed what would be for Boas the "psychic unity of mankind." The "universal pattern" may well be entirely inappropriate to thinking about culture, but at least one has some idea what it consists of and how it is supposed to operate in giving a culture global coherency and stability (as opposed to the local coherency which trait-complexes evidence).

All in all, then, I think it is fair to say that Wissler's contributions to the subject in question lie in his being specific in areas where Boas was a bit vague. This is not to say that Wissler is Boas equipped with a magnifying glass. For example, Boas was wider in his thinking about the nature of culture patterns in that he could accommodate both the idea of universal, pan-human, pan-cultural patterns as

well as the idea of relativistic patterns. Wissler locked into the idea of a universal pattern to the omission of other kinds.

Hallowell's (1926) study of bear ceremonialism in the northern hemisphere is an excellent example of someone applying the idea of a culture complex. It also evidenced a different mode of dealing with the problem of variation, when compared to Wissler's notion of a complex as a variable clustering about a norm. Although not really a theoretical paper with respect to culture complexes, Hallowell's work is illuminating on the subject.

He purposely selected a "psychological" complex (bear ceremonialism) in order to examine then current theories of religion which had their roots in suppositions regarding the interrelation of man and environment (especially those theories based on the existence of "primitive philosophers"). He was thus immediately concerned with indegenous beliefs, attitudes, feelings, etc. of the peoples toward animals and not just with how the animal world of each people was used and exploited in survival efforts. In doing this, Hallowell could highlight the problem posed by very widespread cultural similarities (the distribution of the complex). For, in dealing with a "psychological" complex, an explanatory framework based on notions of environmental adjustments would seem a bit far-fetched (when they do not, for example, in Wissler's thinking regarding the stability of the maize complex), or, at the least, indirectly applicable (à la ideas of "structural dominance," cf. Friedman 1974).

Hallowell began his examination of data by asking how he could talk about a thing such as bear ceremonialism as a unitary phenomenon in the

face of the range of differences in its manifestations. How could he think of it as a thing with manifestations rather than as multiple things? After noting the weak justification that he was not the first to do so, he listed the following basic features of the complex: people in North America and Asia regard bears in a more of less unique way, bears are hunted in certain manners, certain rites in the treatment of the carcass and in consumption are widely found, and the treatment of the bear's bones are widely similar (1926:21-23). This, then, was the initial justification for speaking of "bear ceremonialism" as a real rather than superficially apparent complex.

Following the initial justification for thinking and speaking of bear ceremonialism as a phenomenon, Hallowell systematically reviewed literature bearing on his subject complex. In summary, this literature review did not yield a single, replicated constellation of features throughout the reaches of the northern hemisphere. There were many differences in the ways different peoples felt, acted, and thought toward bears. But, in spite of the great deal of variation, Hallowell was able to discern that the core of the bear ceremonialism complex coincided with the distribution of northern Boreal hunting peoples. As one leaves this distributional area, the core of the complex (defined in terms of the initial features, but sharpened up somewhat) has only partial representation and/or additional features. In other words, the complex has undergone selective borrowing by the peoples surrounding the Boreal hunters (ibid.:154). (This final interpretation was made only after refuting alternative explanations based on "some simple psychological formula" or

on observations of the bear's natural traits or on the bear's usefulness to the people.)

Thus, Hallowell took a quite variable ceremonial complex, identified a core of the complex (by specifying a level of similarity which he regarded as sufficient to call similar instances representations of the same complex), and accounted for the residual cases of only partial manifestation. The conclusion reached was that for this particular complex the best explanation of the distribution was historical factors rather than pan-human psychic laws. This sentiment was nicely expressed as follows:

> Each culture exhibits its own peculiar combination of features which cannot be deduced from any general principles of association. (ibid.:17)
>
> . . . man does not envisage natural phenomena afresh, but looks out upon . . .[them]. . . through the cultural spectacles with which the accident of birth has provided him. (ibid.:18)
>
> . . . "Nature" as we in our sophistication like to abstract it, never exists as a stimulus, unaccompanied by a host of traditional associations, the individual items of which have their historical roots in a still more remote past. . . . Therefore, the explanation of customs and beliefs by some simple psychological formula, couched in terms of "individual psychology," whether applied to origins or later developments, is putting the cart before the horse. . . . The source of their beliefs and practices is, therefore, the historic tradition (culture), and the history of particular customs and beliefs must be pursued at the cultural, not the psychological level. (ibid:19)

Hallowell's conclusion that the distribution of bear ceremonialism should be explained in terms of historical processes is not quite legitimate because he came to this conclusion by defaulting parallelism-style alternatives, but he did not refute possible convergence arguments. In

spite of this shortcoming, we may accept his conclusion. Yet, in accepting "historical factors or processes" as the explanation, we still have a problem since it is unclear how such postulated processes are supposed to work in bringing about the distribution, whether by migration, diffusion, stimulus diffusion, common origin, etc. Yet, even if it were possible to identify which of the range of types of historical processes were involved in bringing about the distribution of the bear complex, there is still no explicit reason why the whole complex should not just disappear or radically modify in the course of a hundred years, a generation, a month, or even a few minutes. Underlying such high-level "historical processes" are thousands, perhaps millions, of individual persons, each of whom had to get a prescription for his "cultural spectacles." Not only do such high-level historical processes presume that an individual maintains the bear complex in himself for the duration of his life, but also that his progeny do similarly. Concern with this ramification of "historical" arguments is evident in later work by Hallowell, and to this we shall now turn.

Hallowell's early interest in "psychological" complexes, especially those characterized as magico-religious, led him to the problem of making the "irrational" reasonable. Arguments based on natural selection seemed to him inapplicable to this realm of culture if they were supposed to be determinate. In addition, we have already seen how he rejected notions of man as a "primitive philosopher" as a mode of accounting for religious phenomena--this line of thinking ran into counter-examples in the distributional data. Yet, surely, he reasoned, belief systems are not completely free of the world of experience:

> . . . it may be assumed that any system of beliefs, insofar as it involves an interpretation of the phenomenal world, is recurrently and, in fact, inevitably, subject to challenge on empirical grounds. Granting for the moment that this is the case, how do beliefs react in the crucible of experience? (1934:392)

Although neglecting the problem of origination of religious beliefs, Hallowell was clearly concerned with the problem posed by the continuance, or stability, of the "irrational." He sought the solution of this problem in the relation between culture and the personal experiencing of the world. The conclusion was that people interpret their experiences rationally, but from differing vantage points ingrained in them through participation in their culture, which itself is alive with history.

> Experience and belief must be harmonized if beliefs are to be believed. The Indian is no fool. He employs the same common sense reasoning processes as ourselves, so that if he firmly holds to certain beliefs, we may be sure that they are supported in some degree by an empirical foundation. Thus experience is obviously the crux of religious rationalization. But dogma furnishes the leverage which makes the reconciliation of belief with experience possible. . . . Since the fundamental assumptions of any religious system are those usually least transparent to its adherents, they are able to retain a relative stability even when the more superficial beliefs of the superstructure are modified. It is in the toils of these implicit underlying tenets that the individual mind is caught. It cannot escape them through the ordinary processes of reasoning because it is uncritical of the assumptions they invoke. . . . Thus it comes about that although experience is consciously recognized by the Indians as a means of verification of beliefs, experience itself is unconsciously interpreted in terms of traditional dogma so that in the end, specific beliefs receive a satisfying empirical support and dogma and experience are reconciled. (ibid.:393)

To summarize Hallowell's contributions, one may recognize two major areas of development. First, Hallowell distinguished between cases in the distribution of a complex with respect to whether they manifested the

whole complex or only partial versions. This mode of coping with the problem of variation in a supposedly unitary phenomenon is quite distinct from Wissler's complex-as-a-variable. Wissler (from his direct statements on the matter) would have to regard each occurrence of a complex as equally representative data from which one must extract the norm. Thus, the norm is a composite. Hallowell, on the other hand, conceptually recognizes the complex as a whole which may or may not be completely manifested in a particular case, but is represented in toto in at least some cases. Second, Hallowell was concerned with the ways in which "history" operates in producing cultural stability. Historical forces provide the individual with the "implicit underlying tenets" which make possible the harmonization, reconciliation of beliefs and personal experience. Culture, an historical product, acts as an intermediary between the individual's eyes (basic rationality characteristic of humanity) and the empirical world which the individual sees. History gives the individual his "cultural spectacles," which go largely unnoticed by the individual seeing the world through them. Thus, for Hallowell, the basis of cultural stability is the degree to which the individual incorporates the basic dogma(s) of his culture and does this unreflectively.

Benedict is closely associated with the idea of pattern in Anthropology. And, as Wissler noted, the pattern concept grew out of the tracing of complexes in their distributions. Yet this connection is not clearly made. In fact, the phenomenal base for the pattern concept has already been mentioned: the observed variation in complexes depending

upon the cultural milieu in which they are found. This phenomenal base is clearly recognized in Benedict's (1923) early work on the guardian spirit complex in North America.

Like Hallowell, she concerned herself with a "psychological" complex. From the outset, Benedict was keenly aware of the difference between historical and psychological unity.

> It has always been rare to find this distinction recognized. The fact of historical connection has usually in theoretical discussions been regarded as involving a psychological unity of rôle. The two aspects are, however, by no means equivalent. Historical unity we can read from the face-value of the facts of distribution; psychological unity or lack of unity, on the contrary, can be determined only by a critical analysis of its connotations, and of the rôles which the concept plays in cultural life. It is exceedingly difficult to realize in its full force the divergence of these connotations and rôles in the case of the guardian spirit concept. . . . In spite of this, it is nevertheless true that each of the culture areas of North America has a certain identifiable interpretation of the guardian-spirit idea. . . (1923:9-10)

The "core" of the guardian spirit complex rests on the more basic complex of the vision. Guardian spirits, however concretely conceived of in a given culture, were merely an intermediate step in the logic of the vision (ibid.:29-30). The vision itself, however, had various concrete versions--it could be sought after, seen as just an accidental happening, regarded as an hereditary propensity, or whatever. So, in speaking of the "vision" or of the "guardian-spirit," Benedict created unities through abstraction from diverse particulars. The uniting property of the vision was that regardless of how or why it was believed to happen, a vision gave the person some sort of control of supernatural power. Similarly with the guardian spirit, it was unimportant whether the spirit

was hereditary, one of an indefinite number possible, one of a finite set of possible spirits, belonging to one individual or to a group. The property that warranted regarding the spirit as a guardian-spirit was that it became associated with the person through a vision. Thus, Benedict defined the guardian spirit complex as an abstracted system of beliefs, itself a pattern. In addition, she felt that the religious experience of a vision, given its continental distribution "necessitates some psychological unity in the observed phenomena . . . Has not the religious experience itself its corollary in belief and practice?" (ibid.:43). In other words, it was possible to conceive of the guardian spirit complex as a unitary phenomenon psychologically as well as historically. This done, the question naturally arose of what, if any, other cultural traits co-occurred with this vision-guardian-spirit complex.

From examination of the data, Benedict concluded that there was "no observed correlation between the vision-guardian-spirit concept, and the other traits with which it is associated, as it were organically, over the continent, and we have found no coalescence which we may regard as being other than fortuitous--an historical happening of definite time and place. The miscellaneous traits that enter in different centers into its make-up are none of them either the inevitable forerunner, the inevitable corollary, or the inevitable accompaniment of the concept, but have each an individual existence and a wider distribution outside this complex." (ibid:84)

The ultimate conclusion of her work on the guardian spirit complex was not limited to the case at hand. The conclusion rather advocated a Boasian vision of the nature of culture:

> It is, so far as we can see, an ultimate fact of human nature that man builds up his culture out of disparate elements, combining and recombining them; and until we have abandoned the superstition that the result is an organism functionally interrelated, we shall be unable to see our cultural life objectively, or to control its manifestations. (ibid:85)

Thus, as of 1923, Benedict was thinking of culture as hodgepodge of "disparate elements." She came to this viewpoint from the study of a complex's distribution, the sort of study that forces the analyst to cope with the double edge of unity with diversity--variation. More specifically, she came to her conclusion only after distilling a unity from the diverse manifestations through a process of abstraction. This was the standard operating procedure for anyone studying a cultural complex rather than the culture of a people, and usually the result was that the analyst became impressed with the force of history. An exception is Van Gennep's (1969) study of rites of passage. But, a residual problem of this type of study is what to make of all the concrete, local, and diverse occurrences of the complex which constituted the data base. It is with respect to this residual problem that the pattern concept exhibits its explanatory power.

The fundamental image of Benedict's conceptualization of culture patterns is the "great arc" of cultural possibilities from which each culture must select. Once a given culture has selected its contents from the "great arc" (this being accomplished in the historical processes

the society has been engaged in), the selected cultural contents interpenetrate with one another to a greater or lesser degree yielding a whole-culture pattern. In such a framework, one may comprehend the diversity of human cultures.

> The diversity of culture . . . is due even more to a complex interweaving of cultural traits. The final form of any traditional institution . . . goes far beyond the original human impulse. In great measure this final form depends upon the way in which the trait has merged with other traits from different fields of experience. (1959:37)

Thus, the distinctiveness of the guardian spirit complex in the Northwest Coast area is due to the meshing of the complex with the adolescents' ceremonials, whereas among the Osage the complex merged with the clan system. In the two areas, the difference is not as much a difference in cultural contents (although there is this) as it is a difference in the range of interpenetration of various complexes (ibid.:39-42, 1923).

The interweavings of cultural complexes are not, however, fixed and rigid. They can modify, become more elaborate, or disappear altogether in the course of time. Benedict did not assume any form of inherent stability with respect to such associations.

> These interpenetrations of traits occur and disappear, and the history of culture is in considerable degree a history of their nature and fates and associations. But the genetic connection we so easily see in a complex trait and our horror at any disturbance of its interrelationships is largle illusory. The diversity of the possible combinations is endless, and adequate social orders can be built indiscriminately upon a great variety of these foundations. (1959:44)

The overall image of a culture, then, is a particular mode of meshing or felting particular traits (complexes) during a particular range of

time in a particular region of space. Any or all of these particulars may change, but Benedict would see that the most important aspect of a culture is the mode of organizing the contents. This is what is responsible for cultural integration, which may be variably achieved in different cultures or in the same culture at different time periods. This is the "configuration" of the culture. Benedict likened the integration of culture and the integration of personality:

> A culture, like an individual, is a more or less consistent pattern of thought and action. Within each culture there come into being characteristic purposes not necessarily shared by other types of society. In obedience to these purposes, each people further and further consolidates its experience, and in proportion to the urgency of these drives the heterogeneous items of behavior take more and more congruous shape. Taken up by a well-integrated culture, the most ill-assorted acts become characteristic of its peculiar goals, often by the most unlikely metamorphoses. (ibid.:46)

In other words, the configuration of a culture, especially if well-developed, recasts incoming complexes so as to make them consonant with the established patterning of existent culture content. (This would account for the extreme variation in the guardian spirit complex, for example.)

Benedict was a strong adherent of the concept that the whole is more than the sum of its parts (ibid.:47). But, she insisted that this view is not mystical. There are definite processes which produce cultural integration, hence configurations.

> This integration of cultures is not in the least mystical. It is the same process by which a style of art comes into being and persists. (ibid.)

Thus, in the case of the development of Gothic art, there were at first merely slight biases, but as time went on these initial predilictions

expressed themselves more and more and evolved into "definite standards." At no time was there necessarily conscious choice or purposes (ibid.:48).

There is a basic weakness at this point in Benedict's thinking about whole-culture patterns, or configurations. The essentials of the concept have been revealed as well as the power of the concept in accounting for cultural variation. But, nowhere has there been an explanation of what is supposed to drive a culture towards integration, pushing it ever on toward higher levels of elaboration and interconnectedness. This is especially severe as a criticism in the face of second-order cultural variation: cultures vary in the degree to which they are integrated and hence have consistent, finely honed configurations. Benedict herself was aware of this second-order variation:

> All cultures, of course, have not shaped their thousand items of behavior to a balanced and rhythmic pattern. . . . This lack of integration seems to be as characteristic of certain cultures as extreme integration is of others. (ibid.:223)

Assuming that this second-order variation exists empirically and is not just a reflection of differences in the personal temperaments and/or theoretical positions of the ethnographers who have generated the "data," it is indeed curious that Benedict did not posit some sort of mechanism(s) for the integrative drive which is so fundamental to the whole notion of configurations. Bateson's "schismogenesis" (1935, 1967a) is such a "mechanism" available to fill Benedict's conceptual vacuum in this area.

In spite of this weakness, Benedict's concept of cultural configuration, or relativistic whole-culture pattern, is a powerful conceptual tool. First, although primarily concerned with the synchronic interrelatedness of culture content, a configuration is seen as an historical

product. And, furthermore, it has embedded in it restrictions on the potentialities for further development. Second, configurations are group phenomena; they do not have their source in each individual, but rather each individual partakes to a greater or lesser extent of one or more configurations. Benedict's conception of a configuration is here different from both extreme superorganic views of culture and extreme individual psychology views of culture.

> The group is fed by tradition; it is 'time-binding.' (1959:231)
>
> . . . the crux of the matter is that the behavior under consideration must pass through the needle's eye of social acceptance, and only history in its widest sense can give an account of these social acceptances and rejections. (ibid.:232)

The configuration concept is not divorced from individuals, it is intimately concerned with the individual, but it puts constraints on the probability of acceptance or rejection of individual developments in thought and action. Thus, Benedict holds that one's understanding of human behavior must move from the configuration to individual psychology, not conversely (ibid.). In this line of thinking, Benedict followed Rivers (1926).

From the foregoing, it should be clear that Benedict's contribution to developing the notion of culture complex is somewhat tangential. Instead of worrying about what gives a complex its unity, she tackled the reverse problem: what makes a complex different in its manifestations. In the guardian spirit study, Benedict essentially followed Wissler's mode of comprehending a complex--she abstracted a common

denominator. In so doing, she created a "unity" but simultaneously was left with a morass of variation. To account for the variation, she developed the concept of configuration. The configuration concept, however, not only is a mode of comprehending variation in manifestations but also provides a framework for understanding cultural integration, selective borrowing, the processes of cultural growth, and the relation between culture and individuals. In spite of the obvious power of the configuration concept, it is unclear how it aids in the understanding of a complex. Complexes are the constituent parts of the various configurations, and Benedict is relatively explicit about how complexes will appear in altered forms depending on which configuration they are in. But, one is left wondering how to recognize a complex as a unity through the diversity of its representations. Would Benedict claim to have improved our understanding of the guardian spirit complex? Or, would she say that her 1923 study illuminated not the subject complex but rather the range of cultural diversity in North America? Or, perhaps she would say that recognizing the range of variation and accounting for it constitutes an understanding of a complex? This later idea seems most likely.

Herskovits's study of the cattle complex in East Africa (1926) is another classic use of the notion of culture complex. Here the primary interest was to use a large, widely encompassing complex to define a culture area and in doing this to illustrate the difference between the "culture area" of American Anthropology and its Kulturkreislehre counterpart. For the present purpose, however, we shall focus on how Herskovits

thought about the complex itself.

Herskovits, like other American authors of the era, was clearly conscious of the difference between historical and psychological unity. He, too, was adamant that complexes must be psychologically similar if one is to regard them as instances of the same complex.

> Are we justified, then, in claiming that merely because similar complexes of material traits are discovered in various areas, we can regard these traits as the same? If enough of them can be shown to be alike in form, and these elements are complex enough, we may be able to posit unity of origin and thus fulfil the object of the kulturkreis, but in the sense of the culture-area, this is not enough. We must insist on sketching the culture-area not only on the basis of actual occurrences of similarity but also according to the psychological value those traits hold for the culture-bearers. (1926:242)

He began his investigation with the obvious point that the cattle complex depends upon the presence of cattle. But, this, by itself, was insufficient to delimit the geographical range of the complex. The important features of the cattle complex in East Africa were the social usages of cattle and attitudes towards them. Thus, the distinguishing features of the complex were things like paying brideprice in cattle, a high symbolic value attached to cattle over and above their economic value, etc. With this sort of constellation of definitive features, Herskovits was able to identify a more or less continuous distribution of the complex. There were, of course, local differences within the complex's distribution, but he felt there was a discernible core of similarity, especially when opposed to other areas which also had cattle.

Contrasting Herskovits's study with Benedict's and Hallowell's, there are several differences. First, he focused on a complex which had

roots in a form of subsistence. Although the cattle complex was riddled with "psychological" characteristics, it was not purely psychological. Second, like Hallowell but contrary to Benedict, Herskovits provided a rather elaborate and rich description of the complex; it was not a single, distilled idea or concept, but a constellation of attitudes and practices. Third, like Benedict but contrary to Hallowell, he did not impose a distinction between complete and partial manifestations of the complex. Fourth, while Benedict became engrossed with the range of variation evidenced in the distribution of the guardian spirit complex, Herskovits was primarily interested in giving the impression of unity within a single culture area, which he was defining. This, then, was Herskovits's early work with the idea of a culture complex. It was solid enough, but not very new or original conceptually.

Herskovits's (1967) general text is considered by many to be one of the clearest, most direct commentaries on the subject of culture complexes in the literature. In spite of this relative position, it is not particularly explicit on some of the issues which have dominated this review. For example, Herskovits leads into the idea of complexes as follows:

> Frequently, not the single trait, but a grouping of traits found to exist in close relationship within a given culture must be the object of study. It is these grouping of traits that comprise the describable outlines of the way of living of a people. As aggregates, they form what are called culture-complexes . . . (1967:176)

To illustrate culture complexes, Herskovits says that "Almost any folk-tale forms such a complex" (ibid:177), and he proceeds to examine the Cinderella

story as a concrete example. He points out that the specific story is quite variable--for example, Cinderella may lose her slipper at midnight leaving a ball or at noon leaving a church--but overall the story is the same.

> The complex represented by the tale as a whole is everywhere recognizable. (ibid)

What I should like to know is how we are able to recognize the Cinderella story and relegate the numerous differences to the status of versions of the same story. Herskovits is clearly opaque on this subject. He seems content to note that "It is possible to follow the manifestation of a given complex from tribe to tribe" (ibid.) without wondering how this is possible.

He begins his "Section 5" with the following tantalizing sentence:

> Let us consider the integrative factor that gives to a cultural complex its unity, no matter what fortuitous traits seemingly comprise it. (ibid.:179)

But, instead of providing a clear, unambiguous statement of how he conceptualizes a complex's unity, he lapses into answering the question by example. The specific example is the men's co-operative work group in Dahomey called the dokpwe. This complex is made up of component traits such as pride in membership, the work of hoeing fields, the feast in payment of labor, the type of music accompanying the work, etc., etc. In accounting for the underlying "logic" of the dopkwe complex, Herskovits relates its multifarious features to the overall economic enterprises in which the dopkwe engages. The dopkwe is also involved in funerals, but here the emphasis put on cooperativeness in the economic sphere is

replaced by a concern for payment for individual services rendered. The overall conclusion as to the nature of the complex's unity appears to be that the complex forms a unity because the Dahomeans think it does. In going through the example, Herskovits has merely tried to retionalize the ways the Dahomeans themselves think about the complex. Thus, he concludes:

> In any culture-complex, little objective relationship between traits, either on the basis of logic, or need, or desirability, is to be discerned by the outsider. (ibid.:181)

What is especially interesting are the logical steps by which Herskovits came to the above conclusion (which, as we have seen, is a common conclusion).

Herskovits's initial definition of the dopkwe complex is that it is a kind of men's co-operative work group. He then proceeds to add some of the associated traits of the Dahomean data. Next comes an explication of how all of these traits make sense as a unity to the Dahomeans, or at least make sense in the Dahomean context. At this point, it would appear that the dopkwe complex consists of all the enumerated traits and not just the core feature of being a men's co-operative work group. But, just when the dopkwe complex in Dahomey has been described enough to begin making sense to the reader, Herskovits compares it to the Ashanti's form of men's labor organization, the asafoche, which is devoted to political and military activities. From the comparison he concludes that the dopkwe complex's association of component elements is not objectively understandable. One may well ask what is the basis of the comparison. The

answer is clearly the fact that both the dopkwe and the asafoche are men's communal organizations. Are, then, the dopkwe and asafoche local variants, versions of some "men's communal organization complex," or, is each a complex in itself? Supposing we take the view that the dopkwe and asafoche are two separate complexes, then could we still reach Herskovits's conclusion that a complex's internal relatedness is not objectively motivated?

I would suggest that the problems raised above follow naturally from what appears to be Herskovits's hidden assumption (from which he occasionally vacillates) that a complex can be defined only within a single culture area, or perhaps only within a single culture. Depending almost entirely on how specifically one defines a given complex, differences, when compared to roughly similar complexes in different cultures, will be taken to indicate either "versions of the same complex" or "different complexes." The more specific one's definition of a complex, the more likely it is that the complex will be found to exist in only a single culture-type or even a single culture--this simply follows from the nature of classification. As we have seen, Herskovits's early work was concerned with using the notion of culture complex in order to define a culture area. Also, he took a complex which was so widely encompassing as to approximate a whole life-style; it almost constituted a configuration. Yet, in his general discussion of culture complex, Herskovits allows for single folk-tales, the guardian spirit complex, the kula ring, and so forth. So, it is apparent that Herskovits applies the word "complex" to many different levels of organization. The only criterion, or

imputation, of usage seems to be that insiders must feel that the proposed "complex" is a unity, whether the insiders be of a single culture, a culture-type, or culture-types. How specifically one defines a proposed complex evidentally depends entirely on one's problem or purpose in defining the complex with the single stipulation that it must have psychological unity for the people who manifest it in their culture. Herskovits, himself, seems to have preferred being very specific in definition of a complex, thereby staying closer to actual empirical cases of it, closer to the ethnographic richness and more removed from distilled, abstracted culture content. This preference for richness in specifics is the logical source for his conception of a culture complex as a pattern. Widely encompassing complexes approximate "configurations" precisely because the pattern of the complex takes in nearly the whole culture. Yet, complexes are distinct from configurations, conceptually, because the pattern inherent in a given complex may or may not operate over the whole range of culture content, e.g., the Cinderella story. Herskovits calls this quality of complexes their "kaleidoscopic aspect" (ibid.:177).

In summary, then, Herskovits explicitly conceived of a complex as more than a simple hanging together of elements of content. This transcendent quality he called their kaleidoscopic aspect, which for other writers (e.g., Kroeber) was captured in the idea of pattern. But, unlike the whole-cultural pattern of Benedict or the pan-culture universal pattern of Wissler, Herskovits's pattern-in-a-complex was variable in its empirical scope. The assumption was that the subjective unity characteristic of a complex was based on the coherence of a pattern. The

Cinderella story is the Cinderella story despite its multiple versions because there is an underlying common pattern, similarly for the cattle complex of East Africa, but on a much larger scale. Pattern, however, is one of the most general of concepts and says little more than parts have characteristic arrangement, of some sort. Thus, to claim that complexes are oriented around patterns is a statement of faith which must be explicated concretely in each case. In spite of this shortcoming, conceiving of complexes as patterns of variable scope is an advance over conceiving of complexes as simple associations of elements.

In reviewing Wissler, Hallowell, Benedict, and Herskovits in sequence, I have followed a rough line of development of the idea of a culture complex. All four are famous for their uses of the notion. All four were leaning in the direction of comprehending complexes as patterns, at least implicitly. This is not to say that each conceived of complexes in the same way, as I hope the reader is by now aware. But, overall, their similarities are representative of a particular thrust both in the kind of use the idea of culture complex has had in anthropological writings and in a mode of comprehending the nature of complexes. The study of culture complexes has highlighted the phenomenon of cultural variation, variation being diversity within an overarching unity. Recognition of this phenomenon lead to the development of the concept of culture pattern as a mode of accounting for cultural variation. Before returning to the "flower" of the culture pattern school, Kroeber, I should like to turn to another major line of development. This second intellectual trajectory is oriented around the problems posed in speaking of cultural similarities,

but, as in the foregoing, I shall not hesitate to include somewhat tangential points arising in particular authors.

The issue tackled in Lowie's (1912) article was of crucial importance for all anthropological work irrespective of particular theoretical orientation. Although phrased as a defense of the principle of convergence, the real issue was how to determine genuine cultural similarities as opposed to generating "similarities" as artifacts of analysts' classificatory schemes. Lowie's article followed in the footsteps of Boas (1966b), and like Boas, Lowie found a living "straw man" in Graebner.

Instead of arguing with Graebner on the basis of which explanatory framework should be used to account for observed distributions of cultural similarities (although this was done also), Lowie challenged Graebner at the very heart of the distributional question: how does one determine distribution in the first place (1912:31). For, a distributional plotting can be done only after a determination of cases which are similar. Thus, Lowie, like Boas, challenged Graebner's criterion of form. Usage of this criterion alone, claimed Lowie, yields "premature classifications."

> Our first duty is rather to ascertain whether the resemblances are superficial or fundamental. (ibid.:39)

Lowie noted two forms of premature classification:

> Premature classification appears in ethnological literature in two principal forms: the ethnologist may either infer from the undoubted identity of certain elements in two different complexes that the complexes themselves are identical; or he may fancy identity of elements or complexes where none exists. (ibid.:32)
>
> Comparing the two types of inadequate classification, we may say that the first type involves the assumption that an organic relationship exists where it does not exist,

> while the second type of error results from the failure to note that the supposedly parallel elements are organically related to two distinct complexes. In this latter case, then, the parallelism is between logical abstractions rather than between psychological and ethnological realities. (ibid.:33)

Adherence to the criterion of form results in a lumping together of disparate elements along with perhaps truly similar units of culture content. The point is that culture consists of more than just the formal, sensible, readily observable aspects of behavioral artifacts.

> The sensible traits of an ethnographic object may completely determine its character from the standpoint of the curiousity-dealer, but never from that of the scientific ethnologist. For the latter a material object has a purely symbolic function: it represents a certain technique, an artistic style, a religious or social usage. . . . the very word "culture" implies a psychogical correlate, or rather determinant, of the material object. (ibid.:39)

Thus, Lowie rejected a view of culture as consisting of nothing but abstract forms and argued for thinking of culture in terms of forms, yes, but forms with sentiments, emotional tones, feelings, and associations attached which cannot be ignored. This "psychological" dimension of culture is a necessary variable to consider before postulating any sort of cultural similarities.

> . . . we must first inquire whether the supposed identities are really such, or become such only by abstracting from the psychological context in which they occur, and which determine them,--whether, that is to say, we are comparing cultural realities, or merely figments of our logical modes of classification. (ibid.:41)

Now, it is very unclear what Lowie may have meant in speaking of cultural identities. Anything less than "identity" would, of course, be relegated to varying degrees of similarity. Within the range of

"similarity," however, Lowie distinguished true resemblances from "false analogies." Yet, he did not specify how similar two or more instances of a cultural feature have to be in order to justify regarding them as "fundamental" similarities as opposed to false analogies. This problem is perhaps a red herring. The important point is that the student of culture must not ignore the psychological dimension, else it is very likely that diverse phenomena will be taken together as a unit and thereby seriously distort subsequent analysis. In his chapter on the determinants of culture (1917), Lowie seems to have recognized the logical consequences of his earlier phrasing of the problem and acknowledges that perhaps the point had been overmade, for simple recognition of a cultural fact involves classification which necessarily entails selection of certain features, among the perhaps infinite number possible, as diagnostic thereby glossing over differences which may exist. Here once again is the problem of specificity of description.

Just in case one should think that this problem of description is restricted to those who would think of human culture in terms of units of culture content, it is perhaps worthwhile to take note of Lowie's (1936) article.

One may temporarily hold that the notion of a culture is free from this line of criticism, but this would be an error. How is a culture defined and bounded (cf. Moerman 1965; and almost any modern introductory text, e.g., Keesing and Keesing 1971)? What one finds is that the notion of a culture is quite dependent on ascertaining the boundaries (conceptually, and, optimally, spatially) of the unit, which means that one is

again confronted with determining how similar does similar have to be to be the "same." Lowie was aware of this.

> Each culture, it is averred, is a closed system to be viewed only as an organic whole. . . . In the first place, a specific culture is an abstraction, an arbitrarily selected fragment. . . . There is only one cultural reality that is not artificial, to wit: the culture of all humanity at all periods and in all places. (1917:305)

> The gist of the matter is simply this. Isolated facts yield no insight; but nothing warrants the assumption that all significant relationships are confined within the system of a particular social tradition. To wrest a fact from its tribal context is no more arbitrary than to wrest the tribe from its contacts with the rest of humanity; than to isolate the extremeties of vertebrates for comparative anatomical study when we all know that the limbs never exist except in association with the organism as a whole. (ibid.:306)

In addition to realizing the fundamental importance of classification in the study of culture, and insisting that the problem was the same regardless of whether one wanted to study complexes or cultures, and insisting that the emotional attachments, sentiments, and other psychological associations were just as important if not more so to the determination of ethnological classifications as form, Lowie is interesting for his views on the relation of psychology and ethnology and for his distillations of several important qualities of culture in general.

Although in large measure seeing psychology as concerned with subjects too general in nature to be of much value to the ethnologist, Lowie realized that it was imprudent to make too rigid a barrier between the two disciplines. His ethnological concern with the discipline of psychology took two forms: ethnological explanations should not run

counter to findings in psychology, and psychology may well be the only route to understanding origins of ethnological traits (1960, 1917). He rejected the notion that collective ideas were generically different from ideas arising from within an individual's mind (1960:190). The principal reason for this negative reaction to Rivers and Lévy-Bruhl was that they nowhere saw "products of individual minds becoming socialized but only social ideas shaping individual thought" (ibid.:195). To illustrate this counter-Lévy-Bruhl stance, Lowie used the data on sacred numbers which had been gathered by Galton. With this data, Lowie made the point that:

> . . . the endowment of Four with certain attributes must somehow have taken shape in an individual mind, and the acceptance of that individual evaluation of Four--its promotion from a psychological to a cultural position--is an example of the influence of the individual on the group. (ibid.:201)

The phrase that I find most interesting in Lowie's comments about the relation of ethnology and psychology is the idea that passing from the psychological to the cultural involves a "promotion." The idea seems to be that an individual's idea, behavior, or whatever, if accepted (adopted?) by other individuals, becomes a bit of culture. As such it may have properties not found so long as it belonged only to one person, but nonetheless, it is not "generically" different.

Perhaps the clearest way to illustrate Lowie's feelings about the overly generalized nature of psychology as a discipline for utility in ethnological concerns is to focus on his comments regarding diffusion of cultural traits. Lowie took as an example the manufacture of paper and pointed out that this trait does not spread through some process similar

to osmosis, a physical force underlying the spread of a trait. Rather, what one finds is that sometimes a trait spreads to neighboring people, sometimes not. "Cultural diffusion, therefore cannot be taken for granted. . . . We are face to face with a selective, with a psychological condition" (1917:10). But, psychology, although a large part of the phenomenon of selective borrowing, cannot, by itself, account for the content, the specific traits borrowed or not (ibid.:11). Following up this line of reasoning, Lowie concludes that culture is a thing sui generis (ibid.:17), but not totally removed from the constraining effects of psychological findings.

In his chapter entitled "The Determinants of Culture," Lowie identified two major features of culture: cultural inertia and secondary rationalization. Secondary rationalization is the ". . . rationalistic explanation of what reason never gave rise to" (1917:94). Lowie was not the originator of this observation about culture, but he phrased the sentiment nicely. "Cultural inertia" covers the idea that culture is bound by its history, and it is observable in two ways: (1) in the persistence of a cultural item once its original contextual surroundings have disappeared or dissipated, i.e., in survivals; and (2) in the recasting of newly accepted cultural items into the native patterns of the people receiving the trait (this being even more strikingly demonstrated in cases of selective borrowing). "Cultural inertia" thus has both a passive and active mode of manifestation (ibid.:83 and 92-93).

Summarizing Lowie's contributions to the subject of conceiving culture complexes, there are four points. First, he advocated the use of the

psychological dimension of cultural items in the determination of ethnological classifications, and in arguing this point, drew attention to the effects of "premature classification" on inferential history. Second, he pointed out that the problem of premature classification is a problem in the abstracting process through which we become able to talk about culture, whether we choose to talk about cultures or complexes. Third, he rejected strict separation of psychology and culture on the grounds that what eventually may become a social idea (and hence part of culture) must have its origin(s) in individual minds. Fourth, he very nicely captured the historical element of culture and the power of it in the phrase "cultural inertia." In other words, a culture complex is an abstraction, just as is a culture, which should be delimited by similarities in form and also in terms of similar meanings, emotional associations, etc., and it should be expected that this complex will persist once developed in spite of the fact that it may be modified each time it is taken up by an alien culture, and that furthermore, it may alter through the input of individuals' innovations.

Goldenweiser (1913) was perhaps the "final word" on the hoary issue of determining cultural similarities. The article was written as a clarification of Lowie's (1912) article on the plausibility of cultural convergence, and it dealt principally with problems arising out of Lowie's discussion of cultural identities. Goldenweiser was interested in how to determine, from cultural similarities, cases of genuine convergence from cases of false convergence, for

> The concept of identity, if applied to cultural, or more generally to any psychological traits, would, indeed, smack of mysticism. (1913:263)

His point was: to insist that similar cultural features have to be historically as well as psychologically and morphologically similar to constitute a case of genuine "cultural similarity" is to preclude the concept of convergence. Goldenweiser recognized that it would be impossible to ascertain identities, and that anthropologists can work only in terms of similarities. Given this, convergence, as an explanatory framework, may be either applicable or not in any given case, either genuine or false. Thus, whereas Lowie might well conclude that in order to establish cultural laws one would have to identify cases of repeated histories yielding morphologically and psychologically similar cultural features (cf. 1917:88 and 90), Goldenweiser felt that such a strict set of criteria would rule out the possibility of convergence, from the study of which one might also discover cultural laws (cf. Flannery 1972 or Steward 1955, as examples of the implications of following this point in Goldenweiser's position).

Goldenweiser then proceeded to distinguish three forms of convergence, which refined Lowie's dichotomy of true and false analogies: (1) "genuine convergence"--"the independent development of psychologically similar cultural traits from dissimilar or less similar sources, in two or more cultural complexes"; (2) "dependent convergence"--"those similarities that develop from different sources, but under the influence of a common cultural medium" (e.g., Newton and Leibniz developing the calculus, Darwin and Wallace developing the idea of natural selection, countless Western European children reinventing scattered ideas associated with Aristotle or some other early Greek); and (3) "false convergence"--"the similarities

between cultural traits are not psychological, but merely objective [morphological, formal] or classificatory" (1913:269).

As an a priori argument favoring the plausibility of cultural convergence, Goldenweiser enunciated the "principle of limited possibilities." The gist of his argument is that "The more complex a feature . . . the greater the number of its possible sources . . ." (ibid.:277). Thus, when one examines concrete actualizations of cultural features, one observes the outcome of a limitation on the possibilities of development, for the number of cultural features is far less than the number of their possible sources. Also, a culture at any given time manifests only a portion of all potentially available cultural features (note that this foreshadows Benedict's "great arc" image); and, once a selection of possible cultural features has emerged, it further restricts subsequent development in that it sets a range of cultural "precedent" (ibid.:270-271, note 1). That there are more possible sources for the development of cultural features than there are cultural features themselves constitutes a proof of the plausibility of convergence, for it makes possible the happening that a given cultural feature may develop from different sources. This argument does not, however, prove the existence of convergence, for it leaves it possible that all cases of cultural similarities may have come about through parallelism.

Regardless of one's feelings of attachment to Goldenweiser's "proof" (which rests on certain assumptions regarding the countability of cultural features, their recurrence in definite types, etc., cf. ibid.:274),

the article remains a highpoint in clear thinking about the nature of cultural similarities. It also introduced in direct fashion the notion that culture has limitations on possible routes of development. The basic limitations are twofold: (1) limitations arising from the psychic unity of mankind as evidenced in the susceptibility of all cultures to description "in a treatise containing sections with similar headings, more or less" (ibid.:271) (note the strong similarity with Wissler's universal pattern); and (2) limitations arising from the history of past selection of cultural features--the content and pattern of a culture at any given time--which are constraints in the form of the power of precedent. These constraints on possible cultural development are alike in that both are rooted in the general nature of culture itself. As such, they are internal constraints.

Goldenweiser contributed more directly to the idea of a culture complex, however, in placing the role of the psychological aspect of culture in perspective, by recognizing the problems involved in identifying a culture complex, and by confronting the problem of cultural originations. I shall take these in order.

Goldenweiser saw the psychological side of culture as an equally valid and important "inner view of the picture" compared to an enumeration of objective traits.

> The objective description of traits gives merely a static view, it presents a culture as if it had stopped in its tracks. The psychological view makes the picture dynamic; the culture moves again, it comes to life. . . . this psychological side consists of 'meanings' which include the interrelations of the traits comprised in the culture complex [in the sense of a whole culture]. (1937:477)

As evident in the 1913 article, Goldenweiser felt the psychological side of cultural facts was crucial for any classification. But beyond this is his view of the psychological side of culture as the integrative aspect of culture, or at least the aspect of culture that evidences integration. Only from this "inner view of the picture" do amounts of culture content coalesce and form synthetic unities. This view of the role of the psychological side of culture seems to follow from the general American emphasis on the inclusion of subjective features of traits in calculations of similarities, but it says a lot more.

With respect to the conceptual sleight-of-hand evident in the classic examples of complex-distribution studies (e.g., those of Benedict, Hallowell, and Herskovits previously discussed) in which the reader is first presented with a sketch of the distribution of a complex and then told that there are extreme variations (thus seemingly negating the rationale behind the plotting of a distribution), Goldenweiser finally made somewhat clearer what was happening and how the operation of plotting a variable-same-thing is supposed to work.

> When we examine a culture trait [trait-complex] as enmeshed in an area of tribal culture, its individuality is marred by the numerous functional, symbolic, and such like associations into which it enters with other traits of a local culture. When it comes to travelling, however, the individuality of a trait is restored. . . . What takes place in such cases may be accounted for by two reasons. The cohesion of a local culture is constituted by the functional relations of its traits. This is a purely psychological matter, a system of meanings. These meanings, intellectual in part but heavily weighted with emotion, are valid only for those of whose experience they are part, and who therefore can understand them. This does not apply to the outsider. (ibid.:482)

The second reason for the restoration of a complex's individuality in "travelling," according to Goldenweiser, lies in the differential ease with which tangible and minor social customs are dislocated from their context when compared to whole social systems or religions.

> The tendency to absorb an entire trait complex--not the entire culture, be it noted, but a tight clump of functionally knit traits--is illustrated by the history of what Wissler called the 'maize culture complex.' (ibid.: 486)

Thus, Goldenweiser seems to be saying that a complex has a core of functionally interrelated components, and it is this core which emerges as an individuality, a unity, when studied comparatively. But, in each concrete case, this core is subjected to psychological forces which act to associate many other extraneous elements with the core, both as a whole and as a set of elements (i.e., associations build up both with the complex as a whole and with individual components). This is implicit in Benedict's, Hallowell's, and Herskovits's studies, but they never bothered to formulate it in so many words.

Given that most American anthropologists who studied culture complexes undertook the task in order either to determine historical relationships among cultures or to test historicism versus parallelism, it is not surprising that the question of how the complexes developed in the first place seldom came up. Contributing to this playing down of the study of cultural origins were Boas's early arguments against environmental and economic determinisms and the whole anti-evolutionism (opposition to unilinear schemes) inherent in the notion of convergence, which, as we have seen, was a dominating topic of the era and which the American

anthropologists, starting with Boas, championed. In spite of this relative neglect, there were some who for various reasons attempted to provide generally applicable explanatory frameworks. Wissler proposed a general model in which the concept of "adjustment" was the center of the explanatory power. He also entertained the idea that some domains of culture were more susceptible to such "adjustment" than others, and that these other domains were loosely articulated with the modes of food procurement, housing, and transportation such that adjustments in these had ripple-like effects (possibly with time-lag) for the rest of culture. All in all, Wissler's rough outline sounds remarkably similar to a general Marxist conception of society. Goldenweiser elaborated Wissler's points, and like Wissler, he did not cite Marx.

Goldenweiser's comments on the subject are placed in perspective with the following question:

> What is it that holds a culture-area together, ample time being thus provided during which the various features of a local culture can interpenetrate? (ibid.:479)

Admitting that he did not really have a full answer for this question, he offered a combined ecological and historical framework for understanding the problem:

> . . . a local culture (or the culture of an area) is anchored to its physical environment; the anchor is the techno-economic equipment and procedure; the other aspects of the culture crystallize around this anchor. (ibid.:480)

The historical element entered the framework to fill in the weak point of the ecological scheme--there are always several possible adjustments tolerable by the environment. Thus, although the environment may permit

several equally good adjustments, "one adjustment is made; and having occurred, persists" (ibid.). Thus, Goldenweiser related the notion that the environment does not determine but only constrains with the notion that environmental adjustment is somehow at the root of cultural origins via the idea that the culture of an area or specific locality is the product of historical selections among alternatives (not that selections are conscious). The stability of a culture is therefore due to the combination of its environmental adjustment, the force of history (tradition or what Lowie might have subsumed under "cultural inertia"), and psychological forces operating to "crystallize" the rest of culture around the selected and well-adjusted anchor.

In summarizing the points raised in connection with Goldenweiser, perhaps the most interesting is the apparent idea that the formation of culture complexes involves different processes from those producing cultural integration. The integration of culture involves just the subjective, psychological aspects of cultural traits as opposed to any inherent, objectively regulating principles of association, but culture complexes are supposedly built around cores of functionally interrelated components which form either through a process of historical-ecological adjustment (in the case of techno-economic complexes) or unspecified processes collectively glossed as "crystallization" (in the case of non-techno-economic complexes). The historical element of selection among possible ecological adjustments may be related (up to identity) with the processes effecting whole culture integration, but this is not made clear. Similarly, "crystallization" may be related to the integrative

factors. But, the ecological adjustment of complexes to environment would seem to be objectively discernible, hence different from integrative processes.

A second major point relevant to conceptualizing culture complexes is Goldenweiser's accounting for the difficulty in delimiting a complex in a given culture versus the relative ease with which the complex can be identified when studied comparatively. His comments on this subject reveal that he thinks of complexes as consisting of basic, or core, elements which are functionally interrelated. The integrative processes operating in any given culture tend to mask the complex's individuality (core) because the complex submerges in a morass of associations.

Although quite general and not confined in the least to culture complexes, Goldenweiser's formulation of the power of history in terms of "precedent" is insightful and relevant. It is a step closer to elucidating how cultural inertia happens, but still at the level of metaphor and imagery.

Finally, Goldenweiser recognized the folly of overconcern with determining cultural similarities. Other than general guidelines to what sorts of data should be considered, it is quite impossible to get rigorous about necessary degree of similarity, for the notion of culture trait may apply to any level of empirical specificity. Lowie (1917) came around to this too. Hence, Goldenweiser conceptualized three aspects of cultural data: the morphological, the psychological, and the historical or developmental. Of these three, he felt that only the morphological and psychological should be considered in assessing "similarities" for comparative work.

Linton (1936) contributed to the conceptualization of culture complexes in three distinct regions: clarification of the place-holding nature of labels for quantities of culture content, elaboration of the role of the individual in the stability and change of culture, and differentiation of culture into four major aspects. Like the other authors reviewed thus far, Linton had a well-developed sense of history, but since this permeates the three topics listed above, it will not be treated separately.

Linton's scheme of labels for speaking about culture content is a bit more elaborate than Wissler's.

> The individual acts and objects which constitute the overt expression of a culture are commonly referred to as traits. Any one of these traits can be analyzed into a number of still samller units, which in the absence of any generally accepted term we will call items. . . . Although the traits which compose the overt expression of a culture can be isolated artificially, they are actually integrated into a functional whole. First, every trait is intimately associated with some other trait or traits to form a larger functional unit commonly known as a trait complex. The traits within such a complex are all more or less interrelated and interdependent from the point of view of both function and use. A number of such trait complexes are, in turn, combined to form a still larger functional unit which, since no term has so far been coined for it, we will call an activity. Lastly, the sum total of these activities constitutes the complete overt expression of the culture. (1936:397)

From the passages quoted, it is clear that Linton incorporated functional considerations in the identification of trait complexes and activities, but it is left unstated how one is supposed to identify traits or items, or even trait complexes, in practice. The problem is that terms such as these do not have definite empirical referrents, or even a range on an absolute scale of empirical specificity. It is obvious that the

terms have relative meaning with respect to amount of culture content encompassed, thus an item involves less content than a trait, and a trait less than a trait complex, etc. Linton recognized this problem (what I have previously called the "place-holding" nature of such terms) and offered an alternative to so-called objective determination of such units.

> A possible way out of the difficulty might be to abandon our strict adherence to the overt expressions of culture and to take as traits those elements which the individuals who share the culture are conscious of as distinct entities. However, the value of this approach is largely nullified by the practical difficulty of such determinations and by the factor of differences in cultural participation. (ibid.: 399)

Although foreshadowed by Wissler, Lowie, Goldenweiser, and others, this idea of identifying units of culture content not as an outside observer but through the culture itself is one of the major contributions of Linton. He was fully on top of the problem, in spite of the fact that he left its solution to the development of research methods.

With respect to the relation of the individual and his culture, Linton saw that culture was dependent upon the numerous individuals who have learned it in the course of their lives. But the nature of this dependency is different from that of the personality on the biology of the individual.

> The personality is dependent upon the brain and nervous system of the individual. . . . Culture, on the other hand, rests on the combined brains of all the individuals who compose a society. While these brains individually develop, stabilize, and die, new brains constantly come forward to take their places. (ibid.:293)

Cultural change is thus easily possible. "Because of this constant presence of personalities which are still in the formative period, cultures

have an almost unlimited capacity for change" (ibid.:294). The real problem for the student of culture who takes Linton's view of the locus of culture is not culture change, but rather cultural stability. For this, however, Linton does not have much to say. He recognized that cultures have a tendency towards stability, but does not account for this with any sort of theory. He only advocated the acceptance of something comparable to "the force of history."

> Every trait which has formed a part of any culture during any period in its history thus leaves its mark upon the culture. . . . Thus the custom of wearing a long sword on the left side was responsible for the custom of mounting horses from the left. The sword-wearing has long since disappeared, but the left-side mounting remains. The situation which exists in a given culture at a given point in its history is thus a direct result of all the changes and vicissitudes which the culture has undergone prior to that time. It is conceivable that if we knew the entire past of any culture we would be able to explain its entire content in terms of historical cause and effect. (ibid.:295)

In the above example of the relation between mounting horses from the left and wearing swords on the left, Linton revealed much of how he thought about culture. Implicit in the example is the idea that functional relations underlay the original trait complex. Once developed, however, the functional interconnectedness may dissolve (in this case through the disappearance of the wearing of long swords), and yet, the complex continues to hold together through the "force of history." This sort of accommodation with both functional and historical explanatory frameworks is characteristic of the later sections of the book. Linton constrained the imputation of functional interrelatedness, however, to the level of trait complexes and larger units of culture: "The smallest

combination of elements to which the qualities pertinent to functional studies pertain is probably the trait complex" (ibid.:403). And, Linton's differentiation of form, meaning, function, and use--the four major aspects of culture content--were defined only with respect to trait complexes.

The definitions of form, meaning, function, and use are as follows:

1. Form--"of a trait complex . . . the sum and arrangement of its component behavior patterns" (ibid.:403);
2. Meaning--[of a trait complex] . . . "consists of the associations which any society attaches to it" (ibid.);
3. Function--[of a trait complex] . . . "its relation to things within that configuration" (ibid.:404); and
4. Use---[of a trait complex] . . . "its relation to things external to the socio-cultural configuration" (ibid.).

These four aspects of culture content (at the trait complex level) were important to Linton in helping to account for the phenomenon of variation in the distribution of a complex. But, before they could aid in thinking about distributional variation or temporal change, Linton had to make one fundamental assumption:

> If such complexes developed in vacuo, form, meaning, use, and function would grow up together in close and constant relationship and would be completely interdependent. (ibid.)

With this initial assumption concerning the intimate interconnectedness of the four aspects of culture content at the trait complex level of organization, Linton could talk about the relative significance of what earlier

writers had called morphology and psychology with respect to the ways in which culture content gets diffused or enculturated.

> . . . the trait complex is presented to the society as a definite entity which is incorporated into the configuration by the attachment to it of use, meaning, and function. Although its form may be progressively modified during the process of incorporation, the initial form has a strong influence on the initial ascriptions of use, meaning, and function and through these on all subsequent ascriptions. Actual studies of diffused complexes show that form may persist with only slight modifications in the face of wide differences in the other qualities. (ibid.:405)

Relative to the enculturation of trait complexes:

> Individuals learn the techniques as wholes and are not conscious that any part of the whole does not contribute toward efficiency. (ibid.:406)

After considering the proposal that form may be explained by reference to function and concluding that such a tack is in principle fruitless (ibid.: 414-419), Linton concludes:

> . . . certain facts are evident. The first of these is the strong tendency for form to persist with only minor changes in the face of much more marked changes in meaning and function. The second is that function derives more completely and directly from meaning than from any other of the complex's qualities. . . . In short, the two most important elements in the trait-function-need configuration are controlled primarily by historic accidents. While it is quite possible to describe this configuration as it exists at a particular moment in time, it can hardly be explained without reference to these historic factors. (ibid.:419)

Summarizing Linton's points, he accommodated both historical and functional factors in his scheme of the behavior of culture both over time and space, but favored history when it came to explanation. He was concerned with the relation between history and the enculturation of individuals, that is, he did not consider human history to be complete without

reference to the individuals who acted it out. He explicitly made a crucial assumption regarding the nature of cultural integration: integration is the "mutual adjustment" of culture elements (cf. ibid.:347-348 and 357). He proposed a criterion for identifying units of culture: individuals of the culture must see the given amount of their culture as forming an entity. And, he made four analytic distinctions with which, when coupled with an auxiliary point concerning the level of consciousness at which individuals learn culture, he could offer more precise descriptions of the processes of culture change or stability in the course of diffusion and enculturation. In general, he was historically oriented to the study of synchronic relationships in culture and thereby overcame an excessive atomism without losing sight of the historic malleability of culture.

Kroeber is the last author who will be reviewed in this section. He is most difficult to summarize succinctly owing both to the range of topics he addressed and to the fact that he changed significantly in his perspective during the half-century or more during which he was writing Anthropology. Nonetheless, I believe his writings can be condensed into three major subject areas without too great violence being done to the interconnectedness of his thoughts.

The first subject area I shall review is how Kroeber saw the individual-culture relation. It is primarily in this area that Kroeber underwent change during his long career.

In his earlier writings pertinent to the subject (1952c, 1969, 1952k), Kroeber developed what might be called the culture-without-man perspective

for the study of cultural phenomena, and, as Opler (1964) notes, this has been very influential in Anthropoly. The fundamental justification for comments such as:

> The concrete effect of each individual upon civilization is determined by civilization itself. (1952c:48);

and

> To know the reactions in Darwin's nervous system at the moment when the thought of natural selection flashed upon him in 1838, would involve a genuine triumph of science. But it would mean nothing historically, since history is concerned with the relation of doctrines such as that of natural selection to other concepts and social phenomena, and not with the relation of Darwin himself to social phenomena or other phenomena. (ibid.:49);

is largely based upon the identification of "culture content" as an independent class of phenomena. To the extent that culture content is naturally and intrinsically partible, the multiple psychological (individual) organizations of culture may be ridden over rough-shod in the pursuit of strictly cultural configurations. In these earlier works, Kroeber seems oblivious to the efforts of many of his American colleagues who were trying to incorporate and delimit the role of the psychological aspect of cultural experience into their overall distributional orientation to the study of culture.

The result of Kroeber's implicit assumption regarding the intrinsic partibility of culture, when followed through to concrete applications, finds itself in a work such as Configurations of Culture Growth. From his early denial of the significance of psychological organization of knowledge for the partibility of culture, it is a short step to conceiving of "patterns" as the locus of both cultural coherency and stability. For, if

cultural units are naturally given and automatically implanted in unsuspecting individuals, then their unity must per force lie outside individuals, in the superorganic.

In Kroeber's later discussions of the individual-culture relation (1963, 1952a), he tries to incorporate the individual into his scheme much in the same ways as Lowie and Goldenweiser did. Thus, "learned" is a fundamental criterion of what culture is:

> In short, culture is superorganic and superindividual in that, although carried, participated in, and produced by organic individuals, it is acquired; and it is acquired by learning. What is learned is the existent culture. The content of this is transmitted between individuals without becoming a part of their inherent endowment. The mass or body of culture, the institutions and practices and ideas constituting it, have a persistence and can be conceived as going on their slowly changing ways "above" or outside the societies that support them. (1963:62)

From this quote, taken in isolation, one notes only a slight shift in basic orientation--that now culture is seen as "produced" by individuals as well as simply residing in them. However, the revised Anthropology is saturated with an effort to interrelate individuals and cultural phenomena. But even there, a considerable portion of the book steadfastly maintains that the study of culture can legitimately ignore the individual substratum of culture. For example, the following quotes are from the same page in a section entitled the "Compositeness of Culture":

> . . . on the whole the passive or receptive faculties of culture tend to be considerably stronger than its active or innovating faculties. This is something that seems to be pretty deeply ingrained in the nature of culture because it is deeply ingrained in the nature of man, something without which it would lose its continuity, and therewith its stability. (1963:65)

> In short, we can see that it is profitable for cultures to carry a considerable degree of ballast in the shape of consistency and continuity, and that those which have fallen markedly short in these qualities have soon disintegrated and perished . . . (ibid.)

In the first of these two quotations, Kroeber is remarkably similar in argument to one of the fundamental points made in Pasternak's novel, Dr. Zhivago: people need a social order so that they can get on with the business of living. Thus, Kroeber relates cultural stability to a human need for continuity and order; he accounts for a cultural phenomenon in terms of the individual substratum. In the second, however, he once again writes in a language reflective of culture reified, culture in cultural terms. He was aware of the impression this sort of language would convey, however, and later in the book, he took pains to make the necessary translation.

> When we say that a culture aims at, prizes, and achieves certain ends, that is a shorthand way of saying that most of the members of a society, through having been molded by its culture, aim at, prize, and help achieve those ends. (ibid.:102)

In spite of such concessions to the individual-in-culture viewpoint, Kroeber did not really see the individual as the agent with respect to coherency of specific culture content. The individual is pictured as a passive recipient of pre-existing knowledge, a creature "molded" by his culture. There are brief passages in which Kroeber takes first steps toward seeing the individual as responsible for the partibility of culture, but these are not followed through. For example, he writes about the hodgepodge of culture when seen from the outside, but its coherent interrelatedness when seen from the inside. This internal sense of coherency

he attributes to the molding power of culture over the individual. With respect to a whole culture, Kroeber allows the individual a role in generating coherency, but not for coherency at the level of specific culture content (ibid.:94). This is especially strange to me, for Kroeber was interested in different ways individuals learn their culture, whether through formal instruction, imitation of others, or in isolation through their own initiative. Yet, he did not believe the process of learning is responsible for the partibility of culture content. His varied comments on learning presuppose that knowledge is already sorted into more or less bounded units which are transmitted and replicated ratherly accurately. This presuppostion is precisely what is at issue when speaking of culture complexes. What is the guardian spirit complex?--the maize complex?--bullfighting? Are they really natural unitary entities existing in the superorganic level of reality? Or, are they constructs of individuals' psychic, cognitive organizations? Kroeber came close to answering in terms of a cognitive viewpoint, but he stopped short, thereby maintaining his earlier view of a "superorganic" locus of cultural partibility.

Leaving the subject of the individual-culture relation, the next topic of concern in this review of Kroeber is how he thought about human culture's distributional manifestations.

To begin, Kroeber agreed fully with his colleague, Lowie, that the proper, natural unit for study by the ethnologist was the culture of all mankind during all times and in all places (1952d:379). Within this delimited range of phenomena, he differentiated between culture content and culture pattern. This was recognized as a distinction between aspects

of all culture. Culture content and culture form always occur in association with each other (1969:799 and 844). A culture is, in principle, defined in terms of its participation in certain kinds of culture patterns; and, conceptually, the boundaries of a particular culture are not absolute, rather, they are determined by the analyst (ibid.:830-831).

Culture content was seen as forming associations, as falling into complexes. We have already seen that there are several different modes of thinking about what a definition of a complex should be (e.g., definition in terms of a composite average, in terms of a full definition and allowing for partial manifestations, or in terms of a least common denominator--Wissler, Hallowell, and Benedict, respectively). In the light of Needham's recent discussions of "polythetic" definition of classes (1972, 1975), it is perhaps very interesting that Kroeber noted in 1931 that "A culture complex often is 'polyphyletic'" (1952f:57). Furthermore, Kroeber argued that "It is the totality of structure which decides relationships between groups of organisms or between culture trait complexes" (ibid.:59). In other words, Kroeber outlined the rudiments for thinking of culture complexes as "polythetic" classes, which is not to say that Needham has not added clarity and explicitness missing in Kroeber's writings. Generally speaking, I think it is fair to say that Kroeber was thinking about complexes in the same way as Wissler, but Wissler left the burden of figuring out what that was to his reader, whereas Kroeber gave a better account of what he was doing. The "polythetic," or "polyphyletic," view of a culture complex envisions each complex as having a set of characteristic features, elements, or

relations between elements. In any given case, only a portion (a subset) of these characteristic features may be actualized, and others may be absent or even counterindicated. If the portion of identifying characteristics is sufficiently large, or includes particularly significant features, then that case is adjudged a manifestation of the complex in question. The notion of "significant" features of a class and what they might be is discussed in Barden (1976).

The concept of a culture complex comprises more than the simple enumeration of characteristic elements of culture content, however. The constitutive content is also organized in some characteristic fashion. For Kroeber, cultural form was conceptualized in terms of patterns.

> Patterns are those arrangements or systems of internal relationship which give to any culture its coherence or plan, and keep it from being a mere accumulation of random bits. They are therefore of primary significance. (1963: 119)

It is in this area that Kroeber is perhaps best known--his furtherence of the "pattern" concept. And, he is undoubtedly its best representative (cf. Singer 1968:528).

Kroeber discussed four different kinds of cultural patterns: the universal pattern, systemic patterns, societal or whole-cultural patterns, and stylistic patterns. He did not think much of the "universal pattern" formulated by Wissler, and he rather summarily dismissed it. The idea of whole-cultural patterns came from Benedict, and Kroeber thought of it much in the same way with two supplementary comments: (1) all cultures should be susceptible to configurational style analysis <u>because</u> culture is the product of psychosomatic activity and in turn has effects upon

such activity (1963:131); and (2) whether or not a particular culture may be accurately described as congealing around a single motif is an empirical question and not to be assumed at the outset (ibid.:131-133). Thus, two of the four kinds of cultural patterns had their original development in other authors (reviewed in earlier portions of this chapter). Systemic and stylistic patterns seem, however, to owe their naming and characterization to Kroeber.

A systemic pattern has the following properties: (a) It consists of a complex of cultural material that has proven utility as a unit; (b) It is only superfically modifiable as regards its underlying organization; and (c) It is limited to one aspect of culture (ibid.:120). Another name for the systemic pattern is the "well-patterned system" (ibid.).

In discussing systemic patterns, Kroeber leaves no doubt as to how he accounts conceptually for the persistence and coherency of them. He quite plainly says that they owe their stability as units to the fact that they have worked well; they have utility because of "a specific interrelation of their component parts" (ibid.:122). Examples given of a systemic patterns are complexes such as plow agriculture, monotheism, the alphabet, and the kula ring. Thus, systemic patterns have both coherency and stability because of their logico-functional interrelatedness; they have proven themselves to be adaptively fit. The primary significance of systemic patterns for Kroeber lay in the way they can be used in the culture historical reconstruction (ibid.).

Stylistic patterns, although a major concern of Kroeber (e.g., 1952e, 1969, and with Richardson 1952), are not as directly related to the notion of a culture complex as are systemic patterns. Therefore, I shall omit this conspicuous aspect of Kroeber's thinking for the time being and review stylistic patterns in Section 4.5.

Given the "pattern" concept, Kroeber was equipped to give some sort of meaning to the idea of "cultural integration." Basically, integration is "an accommodation of discrete parts, largely inflowing parts, into a more or less workable fit. It is not a growth of parts unfolding from a germ in accord with a pre-existing harmonious master plan" (1963: 95). Furthermore, cultures are constantly and inherently tending towards integration, even as new cultural material flows into the inventory (ibid.). The reason for this accommodation of cultural material into established patterns is that the average individual (i.e., the non-historically-minded student of culture) makes no distinction between what is fifty and what is one thousand years old; it is all subsumed as his social inheritance and accepted (ibid.:94). Note that in this context, "pattern" evidently refers to whole-cultural and/or stylistic patterns rather than systemic, for one of the distinctive aspects of a systemic pattern is the persistence of the pattern as the complex travels.

The third major subject area has to do with Kroeber's sense of history and the importance of tradition in understanding culture.

Perhaps the fundamental importance of history with respect to culture, in Kroeber's view, is best reflected in the claim that " . . . a cultural fact is always a historical fact . . ." (ibid.:63), or again,

"it is of the nature of culture to be heavily conditioned by its own cumulative past, so that the most fruitful approach to its understanding is a historical one" (1952a:4). Such a view follows naturally from a definition of culture in terms of learned and transmitted knowledge.

The two fundamental axioms of Kroeber's historical approach he called the continuity of history and the uniqueness of history (1952b, 1952h, 1969:3). These he shared with "historians" of his era. In addition to these axioms, he adopted a deterministic and "mass effects" view of cultural development (1952a:8-9). In these latter two characteristics of Kroeber's historicism, he acknowledged an affinity with the sense of history advocated throughout Tolstoy's War and Peace. The "mass effects" view of history is reflected most clearly in his various considerations of the nature of genius, in which he does not deny the psychological superiority of the genius, indeed quite the contrary, but says that for the purpose of following cultural development, one may assume that geniuses (in terms of psychic equipment) occur with essentially the same regularity in all human conditions. Thus, he rigidly distinguished between potential genius and genius actualized and recognized. The cultural conditions in which a potential genius lives determine whether that person's efforts will become "genius."

Closely intertwined with Kroeber's "social definition of genius" (1969:839) was the concept of pattern, stylistic pattern, and to a large extent, this is the area which incorporated his determinism. Later works dispensed with the determinism characteristic of his earlier works--the change being a consequence of the increased role of the

individual in the production of culture. Stylistic patterns involve an initial selection of possible lines of development, a limitation of potential growth, and their development or growth consists in the filling in of the pattern. Once the pattern has reached fulfillment, further development along the same lines would be merely redundant. So, for an undetermined reason(s), the pattern modifies, thereby opening new fields for culturing. The resulting view of culture history is thus a mapping out of the various trajectories of pattern selection, growth, fulfillment, redundancy, and decline and/or modification. The motivating force underlying pattern growth is summed up in the rather fascinating, but completely opaque concept of "cultural energy" (ibid.:768, 773, 796, and 844).

The value of the growth-configuration, or pattern, approach to history, which Kroeber developed in numerous writings (1952b, 1952c, 1952d, 1952f, 1952g, 1952h, 1952i, 1952j, 1952m), is best summed up by the closing sentence of Configurations of Culture Growth, his major work along these lines.

> . . . the endless events of history are lifted out of their level of near-uniformity into organized relief, by an attitude which consciously recognizes pattern-growth configurations in their space-time relations as well as their value relations. (1969:846)

There are two fundamental weaknesses in Kroeber's earlier deterministic, "mass effect" sense of history. The first is the absence of any accounting for the process of selection, which is so essential to the whole mode of operation. Why are the specific selections that are judged post facto to have been made the ones that were made? How do cultural

patterns come into existence? The second criticism is that "patterns," conceived as being deterministic, are not conceptually rich enough to be deterministic. Hence Kroeber asks his reader to believe that cases of recognized genius have been "predicted" when in fact their particular productions--what has made them "geniuses"--have only been shown post facto to be appropriate in a given milieu. Piaget (1971a) has provided a more precise outline of how one might profitably understand "selection of patterns," to stay within Kroeberian language. But, I know of no modern writer who would be bold enough to think that they have accomplished deterministic prediction by demonstrating appropriateness of an occurrence. At most they might claim to have predicted the event probabilistically. Patterns, contrary to the overall connotations of determinism surrounding them, are rather constraints on acceptable growth. They are general guidelines (in the sense of rules of exclusion) on what might or could be, but not predictors of what will be. Only some sub-set of what could be, what is appropriate, will become actualized. And, actualized potentialities can further constrain what will be appropriate in the future.[3]

Perhaps an awareness of these problems lay behind Kroeber's retreat from historical determinism, but the precise reasons are not so

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[3]I should like to acknowledge the aid of Prof. F. K. Lehman in helping to make this point in this context. The issue here is a very general one involving many and sundry sciences, e.g., directionality of mutations in the biological sciences. It hinges upon conceiving of "structure" in such a way that an infinite number of possibilities are precluded by the existence of the structure, but an infinite number (or at least indefinitely many) are simultaneously allowed. "Patterns" pretend to allow only a finite number of possibilities.

important in this context as is the manner in which the rather extreme superorganic view of culture history became modified. In a sense, this was accomplished by allowing that geniuses deserve at least some credit for originality, that individuals are responsible for the "road not taken" as well as the one that is. Instead of being regarded purely as the index of cultural development, the individual came to be seen as the cultured actor responsible for actualizing, making historical, and thereby largely shaping the lines of culture growth (cf. the discussion above concerning Kroeber's later views on the individual-culture relation). With this concession to individual action, both pattern determinism and "mass effect" history are necessarily abridged.

With respect to the conceptualization of culture complexes, I think Kroeber's contributions may be summarized in the following scheme. First, he obviated the whole issue of accounting for the partibility of culture by direct conceptualization in terms of patterns. Second, he distinguished among kinds of culture patterns, each kind of pattern having its own distinctive source of stability: the universal pattern is locked in the nature of man, whole-culture patterns arise from the human need for continuity, stylistic patterns grow predictably from their initial selection of development potentialities to their natural fulfullment, and systemic patterns persist in their characteristic organizations because they have proven their adaptibility in some range of contexts. Of the four kinds of patterns, the systemic pattern appears to be most pertinent to the notion of a culture complex. Thus, for Kroeber, culture complexes owe their coherency and stability to their relative efficiency

in terms of successful accommodation with some aspects of their larger environment, whether that be natural or cultural or both. And third, although the nature of the human substratum is responsible for the continuity of culture (its generalized stability), culture molds the individual and not conversely, hence the coherency of the multiple units of culture (e.g., culture complexes) is independent of the actual psychological organization of culture in individuals. All three points may be condensed: cultural partibility is embedded in the nature of the superorganic.

4.3 Themes in the Conceptualization of "Culture Complex"

The foregoing section has reviewed the work of several scholars, one at a time, with the objective of isolating their respective modes of comprehending the unity of culture complexes. It has been quite rare for any of them to address the issues directly. By and large, an author's views on this subject were semi-conscious at best and only obliquely expressed. This inexplicitness in the literature has made the extraction of the various perspectives tedious and inferential. At this juncture, I want to stop reviewing each author as an integrated mode of thinking and extract a few grand themes from the lot of them.

There are a few commonalities of interest and orientation which can be seen in slightly varying degrees in each scholar. They were all concerned with coming to an understanding of human culture rather than focusing upon a particular culture-whole as the object of most immediate fascination. All of them had at one time or another compiled distributional data and pondered their significance. And, one of their principal

goals was to reconstruct the natural history of culture. Whether an appreciation of the "forces of history" was the result or the motivating impulse of such distributional studies I cannot judge, but all the scholars incorporated some notion of "historicity" into their most fundamental understandings of the nature of culture. Lesser's critical reaction to Radcliffe-Brown's structural-functionalism beautifully expresses this deep and pervasive sentiment:

> The conditions which functional investigation must take account of can be generalized as historicity--the fact that institutions, customs, beliefs, artifacts, have careers in time, and that their form and character is molded more by what has happened to them in the course of that history than by what particular things they occur associated with at any one time. (1935:393)

Beyond this core of agreement, however, the various authors begin to diverge, even with respect to such seemingly fundamental concerns as what criteria should be used to identify cases of cultural similarity (the first step in the actual plotting of a distribution!). Furthermore, they differ rather markedly as regards preferred frameworks for accounting for the existence of cultural similarities--whether parallelism, convergence, or diffusion requires the burden of proof. These two major points of divergence are very much related in practice, however, for, depending largely upon the specificity of description, a "cultural similarity" is adjudged to be a case of parallelism, convergence, or diffusion. This connection between the "initial delimitations of parts of culture" and appropriate explanatory framework was well recognized. Eventually, all three logical possibilities accounting for any given case of cultural similarity were recognized as just that--logical

possibilities whose operation in any particular case had to be demonstrated. But, the criteria for establishing similarities remained in debate and continued to separate the Germans from the Americans.

Basically, the choice came to whether "psychological" data should be a consideration along with morphological data in the determination of cultural similarities. Generally speaking, the German anthropologists refrained from explicit incorporation of the "meaning" of cultural unities (the psychological aspect), whereas the American anthropologists insisted upon including this aspect. This difference has far-reaching consequences and bears upon how one can account for cultural partibility. For the real question, which found different answers from the Germans and the Americans, is: how can some amount of human culture be delimited? And, this is the beginning of the problem of conceptualizing cultural partibility.

In the German view of the issue, cultural items as unities originate in the consciousness (or unconsciousness) of the analyst. The outside observer defines a constellation of form pertaining to cultural data, then traces out the distribution of this morphologically defined unity in space and time. From the American point of view, however, cultural items as unities must have coherency in the psychological make-up of the various peoples of the world who have either developed or borrowed them, and this in addition to having morphological distinctiveness. This difference Lowie contrasted as "sound phenomenalism" (the American view) versus an "arid conceptual realism" (the German view). It should be clear, however, that neither view has an answer to the thorny question of how

similar must two occurrences be to justify regarding them as instances of the "same." There has never been a formula for determining this; all good practioners have realized the problem and used their good sense and judgement as best they could. The American view just advocated using a broader range of data. But, in advocating the relevance of psychological data, the Americans left open the possibility of conceptualizing cultural partibility in terms of the result of the learning processes. The German view sees cultural partibility as an artifact of analytical definitions and not a real property of culture. Of course, the American view on this issue was not determinate as regards conceptualization of cultural partibility--for example, contrast Kroeber and Goldenweiser--but it allowed two possibilities in addition to the minimal one allowed by the rather agnostic German "it is all definitional" perspective.

There are, thus, three major ways of thinking about cultural partibility reflected in the literature reviewed in Section 4.2. They are somewhat related to the alternative sets of criteria for determining cultural similarities.

The first may be called the definitional view. This perspective would claim that cultural partibility is not a real property of culture, but rather a necessary consequence of particular analyses, each having its own objectives. It is also the extreme, cynically agnostic view, for it would be based upon a belief that it is impossible to know whether culture has any intrinsic, natural clumpings or conglomerations identifiable as such. The analyst creates such clumpings as part of his methodology, not from the impulse to be empirically accurate, but

from the same impulse as motivates his analysis in the first place. Thus, this view may believe culture does have natural, real partibility, but since it is impossible to know what this is, one should go ahead and define delimitations of culture a priori for specific purposes.

The second and third major viewpoints share a contrast with the first in that both see cultural partibility as a natural property of culture and not a conceptual artifact. Both envision a test of otherwise purely definitional entities--the "best" definitional entity is the one among several possible which appears to exist as a psychological entity among the peoples supposed to possess or support the delimitation of culture in question. Both believe definitional delimitations should be grounded in psychological data. The second viewpoint is different from the third in the respective ways each envisions cultural partibility to come about.

The second, the superorganic view, sees cultural partibility as a real property of culture, but intrinsically so. That is, culture is naturally partible and this partibility is reflected in the psychological make-up of individuals, but the locus of the partibility is in culture itself.

The third view, the man-in-culture view, would see cultural partibility arising from the processes whereby an individual learns and makes his culture part of his memory. Here the primary locus of cultural partibility is the psychological make-up of the individual, and it is a moot issue whether culture (conceived as something other than some sort of conglomerate abstracted from collections of individuals) has any

natural organization because the only locus of cultural data evidences the property of partibility.

The difference, then, between the superorganic and the man-in-culture views is very much related to whether one sees the individual as a passive recipient molded by his culture or as an active creator of his knowledge. In the superorganic perspective, culture has partibility because that is its nature. The fact that individuals are molded by their culture implies that it is in principle possible to make empirically real delimitations of culture. In the man-in-culture perspective, it is irrelevant whether "culture" is by its nature partible; that would depend on whether one conceives of culture as something other than its manifestations in individuals. The important point is that our only source of cultural data--the behavior and thoughts of people--evidences partibility.

To understand "cultural" partibility, then, from the man-in-culture perspective, means to understand how the psychological make-up of individuals forms (learning) and how it operates in their lives (memory).

Given that there are different modes of thinking about the nature of culture content associations--the definitional, superorganic, and man-in-culture perspectives--there is a second and equally important point of divergence. Regardless of how one regards the process whereby he has identified a culture complex, there is the additional issue of how one conceptualizes the forces holding the association together so that it can be recognized. Basically, the choices in the literature are: (1) The association has coherency and stability owing to logico-functional relations among the elements of the complex; (2) The association formed

as an historical accident and continues (coherency with stability) owing to the "forces of history;" or (3) Both of the above. Selective borrowing, cultural survivals, and cultural innovations are the crucial phenomena which highlight this problem. For the present, it is sufficient to note that there are alternatives, and I shall postpone until Section 4.5 a detailed argument in favor of the "forces of history" conceptualization as the deepest understanding.

Another general theme evident in the literature is that "complex" is a place-holding concept. When they speak of such-and-such complex, they are speaking of a unity with internal organization of parts, and, by and large, it is the mode of organization which is most important in the definition of the complex. Thus, although a complex once named may be treated linguistically as if it were an elemental unanalyzable entity, it carries the implication that one could break the complex down into smaller associations of culture content if it were desired. Also, there is no rule for applying the term "complex" only to certain scales of culture content, that is, Herskovits can call the East African Cattle Complex a "complex" and I could speak of a Bow and Arrow Complex as a "complex." The only author explicitly to give the term "complex" any additional significance other than its relational, place-holding meaning vis-à-vis other terms such as "trait," "configuration," etc. was Linton. He suggested that the "trait-complex" was the lowest level of culture content at which one could speak of form, meaning, use, and function.

These, then, are the major themes in the literature reviewed in Section 4.2. The underlying concern is how to conceptualize the

intuitively recognized phenomenon that culture, human culture, is not a singular, homogeneous reality. How can human culture be partible? The more specific question is how to delimit some range, volume, amount, congeries of culture both in space and time. Is it, in principle, possible to be "accurate" in such delimitations, and if so, what sorts of data are pertinent in the endeavor? Furthermore, what sorts of forces act to give ranges of culture content coherency and coherency through time (stability)? In referring to some range of culture as a "complex," one recognizes it as a clumping, but the existential nature of the clumping, what is holding it together, and even the amount of culture content encompassed are not determinately specified. In other words, there is only a minimum of consensus among authors using supposedly the "same" concept.

At this time in the history of Anthropology, there are only a few sociocultural anthropologists actively concerned with the issues embedded in the notion of a culture complex. Jacobs (1964) reads like an epitaph on this mode of studying culture. The issues are not in any sense resolved; rather, they have just fallen from our collective attention. Nonetheless, there are some recent examples of anthropological work predicated upon the notion of culture complex. I shall very briefly examine some of these in order to see if they have added anything significant to our understanding of how complexes cohere and persist.

4.4 Some Modern Uses and Thinking about Culture Complexes

Perhaps one of the most significant recent developments in thinking about what may be called culture complexes is the replacement of the word

"complex" in favor of "system." This is not to say that all "systems" are necessarily "complexes," but in at least some cases, the translation holds.

I have selected three examples of modern studies focusing on what in the early decades of this century would have been called complexes. They are: Strickon's account of the Euro-American ranching complex (1965), Rappaport's study of highland New Guinea ritual pig slaughtering (1967, 1968), and Lathrap's description of the religious complex manifested in the Obelisk Tello (1973). Obviously, this list of examples could be extended, but these few shall suffice for the purpose of illustrating the modern mode of thinking.

The Euro-American ranching complex is found in Argentina, the western United States, and the Australian outback. The complex requires a lot of land, but only minimal labor. The took kit of the laborers, the cowboys, is remarkably similar throughout the complex's distribution, perhaps owing to its common origination in the Iberian penisula. The ranching complex developed as an agricultural industry concommitant with the industrial revolution, for ranching, as a business, supplies meat, hide, wool, etc. to urban workers. These are the general features of the complex as described by Strickon, but such an enumeration of its distinctive features is far from the point of his study. The major emphasis of his account seems rather to be identifying ecological factors acting upon the complex and thereby contributing to its stability, spread, or decline. In addition to obvious external parameters such as rainfall (abundance and regularity), he traces the interrelations between

the complex and its chief competitor, farming, and between the complex and price fluctuations.

The overall explanatory conceptualization is in essence one of balance achieved through the interaction of opposed forces. For example, in accounting for the characteristic size of a ranching operation, Strickon says a certain acreage is necessary to avoid succumbing to the dictation of market price fluctuations.

> A stable ranching operation is one which makes sufficiently extensive use of its land during normal years so that it can, during periods of falling prices, hold its cattle off the market rather than either sell them at a loss or risk damage to its range. (1965:246)

In addition to this relation, the ratios of both capital investment per animal and labor needed per animal decrease as size of ranch increases, thus the rate of profit is directly proportional to the size of the ranch. Hence, the classic "cattle baron" depicted in Hollywood movies was motivated not only by greed, but also by a desire for his business's security--if he had the "sodbuster's" land, then he was more able to hold off selling his cattle until times of high prices. Commercial agriculture, however, can out-compete ranching in terms of profit per acre.

The article, then, tries to account for the distribution of the ranching complex and some of its general features in terms of certain ecological adjustments. In this endeavor, although much more detailed and explicit than earlier accounts of complexes, Strickon is really just following the general mode of thinking evident in, for example, Wissler's study of the aboriginal maize complex. That is, Strickon is conceiving of the complex's coherency and stability in terms of its functional

interconnectedness. For Strickon, functional interconnections are identified not only internally, but also with respect to variable ecological parameters--he tries to show how the ranching complex "fits" with a certain kind of environmental setting.

Rappaport's studies of New Guinea pig slaughtering follow in the same vein as Strickon, but are even more explicitly representative of the so-called "systems approach."

Rappaport does not claim to have accounted for why it is that the Tsembaga practice the customs they do. Rather, he takes their cultural traditions as given. He is concerned with showing how their system "fits" with selected aspects of their natural environment, sociological environment, and certain constraints arising out of human physiology. Thus, the system coheres owing to cybernetic causality--each variable in the system constrains the behavior of other variables (cf. Bateson 1967b)--and exhibits stability insofar as the system's variables stay within certain ranges of fluctuation. His book is especially illustrative of this mode of thinking, and it stands as a modern classic in its field. The systems approach taken this far, however, begins to lose focus on complexes as an analytical concept or an empirical reality. The system has no boundaries, but grows through the network of manifest and latent functions as far as the particular analyst wishes to push it. Thus a system may incorporate multiple complexes, thought of as psychologically felt unities. Complexes also exhibit this property of seeming unboundedness, but in principle they cannot expand to include everything.

Rappaport's work clearly reveals the rapidly developing mode of understanding characteristic of "neo-functionalism," or "cultural

ecology." Like Strickon, he accounts for both coherency and stability through the idea of functional relatedness among variables defining the system. This sort of functionalism incorporates the notion of "adaptation" as its cornerstone. Unlike the structural Marxists (cf. Friedman 1974) but like Harris (1965, 1971), for him, there is only one level in the system. That is, a variable is a variable without restrictions as to what pair-wise relations it may have vis-à-vis other variables. Thus, the Rappaport (and Harris) version of the systems approach indulges in piecemeal functionalism rather than reducing whole sub-systems to the basic, materialist sub-system through notions of "structural dominance." The latter version envisions levels within the sociocultural system and "reduces" them by seeing each as a homomorphic replication of a causally more primary level.

The last of the three examples drawn from recent literature is Lathrap's study of the Obelisk Tello. It stands out from most other studies of culture complexes in that its conclusion poses an implicit question of major theoretical significance.

The article describes the iconography found on a Chavín stone monument, deciphers the symbolic images, and concludes that the religious system represented in the iconography must have developed in the tropical forest of South America. The conclusion seems eminently sound, yet in this case, one cannot help wondering why the obelisk is sitting high up in the Andes. Thus, Lathrap's study demonstrates that the essential formation of Chavín religion occurred hundreds of years before the manufacture of the Obelisk Tello and in a very different ecological milieu.

The question is, then, how this religious system could maintain its coherency and persist for hundreds of years. These data are incompatible with functionalist theories of religion, such as Radcliffe-Brown's (1965), which state that those features of the natural environment upon which man is dependent will become the object of his ritual attitude. Lathrap's analysis highlights the problem of coherency and stability. What are the "forces of history" which maintained the system "out of context?" If one understands the formation of the complex in functionalist terms--uses Radcliffe-Brown's theory to understand why the cayman, harpy eagle, anaconda, etc. should be objects of the ritual attitude when living in the tropical forest--then one should also expect the system to have modified when the society moved to the Andean environment. But it did not, at least so far as the revered animals are concerned. Lathrap's analysis thus provides data on a remarkable case of stability which begs understanding.

One possible manner of thinking about this is that the logico-functional interrelation of the religious complex was so well-developed that influence from changes in external parameters (e.g., moving out of the tropical forest environment) were insufficient to modify seriously the basic world view inherent in the religion. That is, once the religious system became well articulated and interpenetrating conceptually, the god-like creatures reflected in the iconography became disassociated, relatively speaking, from their actual representatives through a process of progressive abstraction. In such a way, the complex became insulated from larger portions of its context and

achieved stability owing to its functional isolation. But this attempt to uphold functionalism would still have to come to grips with why the complex should become insulated rather than just change with changes in the broader context. The persistence of the complex seems to be rooted in the power of tradition, whatever that is.

In these modern examples one can see a growing interest in explicitness of understanding. Whereas Wissler or Goldenweiser or other earlier anthropologists used to proclaim grand schemes about how complexes held together through functional interrelation, recent writers seem to be bent on concretely demonstrating the effectiveness of such thinking in cases. In pursuing this end, the notion of complex is frequently replaced by a less restricted notion, system. In at least some cases, the Boasian concern of using psychological unity as a criterion is overridden in favor of following out whatever factors seem to impinge upon a complex. Lathrap's article, however, follows in the older culture historical tradition, but without using the comprehensive distributional plotting technique characteristic of ethnological culture historians. His work challenges simple versions of functionalism through its focus on stability despite contextual change.

4.5 Summary and Conclusions

From the foregoing sections, it should be clear that "culture complex" is not a well understood concept. As Herskovits suggested, perhaps the lack of explicitness is partially caused by its grounding in our everyday level intuitions. Whatever the reasons, the concept has been used in anthropological writing and thinking for decades, and only

recently have efforts been made to delve deeper into its specifications and implications. Not surprisingly, given the poly-paradigmatic state of Anthropology as a discipline, "culture complex" has various conceptualizations, nascent as they may be.

Irrespective of how one regards the ontological status of culture complexes or how one chooses to understand their coherency and stability, seemingly everyone agrees that "culture complex" is a "place-holding" concept. I believe this is a reasonable point of agreement given what seems to be the nature of cultural partibility. For, following the idea that "culture" is distributed in "clumpings" (complexes), there are two general characteristics of cultural partibility:

1. There is no atomic level of culture. A "trait" refers to no definable level in an embedded and cross-cutting stratum of cultural phenomena. There can be, in principle, nothing comparable to the periodic chart for matter. Thus, even the seemingly lowest level "part" of culture (a trait) involves a complex organization. (cf. Kluckhohn 1970:315)

2. Clumpings of culture are seldom distributionally stable to the point of identity. Clumpings are polythetic (in Needham's sense); they are n-dimensionally variable, thus permitting of a variation approaching continuous gradation of similarity and difference. This characteristic underlies the "how similar is similar enough to regard as the same" problem, and also shows that this problem is falsely conceived and really an artifact of the rigid categorization of the word "same."

These two characteristics are related inasmuch as if it were not for the second, then one might think that the first would be possible. That is, if clumpings were distributionally stable, then this feature could be used in developing a "cultural periodic chart." However, such a chart would still have to contend with the issue of level of embedding,

because the chart must partition culture in one level.

An image of cultural partibility arising from these two characteristics taken jointly is that of a volume of glutinous mass with differential solidity in various regions. The regions of relative solidity correspond to "clumpings," and ranges of their dimensional coordinates are the indices of comparison. Each point in space and time has such a mass associated with it which can be compared to others. Movement (comparison) through time and space sees the deformations modify continuously, congealing and dissolving fluidly. In this image, there is no atomic level, except the ideal but non-actual concept of "point," and the clumpings are continuously variable in their nature. One might think of all this in terms of "culture" as a morass of differentially interlacing billows of form.

To the extent that the above image is accurate, it is no wonder that attempts to define culture complexes have been either unsuccessful or too antiseptic to be believed. It is our cultural expectations that "definitions" segregate things clearly, sharply, and unambiguously from other things. But, if culture complexes are billows of form, if they are regions of relative solidity, if they are truly complex amalgamations, then the standard definitional approach is inappropriate.

This, then, is roughly the level at which there is considerable agreement with respect to conceptualizing culture complexes. But it is quite possible to fully subscribe to all of these statements about the nature of cultural partibility and distribution and yet hold very different answers to the questions underlying this chapter. The differences

arise after this minimal understanding. Yet, I find it fascinating in itself that not one of the literature selections made an attempt to lay bare the nature of the beast they were studying and theorizing about. Once again, maybe this is due to intense familiarity with the subject at an everyday level, but it is nonetheless noteworthy. The closest facsimiles to direct confrontations with the issues were the turn of this century debates concerning "cultural similarities."

In summary, we have seen (in Section 4.3) that there are roughly three views in response to the first question about "culture complexes"--what is their ontological status? The first of these is the definitional perspective, which sees culture complex as a conceptual artifact of analysis. Complexes may or may not exist, but since it is impossible to know what they are, the analyst defines them himself. The second perspective believes complexes exist as part of the natural order of the superorganic level of reality. To the extent that human beings partake of and support this superorganic level, they exemplify in their being the natural partibility of culture. Thus, the second perspective believes in the reality of complexes and locates them in culture itself. The third perspective also believes in the reality of complexes, but by holding to the view that individuals construct their knowledge, by seeing learning as an active process rather than a process of being molded, the man-in-culture view locates complexes fundamentally in the psychological make-up of individuals. Cultural partibility is only a portion of the partibility of personal knowledge and activity.

With respect to the second question about "culture complexes"--whence their coherency and stability?--there are also three major categories of answers. They are: culture complexes owe their coherency and stability to functional interconnectedness, logical patterning, or the forces of history. Since I have not yet examined in any detail the modes of thinking underlying each of these labels, I shall do so at this point.

There are essentially two forms of functionalism, functional thinking with or without the auxiliary idea of "adaptation." The fundamental mode of thinking underlying either version of functionalism is that one identifies things (there is no better word) and then relates them one to another "functionally." "Function" has several senses in anthropological writing (cf. Durkheim 1969 and Radcliffe-Brown 1935; Firth 1956; Bateson 1967a; and Leach 1968) ranging from the notion of "purpose" to the more mathematical notion of "correspondence." Excepting the use of function in the mathematical sense, all functional studies are concerned with some notion of causality, whether it be determinate or cybernetic. Some functionalisms describe "causality" as seen by the people themselves; others understand "causality" as an absolute and according to our Western science and technology. Malinowski's (1961) functional account of Trobriand canoe magic is a good example of the former; Rappaport's (1967, 1968) ecological accounts of Tsembaga pig slaughtering illustrate the latter. Malinowski, though disinclined to believe in the efficacy of Trobriand magic, went to great pains to describe the systems of causality as understood by the natives. Rappaport was much more concerned with seeing to what extent the (emic) thinking of the Tsembaga was effectual

in bringing about the conditions deemed necessary, according to our world view, for life. These two examples also differ in that Rappaport's functionalism incorporates "adaptation," whereas Malinowski's does not.[4]

The general logic of all functionalisms--whether Malinowski, Durkheim and Radcliffe-Brown, neo-functionalism, human ecology, cultural ecology, biogeography, or whatever--is that phenomenon X is to be understood as it is related to portions of its context. Whether that context is conceived of as universal human needs, universal cultural imperatives, system-relative variables, or variable ecological parameters, they are all alike in that they understand coherency as the effect of close dependency, either A upon B or A and B upon C, and stability as a floating equilibrium achieved by some kind of balancing of opposed forces (cf. Thompson 1972).

The auxiliary concept of "adaptation" dictates that a culture, functionally understood, is subject to selective pressures arising in the natural environment, and thus is not a closed system. In functionalisms without adaptation (e.g., Radcliffe-Brown), the culture-social structure floats freely and has stability owing to the way it is internally constructed and connected. Functionalisms with adaptation, however, locate the source of stability either in an evolved optimal interface of the culture and its environment or in causal constraints imposed by the natural world. The first view sees culture and environment in interaction

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[4]Malinowski's functionalism related cultural phenomena to an invariant list of biopsychic needs (1944) whereas "adaptation" involves relating cultural variables to other variables.

equally; the second envisions the natural world in non-uniform motion, and culture partakes of this because it is intimately connected to (part of) the natural world. The overall image is one of a dynamic surface composed of all cultural, ecological, chemical, astronomical, etc.--that is, all--phenomena: to understand any thread in the fabric, one must comprehend its relations to the surrounding milieu. Conceptually, context is equivalent to the entire surface, but operationally it is only a subset consisting of the identified and most immediate environment (cf. Bohm 1957). Each aspect of the context restricts the possible nature of the phenomenon being investigated. The more aspects of the context identified, the more "predicted" is the phenomenon for it is more constrained in its possible form of occurrence. Thus, functionalisms "understand" the phenomena investigated through the logic of intersection. A classic example of this mode of thinking is Sahlins's study of social stratification (1972). The so-called "systems approach" modifies older versions of functionalisms by allowing mutual constraints between variables, but it is otherwise identical with respect to this point.

This, I hope, is a fair outline of functionalisms, at least as found in anthropological work. Within this broad category, there are, of course, numerous versions which I have not attempted to catalogue specifically. Some are now regarded as simple-minded, but some are quite sophisticated. The intention here has been not to evaluate so much as to isolate how they "understand."

The second major paradigm of understanding the coherency and stability of culture complexes is based on a notion of logical patterning. I suppose the simplest example of this is the idea of the coherency of the members of a set defined by some sort of predicate definition. For example, the set 1,2,3,5,7,11,13,17,19,23, . . . "coheres" in that each element of the set has in common the fact of being a prime number. Similarly, one could speak of the linguistic complex comprised of all utterances following the logic of, "Ask not what your country can do for you, ask what you can do for your country." This complex would include the also famous saying, "It's not the man in my life, but the life in my man" (this example taken from Peacock and Kirsch 1973). Thus, culture complexes would be thought of as all the replications of some logical pattern. This version is not so trivial as some might at first think. It is the basic format for Lévi-Strauss's structural studies of myth (1967 Chapters XI and XII). In his studies, Lévi-Strauss reduces substantive statements to formal representations, a myth's structure, and then distinguishes a myth from others on the basis of its structure, which is a logical pattern.[5]

There is another major version of the logical pattern paradigm. It is found concretely in Kroeber's idea of stylistic pattern. A style is certainly a culture complex, and it holds together as a unity owing to the "rules" which define-identify the style. For Kroeber, however, a style grows, hence it is not merely an equivalence class of replications

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[5] A particular myth is appropriately regarded as a culture complex, cf. Herskovits (1967).

of the same pattern. Its growth is not free, quite the contrary, it grows along identifiable trajectories. Thus, a style's "rules" are similar to mathematical operations, which generate a matrix of elements all conforming to the rules of combination. Kroeber's notion of style gives the impression that a style is a finite set of possibilities, hence a particular style can be exhausted. He also incorporates the idea of a "masterpiece" into the basic concept of style--a "masterpiece" is a production in a style that solves the "problems" of the style simultaneously. Thus, a style resembles a good riddle: several premises are given and the "answer" is whatever simultaneously connects (generates coherency for) them. Productions in a style approximate its "answer" until eventually the masterpiece appears. All subsequent work along the same lines is anticlimactic and redundant. Seeing styles as complex riddles differs, however, from seeing them as some sort of mathematical operation, for in the latter case all elements are equally good answers whereas the concept of a "masterpiece" expressly disavows such equality. Style-as-riddle makes use of the logic of intersection, but in addition to the masterpiece, all attempts (reasonable answers) are included in the style. In this fashion, the second version of the logical pattern paradigm understands coherency as membership in an equivalence class, but incorporates the phenomenon of growth in terms of successive approximation relative to an ideal intersection.

In both major versions of the logical pattern paradigm (commonly known as "structuralism"), coherency is understood as based on sameness. The first version does this straightforwardly; the second does this

indirectly by regarding all productions in a style as forming an equivalence class. Both are radically different from functionalisms, in principle, but very closely approach the so-called functionalism where "function" is used in the sense of mathematical correspondence. In this special brand of structural-functionalism, the overall idea is to understand X by showing how it "fits" with other phenomena in the culture-social structure, that is, by identifying equivalence classes of which X is a member, doing a total structural analysis. Thus, X is understood by relating it to its context, the essence of functionalisms, but this is done by looking for other phenomena which have the same structure.[6] This is well illustrated in Lévi-Strauss (1967 Chapter II) and Leach (1968 Chapter I), who differ from an earlier and related study by Radcliffe-Brown (1924) in that neither Lévi-Strauss nor Leach regard structural correspondences as contributing to the maintenance of the social order. "Maintenance" implies some sort of causality, either determinate or in the sense of constraining, and to the extent that Radcliffe-Brown's concept of function involves a contribution to the maintenance of the whole (1935), he proposed a functionalism rather than a structuralism.

But this detailed discussion of specific authors is getting away from the point, which is that "structuralisms," though conceptualizing

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[6] Although used in widely varying senses, the concepts of "structure" and "function" are intimately bound to one another. If one uses some notion of "structure," then he will also use some notion of "function," and conversely. Thus, all structuralisms employ some notion of "function"--the mathematical sense--but this does not make them functionalisms. (First pointed out to me by Prof. F. K. Lehman in the fall of 1971.)

coherency explicitly, do not account for stability, for they have nothing to say about temporality.[7] Functionalisms have the reverse problem: they weakly account for coherency by relying on various and changing notions of causality, but given this weakness, they conceptualize stability with some clarity, because their understanding of causality provides an avenue for comprehending what generates motion.

Historicism[8] is the last major paradigm for understanding coherency and stability. In principle, an historicism should take as its major focus the explication of certain concepts, such as "forces of history," "tradition," "cultural inertia," "cultural energy," and so forth. What is it about us that makes our past essential to understanding our present

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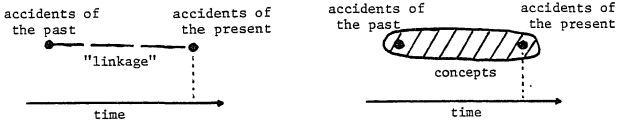
[7]Should one wish one's world view to be free of the concept of temporality, then, of course, pure structuralism would have an account of stability for stability would be identical with coherency. "Growth" would still be possible, just not measured in durations, only in order of appearance.

[8]Largely owing to critiques such as that of Popper (1964), "historicism" is often taken to mean nothing more than an attempt to explain the accidents of the present in terms of accidents in the past, which are themselves to be explained in terms of equally accidental happenings even further back in the past. I agree that such an historicism is indeed impoverished. But, the sort of historicisms I have in mind here would go much beyond this minimal form of temporal linkage. The goal of solid historicisms must be to develop concepts which underlie temporal relations. The kind of historicism criticized by Popper (see A) merely identifies events and claims they are causally related, somehow. Solid historicisms must fill the gaps between the events with concepts such that the past does have implications for the present and future (see B).



and future? How is the past lodged in us? What is the nature of historicity? To my thinking, this is largely an unexplored region. Most of us seem content to use opaque phrasings which merely label the phenomenon without conceptualizing it. Thus, Lowie's metaphor "cultural inertia" struck a resonant chord in me, but as I thought about it, I realized I did not have any clear idea what was being said. Nonetheless, it made me aware of how important the assumption of continuity was in all my understandings. I see that I have assumed that things will have stability unless made to change, rather than the equally logical assumption of the permanence of change. Similarly, Hallowell's study of Northern Saulteaux religion made me aware I had always taken for granted that a cultural notion continued to exist unchanged between its concrete activations in behavior and/or thought and that the only time things changed was when they were being "put to the test" in use.

But why should not culture change in an instant? Why should not I understand the world about me in completely new ways from moment to moment? It is here that anthropologists have traditionally called in the notion of human nature. We do not normally lose continuity with ourselves from moment to moment or day to day because that is the way we are put together. In this way, appeals to human nature locate the source of cultural stability in the individual substratum of culture (cf. Kroeber). But if Anthropology is the study of man, then surely human nature is one of its primary concerns and cannot be artificially ignored by assigning it to psychologists. Thus, understanding the nature of individual continuity becomes a central issue for Anthropology. In order to comprehend

the continuity of culture, one must understand how culture exists in man. This is the essential focus of what I think of as "psychological historicism." It derives from the man-in-culture or the superorganic perspectives discussed earlier and/or from a view seeing the individual as the "culture-bearing unit." From this vantage point, complexes would cohere owing to their interpentration in the course of their historical development within the individual, not because of what appears to the analyst as functional or logical relations. And, complexes owe their stability to the nature of individuals' subjective continuity (which will be the subject of the next chapter).

The other major kind of historicism, "sociological historicism," is rather difficult to imagine. The shining example of it is Tolstoy's conception of history as revealed in War and Peace, but here it is not made explicit, rather it impresses itself upon the reader semi-consciously. The image conjured is one of multiple subjectivities, each to be comprehended after the fashion of psychological historicism, but organized by a higher level of historicity pertaining not to people but to ideas themselves. This higher level may be the "superorganic" for Kroeber, the realm of "social facts" for Durkheim, and "culture" for White. For Tolstoy it was God. All of these difficult to imagine concepts may be seen as attempts to develop seriously some notion of a "group mind," a thing which accomplishes in its functioning what Wallace called the "organization of diversity" (1968). This anthropological motivation for postulating such an order of reality is that there are numerous ranges of knowledge which no one individual, no single subjectivity knows, that

is, that cultural knowledge is often distributed among multiple individuals, each person knowing only a part of the whole. The recently popular alternative to developing some notion of group mind is to stay at the level of incorporated subjectivities but see them as involved in same-level communication (cf. Wallace 1968; Van Baal 1971; and others). These issues are beyond my imaginative grasp, so I shall confine myself in this dissertation to the smaller scale problems posed by the question: how does culture exist in individuals? This is the first step, I believe, to the real issue of "sociological historicism" for it is part of it. But ultimately, psychological historicism is insufficient for understanding culture complexes, excepting those inclined toward full solipsism.

The task of an historicism is thus first to conceptualize the nature of man such that the apparent stability and coherency of culture is in fact possible. This will involve conceptualizing subjective continuity.

The kinds of phenomena that an historicism may hope to account for are those falling into the categories anthropologists call "survivals" and "selective borrowing." In these categories, functionalisms and structuralisms flounder. But the more fundamental, less obvious value of an historicism lies in its potential to account for the coherency and stability of "irrational" culture, that is, culture before it undergoes secondary rationalization. To do this, however, one must develop a paradigm of historicism, for very few if any are well-known at the present. At this point, I can only outline what sorts of questions such a paradigm must address itself to. How can the religious complex represented on the Obelisk Tello survive for hundreds of years severed from the natural

environment out of which it grew and made sense of? This is an incredible happening and an instance par excellence of cultural inertia.

In conclusion, I hope it has become clear that a very fundamental notion of Anthropology, one so "simple" it was regarded as intuitively obvious, is really a quagmire of confusion and debate. The most prominent modes of understanding culture complexes--functionalisms and structuralisms--I cannot accept completely. Functionalisms, understanding through contextual relativism, strike me as explaining portions of reality by pointing out other portions--they seem to have reality explaining itself. Structuralisms hold up logic as a metaphysical primary and lose all sense of temporality. Historicism offers the only potential to conceptualize what I sense as the real data of Anthropology without undue bias. The basic cultural data for me are what an individual experiences, thinks, and feels. The first fundamental problem to conceptualize is thus how cultural partibility forms and remains in the individual. Thus, I am saying that both coherency and one large aspect of stability are to be explained by how one envisions human learning and memory. Functionalisms and structuralisms are our particular cultural forms of secondary rationalization.

CHAPTER 5

BEHAVIOR, KNOWLEDGE, MEMORY, AND TEMPORALITY

5.1 Introduction

The preceding chapter concluded that the constitution of the individual's subjectivity is a major consideration for any deep understanding of cultural stability. Thus, I called for the development of a "psychological historicism" which would explicate how one's past resides and functions in the ongoing continuity of subjectivity which each person feels is his life. This chapter will propose some fundaments for such a psychological historicism. The basic issue is how culture lives in the individual.

The first section explores the relation of knowledge to behavior. The goal here is to isolate why knowing and behaving are so often regarded as some sort of dichotomous pair. When seen as a dichotomy, the distinction often leads to a dualistic philosophy--a mental world opposed to the material world. I feel it is necessary to regain a monism if one is interested in asking about the stability and coherency of ideas (components of culture). But, first one must understand knowing as a form of more general behaving.

The second section follows from the first and tries to demonstrate that most current modes of representing knowledge fail to perserve adequately the temporality of knowing. Essentially, this is a review of cognitive anthropology.

The third section discusses ways one might attempt to deal effectively with the time dimension of knowing and behaving. Rather than being content to build models of understander-systems, we must expand our aim and explore ways of modelling understander-producer systems.

A tangential section then interrupts this line of thinking. I shall digress to make reference to studies of biological rhythms. Here the point is to show that the time dimension of biological processes is becoming increasingly important for theorizing about how life systems cohere and achieve integration.

The last section comes to the heart of the matter. How can we best conceptualize the causes of cultural stability? How can knowledge exhibit stability? In other words, how should we conceive of memory? Several general modes of thinking about memory will be reviewed and then a selection made. Part of the consideration here is what image of memory naturally accommodates the fully temporal vision of active knowledge described in Section 5.4. That is, the first three sections of the chapter come together as part of the argument in favor of selecting the "deformed jelly" image of memory over more familiar and popular images.

To recapitulate, the first thirty or so pages concentrate on dispelling the philosophical dualism imbedded in seeing knowledge and behavior as a dichotomy. The focus is upon active knowing (i.e., thinking, feeling, cogitating) and developing an appreciation of temporality. Then there is a digression to show that the problem is felt in other disciplines. Finally, all of the foregoing plays a part in the evaluation of images of memory. Memory is the source of cultural stability.

5.2 Knowledge and Behavior

In common speech, "knowledge" is a grasping of Truth. This is an absolutist understanding of knowledge and smacks of metaphysics. Social scientists, however, speak of knowledge is a very different, relativistic sense. Rather than the handmaiden of Truth, knowledge is whatever someone or something knows. In this context, knowledge is just an analytical concept postulated in order to account for behavior.

This section will explore certain general notions which are part and parcel of the social scientist's sense of knowledge. What distinguishes knowledge and behavior? What is the relation of knowledge to behavior? I shall be concerned principally with two issues. The first I call the assumed reality of behavior. Here I hope to show that observability underlies our usual feelings of what constitutes behavior as opposed to knowledge. The second concern is the contrast between knowledge and behavior with respect to temporality. Most everyone agrees that behavior happens in time, but this is awkward to say about knowledge. Generally, I hope to reveal in this section that the knowledge/behavior dichotomy is a result of a matter of focus and degree (observability differences), and that this is reinforced insofar as knowledge is usually regarded atemporally.

The first comment I should make is that I am firmly convinced that we require some conception of "knowledge" in order to adequately account for "behavior." That is, I am convinced by and subscribe to a cognitive framework as opposed to a behaviorism. Not only do we process stimuli

and interpret them, hence knowledge intervenes between input and output, but our input is filtered and transformed as we initially receive it. Thus, even at the portals of the mind, we do not operate in accordance with the "dogma of immaculate perception" (Fraser 1975:75 quoting phrase by B. Kaplan). I do not feel I could argue more forcefully on this point than has already been done by numerous others. A particularly comprehensive argument is Chomsky's (1959) review of Skinner's Verbal Behavior, and a more readable exposition is Boulding (1975). Henceforth, I shall assume that knowledge is essentially related to behavior and responsible for it. Our concerns are how this is the case and how we should imagine or represent knowledge. First, let us consider some general observations before getting more specific in Sections 5.3 and 5.4.

Social scientists appear to accept the reality of behavior without question, whereas knowledge is often defined so narrowly and given so little scope that it becomes almost trivial. Behavior, however, never receives such demeaning treatment, even amongst those emphatically advocating ideation. In the least, behavior is revered as the data of social science. No one seriously advocates a science of "knowledge" without some reference to behavior, but the converse does occur. This asymmetry is intriguing and arises, I think, owing to the relative observability of "behavior." It is what we see, hear, smell, touch, or taste, and therefore part of what is real.

The usual social science usage of "behavior" is almost identical with common speech. If asked to give examples of behavior,[1] the

[1] I refer to "behavior" generally, not in the sense of "good behavior" or propriety.

man-on-the-street is likely to mention walking, talking, waving, winking, shrugging, smiling, laughing, sitting, and so forth. Few people would immediately think of things like listening, daydreaming, agreeing, cogitating, and smelling. The first sorts of things (the natural examples) are all easily and directly perceived by human observers. They involve gross muscular movements, large postural shifts or positionings, and/or certain specialized human doings to which we, as fellow humans, are particularly attuned. Although each of us is personally familiar with things in the second category (unlikely examples), it is very difficult to identify when and if someone else is doing them. On this extreme, there are some human doings which are difficult to recognize even in oneself, for example, "believing" (cf. Needham 1972). But, I do not want to get entangled in the semantics of the word "behavior," rather I want to draw attention to the fact that behavior is closely intertwined with human observational capacities.

As our technology has grown, so has the range of human observation. For example, I normally consider that the "behavior of the Sun" is its apparent daily course across the sky. Thanks to Copernicus, I disavow this common sense understanding and believe it is an effect of the Earth's behavior rather than of the Sun's. And, modern astronomers have been able to observe that the Sun revolves on an axis, has sun spots, and several other things so that the "behavior of the Sun" has many aspects. The telescope, mass-spectrometer, and other sophisticated instruments have had the effect of expanding the range of human observational capacities by bringing otherwise imperceptible motions into the

human perceptual range. Similarly, as de Kruif (1926) relates so eloquently, certain instruments and experimental apparatus have made it possible to observe microbes and hence to speak of their behavior. Such technological expansions occur frequently. Instead of direct observation, however, the humans using the instruments watch needles jump or lights flash or hear mechanical clicks.

While there are no instruments currently available capable of registering the behavior of knowledge in specifics, devices such as lie detectors seem to work reasonably well for gauging specific global mental states, and other instruments working on bio-electric phenomena of the nervous system indicate that perhaps someday knowing will be observable. Should that day arrive, then "mental behavior" may well replace "knowledge" in the terminology of social scientists. In the meantime, one is well advised to consider knowing as a form of behaving and not rigidly segregate the two realms.

If one agrees with this line of thinking, then knowing and behaving are not so different as we normally tend to imagine. For example, both should be susceptible to distributional plotting in space and time. Just as we can ask without awkwardness the contexts of some behavior, we can ask the contexts of some clump of knowledge. "Context," in a scientific framework, must minimally include spatial and temporal relations. With few exceptions, however, we social scientists tend to downplay temporality as compared with spatiality, and both are overshadowed with our predilection for intentionality. Given that my principal concern is cultural stability, I want to pursue and try to understand the temporal dimension,

which is not to say that I do not care about the others, but that I feel temporality is not currently appreciated sufficiently.

Temporality has two aspects: order and duration. As a tightly fused conjunction, these two aspects allow us to alternately see the progression of time as a continuous flow or as an incredibly dense, continuous abutment of internally cohesive events. The second image of time-flow is more compatible with the analytical impulse common amongst non-mathematical scientists, for the former view would require considerable detachment from a world where things are named and parcelled. Personally, I find the first image appealing, but I cannot operate in it, only with it, as an independent time scale by means of which I may keep track of events. So, I tend to see the world as a moving and changing constellation of events, a sea of cohesions. All movements (and "non-movements"[2]) are behaviors. As a social scientist, I am not concerned with the behavior of the Sun, of microbes, nor of magnetic fields; I restrict my attention to human behavior. And, as a social anthropologist, I concentrate mainly on cultural behavior. But, however restricted my area of interest, I am nonetheless interested in behavior, and one of the most fundamental aspects of behavior is that it transpires; it takes time. But, is this true also of knowledge?

There are four reasons why knowledge should be placed in a temporal framework. (1) Knowledge, in the relativistic social science sense, grows and develops in an individual throughout his or her lifetime. This

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[2]Following kinetic theories of matter, I think everything is in motion at some level. But, "sitting," for example, is non-movement at the level at which it is a phenomenon.

sort of temporality of knowledge is almost universally recognized. (2) When perceptions are received, there is an intervening processing time before the responses. This is true even of physiological reflexes, where the time is very small, and much more apparent in situations such as responding to questions, interpreting a motion picture, answering a pre-lim question, or deciding whether to strap or brail a catch of salmon. Here the temporality in not that of long-term construction, but of interpretative use, that is, perception processing. Another way of saying this is that understanding information takes time. (3) In addition to "processing time," which focuses upon the cognitive make-up of a person as an understander-system, knowledge is also active as behavior transpires. While frequently the case that such knowledge is sub-consciously active--one need not fix attention upon the acts of picking up a cup of coffee, for example--it is nonetheless the case that knowledge, as the source of coordination in behavioral acts, must be active as behavior is produced. Hence, in addition to construction and processing times, there is also "supervision of performance" time. And (4), as already discussed, knowing something is a form of behaving, and since behaving takes time, so does knowing. This phrasing is, however, ambiguous and loose because we normally would say that an individual "knows" X if he or she can conjure that knowledge from his or her memory, and we do not require that it be constantly active from moment to moment. Thus, perhaps we should speak of the active and passive aspects of "knowing." Here, I merely want to highlight the fact that "thinking X" or "cognitating X," although perhaps not producing any directly and easily observable

behavior, is nonetheless a kind of behaving and takes time, whether or not it is "observable" with today's techniques and instruments.

In a way, the above arguments for imbuing knowledge with temporality is a natural outgrowth of seeing knowledge as grounded in and part of the functioning of living things. For, while stones may be said to exhibit behavior, they do not have knowledge. The difference between stones and the higher life forms is an organizational difference, and "knowledge" arises in and is made possible by the internal complexity of such highly organized biological systems. In these systems, behavior is guided by intention as well as constrained by the physical processes characteristic of their constituent parts. I would suggest, however, that "intention" is noticeable owing as much to characteristic timings in behavioral responses as to one-to-many relations obtaining between inputs and outputs. For example, if humans lived a million years, then we might speak of the "intentions" of rivers as they cut meander patterns along their banks. This "behavior" would appear to us as a rapidly transpiring phenomenon, and we might impute "knowledge" to the river in order to account for it. Less outlandishly, imagine that we normally perceived the diurnal behavior of sunflowers in the way they appear to behave in time-lapse photography. It is very easy to see their movement relative to the Sun as a "desire" to face this orb of light and warmth. Perhaps they are even "studying" the Sun as part of a long-term astronomical research project? Such speculations aside, we associate knowledge with biological complexity and ground it in biological processes. From this framework, knowledge must have temporality in all the senses given above.

"Cognition is a biological phenomenon and can only be understood as such" (Maturana 1970:3); knowing is a kind of behavior peculiar to higher life forms.

Unfortunately, we social scientists tend to talk about knowledge, or knowing, in atemporal ways. We use words such as "idea," "notion," and "image" which obscure the temporal dimension. Perhaps this is understandable given how poorly most of us handle (perserve) the inherent temporality even of behavior. For example, in Chapter 3's characterizations of seining, I preserved only the sequential ordering of the work, yet that was more specific than many, or most, anthropological descriptions of cultural behavior.

Summarizing this section, I have argued that knowing is a form of behaving,[3] and following from this, that knowledge, like behavior, is inherently temporal and in several senses. Since culture consists of human knowledge (either all or only socially transmitted, depending on whose definition), it is completely fundamental to a consideration of cultural stability that one consider also how knowledge has stability. But, if knowledge is represented atemporally (as I hope to show is the common practice), then one cannot speak of stability at all, for stability is a characteristic of something in time.

In the next section, I shall examine some examples of modes for representing knowledge. I believe it will become clear that all of them

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[3]"Knowing" in the active aspect is straightforwardly "mental behaving." The passive aspect of "knowing"--knowledge inactive and in memory--is rather awkward to talk about at this point. It can be thought of as "potentially behaving," but this subject is the concern of Section 5.6.

are generally atemporal. Together, Sections 5.3 and 5.4 concentrate on the temporality of active knowledge, that is, they will evaluate models of knowing in terms of whether they incorporate processing time and behavior-supervision time into their schemata. In Section 5.6, we will turn our attention to how knowledge can have stability, that is, we will focus on memory--where knowledge is when passive. But, before dealing with memory as the source of stability of knowledge and hence culture, I want to illustrate my claim that most current images of knowledge are indeed atemporal.

5.3 The Dualism Inherent in Common Images of Knowledge

As we observed at the end of the last section, knowledge (or knowing) has two modes--active and passive. Behavior, however, is only active. Perhaps this difference underlies the widespread illusion that knowledge is not fundamentally temporal. We feel that knowledge does not dissipate in the organism when passive, and we therefore postulate "memory" as an essential component of human cognitive make-up. But, instead of taking account of both modes of knowledge and recognizing the basic temporal dimensions thereof (especially of the active mode), we commonly fixate on the seemingly atemporal qualities of passive knowledge and build our representations on this grounding. True, standard representations of knowledge allow for change and gradual build-up in the individual, but the changes are discontinuities, leapings from one static structure to other equally atemporal structures. For example, given a knowledge structure K, we may well consider long-term developments leading to K (structural changes), but we seldom ask how long it takes to think K.

This common oversight is compatible with a view seeing "ideas" as the constituent units of "knowledge."

But, "ideas" (Platonic forms) exist in a mental world devoid of time. This mental world (the mind in each individual) is a place of immutable forms where the only sense of temporality is one of ordering on transformations between structures. Hence, we incur a subtle dualism rather than a scientific monism. The cleavage into two orders--mind and body--is a result of refusing to place knowledge in the same space-time matrix as behavior.

In the following portions of this section, I shall synoptically review three current images of knowledge, each of which represents knowledge after the fashion of atemporal ideational systems. My purpose is to show that each mode of representation fails to incorporate the temporality of active knowledge, and by so doing, fosters a dualism based upon the mind/body dichotomy (philosophy) and reflected in the knowledge/behavior dichotomy (social science).

Cognitive Maps--

The notion of "cognitive map" has become in cognitive anthropology what "culture complex" was for the Boasians. It is used rather frequently and with little or no attempt to define it either directly or through citation. Its appeal is intuitive and based on the obvious analogy with geographic maps. The earliest citation for the expression I have found is Wallace's (1968:19) reference to an article by Tolman (1948). Perhaps for the best, it has become a loose metaphor for certain kinds of

depictions of knowledge rather than a well-defined, specific formalism. In any case, here I shall use it as a covering label for a genre of representational modes including such things as componential analysis (Goodenough 1951, 1956, 1970; Lounsbury 1951, 1964a; Conklin 1955; etc.), folk taxonomy (Frake 1969; Conklin 1969; Kay 1969; etc.), semantic networks and tree structures (Lamb 1970; McCawley 1970; Woods 1975), relational representations (Witz and Earls 1974; Van Esterik 1972), and assorted forms of distinctive feature charts.

In simplest terms, a cognitive map provides a breakdown of some portion of the world, tangible or intangible, into categories and their relations one to another. It is like a map in that it charts some terrain, and it is "cognitive" because the terrain is composed of conceptual categories. While most of the specific formalisms I have lumped together under the heading of cognitive maps concentrate on the distinguishing features of categories (their intensional or definitional meanings, Wallace 1965), this is not universally true. For example, "relational representations" are quite free, as a formalism, to capture and incorporate as much of the "encyclopedic knowledge" (Sperber 1975) as the analyst requires or has the energy to identify. My reason for this somewhat unusual lumping is that each variety depicts knowledge as a static constellation of related categories. The apparent principal impulse motivating such a mode of depicting knowledge is the hope of revealing the logical relations amongst systems of differences, hence all fall under the general rubric of "structural analysis."

The usual criticisms of cognitive maps, as here used, fall into two classes. On the one hand, some antagonists argue that it may be well and good to have a rigorous description of how the Bonga Bonga classify insects, but that no insight into the nature of human life seems to follow. This line of attack seems really an expression of aversion to formalistic studies of culture rather than a genuine criticism of the basic concepts. For example, if the people responsible for doing "ethnoscience" (a major producer of cognitive maps) had concerned themselves with religious concepts or abstract political concepts or human values, I rather doubt whether this humanistic objection would be made (cf. Berreman 1972). On the other hand, several authors have wondered about the basic significance of cognitive maps, that is, are they empirically accurate representations or merely analytically adequate. This second line of discontent centers around the issue of "psychological saliency" and arises principally from within the ranks of practioners (e.g., Burling 1964; Wallace 1965; Wallace and Atkins 1960; Romney and D'Andrade 1964; Spradley 1972).

The first body of criticism is based on an unfavorable evaluation of the results achieved by those who construct cognitive maps, and though perhaps justified, is not an <u>in principle</u> argument against the concept. The second sort of criticism raises a more serious objection, but again does not really jeopardize the notion of cognitive map specifically. Rather, the issue of psychological saliency poses an epistemological problem for all of social anthropology insofar as the discipline would attempt to describe knowledge instead of being content

with grossly observable behavior (see discussion of range of "empirical incorporation" in Section 3.5).

My criticism of the cognitive map genre of representing knowledge is that it almost totally ignores the temporal dimension.

> A central topic in the discussion of cognition is that of concepts. For many linguists and anthropologists, concepts are static entities to be isolated. Many anthropologists are not interested in cognitive processes, in other words in the dynamics, but in learning as much as possible of the cognitive categories enforced in a culture at any one time. A cognitive system is then not dynamic, but is rather some sort of static world view, or picture, or map. (Arbib 1970:332)

Of those concerned with the psychological saliency (reality of their cognitive map representations, a few have made attempts to incorporate aspects of temporality (e.g., Romney and D'Andrade 1964; Sanday 1968) in the hopes of getting a more "real" characterization. These efforts toward making cognitive maps more empirical are admirable but hardly sufficient because they use only the ordering aspect of time-flow and not full temporality. In this respect, they are akin to the Toy 1 and 2 Series in Chapter 3 and suffer similar maladies. Furthermore, though admittedly exhibiting a step in the direction of appreciating processing time, they say very little about behavior-supervision time. For example, the typical ethnoscience elicitation procedure concentrates on man-the-cognitive-judgement-maker when informants are asked, "What kind of P is x?". I will agree that this line of inquiry poses a somewhat natural behavioral scene (similar to children's questioning, medical diagnosis, sorting salmon while unloading, etc.), but are we really to suppose that when we act spontaneously under our own motivations we guide our behavior

from moment to moment with cognitive maps? How? As I walk into my classes in the morning, I do not "sort" ("classify," etc.) my behavior to the terminal category WALK (as opposed to SKI, JOG, SHUFFLE, . . .). Rather, I walk. An exercise in concept sorting, although perhaps done briefly as I decide to walk rather than take a bus, will not get me to class on time.[4] I can see no way that it is related in a responsible way to the feat of walking. So, I cannot accept cognitive maps as more than a very static mode of depicting one kind of mental process (sorting, classifying), and, even in those versions which pay some heed to processing time, the only aspect of temporality preserved is that of order. How long does it take a Bonga Bonga to classify a given beetle?--.5 seconds, 13 seconds, 2 minutes, 1½ hours, 20 years? Do Bonga Bonga accelerate processing time as they near the terminal categories of their folk taxonomies? These sorts of questions highlight the atemporal nature of cognitive maps as a form of representation. Thus, the notion of cognitive map reinforces the dualism expressed in the knowledge/behavior dichotomy.

Grammars (Axiomatic Systems)--

"Culture grammar," like cognitive map, is an often used expression among social anthropologists of the cognitive persuasion. Rather than alluding to geography, however, the notion of culture grammar casts culture as some sort of language or code system. The basic notion can be

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[4]"Unless you can use your Image [Boulding's (1975) sense] to do something, you are like a man who collects maps but never takes a trip." (Miller, Galanter, and Pribram 1960:2)

traced back to famous ancestors in Anthropology (cf. Petit 1975 or Kessing 1976:138-139), but one of the earlier anthropologists to speak of a "grammar of culture" was Kluckhohn in his lectures at Harvard in the late 1950's (Colby 1975:914). But, by "culture grammar" I do not refer to an articulated set of core social values. Instead, I use the phrase after the fashion of Goodenough, Frake, Keesing, Geoghegan, and others who envision many small-scale, domain-specific grammars.

> Culture is not a well integrated, holistic system but rather an orchestrated constellation of many small systems. These small systems are what I shall refer to as culture grammars. (Colby 1975:914)

Before outlining what I perceive as the general features of the grammar notion, I must mention that I am using it as a covering label for assorted formalisms, as a linguistic umbrella under which I find several sorts of work. All of these are distinct from the cognitive map genre in that they are concerned with preserving sequential ordering relations obtaining in strings of behavior (either speech or action) and are not content with some simple display of categorial relations. The emphasis is upon cultural processes over and beyond categorial structuring. Other than this seemingly basic commonality, grammars vary rather widely in their specific mathematical configurations.

From perusal of selected literature, I see two major camps or ways to construe culture grammars.[5] The first major group employs some

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[5]Certain kinds of kin terminology analyses have employed the notion of "grammar" as used here (e.g., Bock 1968; Coult 1966; Gatewood 1972; Lounsbury 1964b; Scheffler and Lounsbury 1971), but since the output of such systems of generative rules are single labelled categories, I shall not discuss them in the present context. They are applications of generative rules (grammars) which accomplish in principled ways what componential analysis did ad hoc.

axiomatic system (cf. Wall 1972 for definition) to generate tree structures in which time goes from left to right at the level of the surface outputs. The most famous example of this mode is Chomsky's formalism applying to the syntax of language (1969, 1970). The second major group envisions an axiomatic system like the first, but sees time as following the routes established by the productions of the system itself, that is, time flows along the branches of the tree structure itself rather than left to right on its derivations. Figure 11 illustrates this difference.

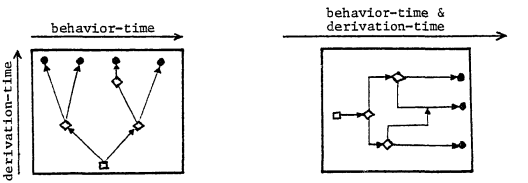

Figure 11. Two ways axiomatic systems (grammars) deal with temporality

Within Grammar Type II, there is a division into two sub-types. The first of these uses an indefinite number of nodes (has a large "alphabet") and correspondingly has a large number of production rules. Thus, for example, Keesing's (1971) application of Geoghegan's (1971) formalism has as many different nodes and "decisions" as were deemed necessary to cope

with his ethnographic data with the result that each node and decision occurred only once in the representation. The second sub-type employs a proportionally small number of nodes (small alphabet) and production rules, but they can feed into one another and by recursive application generate a potentially infinite number of derivations.[6] This second sub-type is exemplified in the Colby's narrative grammar for Eskimo folktales and in much more sophisticated form by Newell, Shaw, and Simon (1958), Hovland (1960), and others in artificial intelligence work.

The basic idea inherent in speaking of grammars is that one hopes to describe behavior as a reflection of the derivations produced by some assortment of axiomatic systems. And, the power of such systems lies in the concept of "rule." This is an exceedingly general and powerful notion which may or may not be tied together with, alternatively, mechanism or intentionality. Thus, for example, one could consider the law of gravity as a kind of "rule" (taking a formal sense of rule) and thereby construe rules as "just so" descriptions, as statements of mechanistic facts. In this sense, rules provide universal characterizations of the distribution of occurrences. In a different sense, rules are statements of "proper" behavior (behavior seen as guided by intentions), and they do not necessarily predict behavior. In yet a different sense, rules are regarded to underlie understandings and actions be they "proper" or not (cf. Scandura 1970; Spradley 1972). Contemporary social scientists interested in cognition tend to regard "rule" either in the first or

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[6]This second sub-type has more "generative power" than the first.

third sense listed above, usually the third.

Amongst authors interested in describing culture grammars, whether in some formalism or in natural language, there is variation with respect to how rigidly they maintain some distinction between knowledge and behavior, or as it is often phrased, between competence and performance. Some, for example Colby, would prefer to restrict the use of the term "grammar" to only those representations that deal with actual neurophysiological coding or attempts to approximate these. Others, for example Chomsky, Wallace (1968), and Goodenough (1971), maintain a rather abrupt break between knowledge and behavior. In this latter case, grammars are supposed to describe what might "acceptably"[7] occur, but not how this is to be accomplished with the human body.

In another jargon, "recipes" consist of ideas and understandings, as opposed to "routines," which are recurrent ways of performing recipes.

> . . . when we speak of recipes, we refer to ideas and understandings about how to do things, and when we speak of routines and customs, we refer to the actual doing of them. (Goodenough 1971:31)

Recipes appear to be a smaller scale version of Goodenough's earlier notion of patterns for behavior, and routines correspond to patterns of behavior (1969). The change in scale has not, however, overcome the fundamental differentiation between knowledge and behavior.

While not advocating reduction of "ideas" to the neurophysiological level of description, I cannot be comfortable with such strong distinctions between knowledge and behavior as advocated by Chomsky and

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[7] Chomsky uses "grammaticality" rather than "acceptability" (1970: 11). Acceptability, he says, belongs to the study of performance, not competence.

Goodenough. In principle, I agree with Colby, but operationally I am content with characterizing "knowledge" without reducing it to the level of cellular mechanism. But, there is a halfway point between the two views! So long as the representations do not generate views of knowledge irreconcilable with real-time understandings pertaining to biology, they are sufficient. That is, representations of knowledge must incorporate temporality into their basic concepts. Grammars do not accomplish this sufficiency condition because they address only sequence (ordering) to the omission of duration.[8] This is the case whether the particular grammar be my Toy 1 and 2 Series from Chapter 3, Geoghegan's grammar for personal addressing in the Philipines, Colby's abstract grammar for folktales, or Chomsky's grammar for English. That is, whether a grammar has generative power or not, whether it has seemingly "natural" units (its alphabet or auxiliary alphabet) or not, grammars generate strings of elements in ordinal-time. Thus, they do not preserve the real-time quality of the behavior and knowledge they address.

Therefore, the notion of culture grammar, while an advance over cognitive map, still reinforces the dualism inherent in speaking of knowledge and behavior as some kind of genuine dichotomy.

Developmental (Morphogenetic) Structuralism--

Unlike "cognitive maps" and "culture grammars," developmental structuralism is associated with one scholar and his collaboraters.

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[8] In Section 5.4 I shall review two ways in which "grammars" may attempt to capture durations. Here I exhibit my judgement that neither way succeeds satisfactorily.

This man is Jean Piaget, the founder of genetic epistemology.

Given the scope of genetic epistemology and its wide ramifications, it is rather difficult to extract just those portions which are directly pertinent to the issue being investigated in this section, but I feel no motivation to review Piaget's thinking in toto. He has expounded his views quite cogently in three small books (1971a, 1971b, 1972), and others have attempted holistic reviews of his work (e.g., Phillips 1975; Turner 1973). So, I shall focus my attention upon how Piaget copes with the temporality of knowledge.

> Intelligence is an adaptation. . . . Life is a continuous creation of ever more complex forms, and a progressive equilibrating of these with the environment. (Piaget 1951a:180)

> Our problem . . . is to explain how the transition is made from a lower level of knowledge to a level that is judged to be higher. The nature of these transitions is a factual question. (Piaget 1971b:13)

These quotes reveal one of Piaget's central interests: "intellectual evolution, that is, the development of knowledge and of its different modes, the genesis of the forms of thought, of their adaptation to experience, and the rules which they obey" (Piaget (1951b:154). Thus, certainly Piaget is concerned with the becoming of knowledge, its psychogenesis, its development.

Eschewing both pre-formism and innatism, he argues most forcefully for the view which sees knowledge as constructed through an ongoing process of equilibration between assimilation and accommodation. "Assimilation" is the process through which a scheme (structure) absorbs sensory stimuli. "Accommodation" is the process through which a scheme

(structure) changes so as to recognize differences in external objects. They are inverse processes which, when in balance, or equilibrated, have temporary stability. As the individual develops, the structures transform to higher level structures. The driving force of these transformations is, however, the constant process of equilibration whereby the complementary processes of assimilation (synthesis) and accommodation (analysis) form progressively more abstract cognitive structures which are more attuned to externally perceived reality.

> In brief, intellectual adaptation, like any other adaptation, is a succession of equilibria between the complementary mechanisms of assimilation and accommodation. The mind is adapted to reality only if accommodation is complete, that is, if no further change occurs in reality that would necessitate an alteration of the subject's schemata. Conversely, there is no adaptation when a new reality imposes mental and motor attitudes contrary to those adopted through the contact with the givens of a previous reality: there is no adaptation without synthesis, that is to say, assimilation. (Piaget 1951a:184)

Without further entanglement in the generalities of genetic epistemology, especially the concept of "reflective abstraction" and the properties of Piagetian "structures" (cf. Piaget 1971b:17-18 and 1971a:3-16, respectively), we can see that Piaget conceives of knowledge in terms of schemes at the sensory-motor level of intelligence and in terms of structures at higher levels. Apart from the considerable attention he gives to long-term development of such intelligence, does he really incorporate the temporality of active knowledge in these concepts?

> I think that human knowledge is essentially active. To know is to assimilate reality into systems of transformations. To know is to transform reality in order to understand how a certain state is brought about. . . . To my way of thinking, knowing an object does not mean copying it--it means acting upon it. . . . Knowing reality means constructing systems of transformations that

> correspond, more or less adequately, to reality. . . . Knowledge, then, is a system of transformations that become progressively adequate. (Piaget 1971b:15)

Despite the language in the quote above, which emphasizes acting upon objects, moment-to-moment temporality of knowledge is not addressed. Piaget does not concern himself with "assimilation time" or other aspects of the time dimension in his studies of intellectual evolution. Rather, he is interested in how one static system of structures becomes transformed to others in the course of a lifetime. And, his methodology is geared to this problem alone. Instead of recording how long it takes a child to respond to one of his experimental situations, he is content to extract the results of experimentations and compare them with results obtained months or years later.

In the case of sensory-motor schemes, Piaget does deal with the real-time operation of knowledge structures to a degree. He notes that simple actions exhibit four forms of coordination--additive coordination, sequential coordination, setting up correspondences amongst actions, and establishing intersections amongst actions (Piaget 1971b:18, 1971a:63)--which contain the elements for and are the basis of reflective abstraction. In this way he argues that the roots of intelligence are in sensory-motor schemes. But, schemes do not appear to capture the full temporality even of actions. Rather, they, once again, are held to account only for ordering relations to the omission of duration.

Thus, whilst Piaget is greatly concerned with temporality of knowledge at the level of years and lifetimes, that is, its developmental history, he does not really conceive of knowledge in such a way that he

could conclude thinking takes time. Processing time and behavior-supervision time are not conceptually incorporated into the notions of scheme and structure. So, despite the major advances of his genetic epistemology over extremely static conceptions of knowledge, his recognition of temporality is at a very much higher level of scale than the sort of moment-to-moment, real-time conceptions that are needed if the knowledge/behavior dichotomy is to be dissolved.

In summary, none of the widespread images of knowledge current in the social sciences adequately appreciate the temporality inherent in active knowledge. I suggest that this omission largely underlies and reinforces a dichotomy which if taken seriously implies that the social sciences are in fact branches of dualistic philosophy. The scientific conception of reality requires that all existences be amenable to placement in a space-time framework. But, current images of knowledge appear to do one of two alternatives: (1) place knowledge in a mental world distinct from the rest of reality; or (2) put the burden of recognizing real-time processes of thinking and knowing in the neurophysiologist's lap.

I would suggest that there is an alternative in our manner of imaging knowledge. While we wait for major breakthroughs from brain surgeons, biochemists, and neurologists, we can begin to describe knowledge in ways which fundamentally recognize durations as well as orderings. This, however, will require accompanying alterations in our methodologies. No longer could students of cognition jerk "ideas,"

"cognitive maps," or "schemes" from the flow of behavior, collate them, arrange them into a pictorial system of vertices and arrows, and be content with such models of understander systems. Instead, the behavioral flow would be diagrammed on a real-time line, and whatever analysis follows will have to be capable, in principle, of reconstituting that initial real-time depiction of the data.

So long as the temporality of knowledge is preserved, the possibility of reconciling concepts with biological activity remains open.[9] Failure to do even this much makes it difficult indeed to imagine how ideas arise out of the material world and its processes.[10]

5.4 Dispelling the Dualism, Knowledge with Temporality

The general problem in representing knowledge is to make the representations articulate with the real-time flow of thought and behavior. Failure to do so results in a dualism, a conceptual abyss separating knowledge and its empirical referent. How then can we bridge the competence/performance, or knowledge/behavior, gulf? There are several

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[9] Similar objections could be made with respect to social science treatment of space, but we tend to think that knowledge can be associated with human beings as individuals, and the unity of each person's "me-ness" provides the basis for making the spatial locus of knowledge coincident with that of the "knower." "The propensity of states of consciousness to cluster around a me which persists and remains the same in the course of time, is a postulate of psychology, as space is a postulate of geometry" (Claparedé 1951:67).

[10] An interesting metaphysical argument predicated upon our usual feelings about consciousness, space, and time may be found in D'Arcy (1912) in which he explicitly argues that the Self, as distinct from the Body, does not exist in space nor time.

solution strategies available.

One way to overcome the dualism is through some kind of monist idealism. In this case, time, space, and the material world would themselves become merely "ideas," ideas which exist insofar as they are churned out by some consciousness. I can try to imagine such a system of thinking in my spare time, but it would not pass as Anthropology nor increase my understanding, as I have come to "understand" understanding.

A second strategy would be to uphold the distinction between competence and performance, in Chomsky's sense of the terms, but to envision competence as part of a larger, more encompassing performance system. In this case, the relation between competence and performance would be that of competence monitoring performance and precluding uninterpretable performances before they are executed (cf. Lehman 1974:xii-xiii; Durrenberger 1971). Competence, here, would not directly generate performances, that would be handled elsewhere in the performance model, but it would impose general well-formedness conditions on performances, thus ensuring that "behavior-salad" does not occur. While this possible solution has its attraction, it also has two problems: (1) How does this all important monitoring take place? and (2) The issues of modelling behaviorial production are merely pushed under another labelled area of the overall system and not resolved.

The third strategy is to imbue conceptions of knowledge with full temporality. Rather than beginning with atemporal knowledge structures and working down to temporal behavior, one begins with temporal behavior and tries to build up to knowledge structures, but without losing

temporality. This appraoch would begin with recognizing sensory-motor behavior as coordinated action in which "knowledge" is responsible for the coordination.[11] At this level of "knowledge," incorporation of full temporality is nearly unavoidable because the notion of coordination makes sense only in terms of timing--initiations, durations, decays, and the real-time interrelations of these.

Essentially, the third solution strategy regards active knowing (thinking, cogitating, action coordination) as behavior. The reason for beginning with sensory-motor activity is methodological rather than a reflection of strong adherence to positivism. Simply, it is easier to obtain high quality data of the sort required if the "behavior" is easily observable. This does not mean that all active knowing need necessarily produce behavior observable with today's technology. But, the conceptual techniques whereby sensory-motor "knowledge" perserves its basic temporality may well have utility when the analyst comes to study "in the head" active knowledge.

There appears to be two modes of grappling with the task of behavioral simulation, for that is what we are discussing. The first mode aims to satisfy two yearnings by cleverly articulating static representations of knowledge (Images) with fully temporal behavior via the concept of Plans. Here, knowledge structures retain their usual character of categorial relations, but Plans unravel these to form multi-level programs for the performance of sequences of behavior. The second mode follows

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[11] At this juncture, it is interesting that Kroeber regarded motor habits as part of culture.

the third strategy (above) more closely and aims to imbue all knowledge structures with temporality.

Using abstract schematics and not particulars, the first mode of behavioral simulation operates as diagrammed in Figure 12 below.

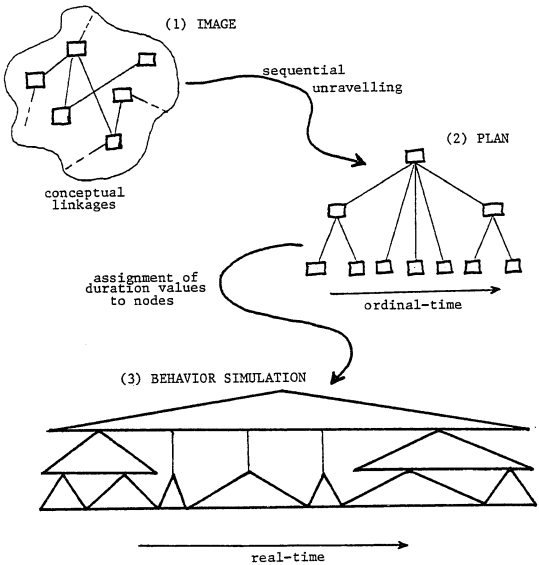

Figure 12. Behavior simulation by assignment of duration values

Although their models look slightly different from Figure 12, Miller, Galanter, and Pribram exemplify a relatively early and solid attempt to fill in the "theoretical vacuum between cognition and action" (1960:11). In addition to the question of getting aspects of temporality (explicit ordering and hierarchical levels of ordering) into the overall picture of cognition-plans-action, they incorporate feedback loops and the notion of equilibrium. These two concepts operating together provide some explanation for what drives plans forward in their execution. The two come together in the concept of a TOTE unit, which is the unit of analysis in their system of thinking (ibid:21-39) as opposed to a reflex arc unit. Also, they explicitly state that it is only control which their ordering relations (arrows) imply, not energy or even information (ibid:27-29).

There are several possible criticisms of this first mode of dealing with behavior simulation, but one stands out most in my mind and is a rarer form of criticism than the others. Basically, the dynamics of the simulation arise externally and not from within the model itself. The external flow of time is the chain drive which propels the time-chunks of the model. Let me expand on this with an analogy.

Imagine that concepts are like little paratroopers in a plane waiting to jump into action. They do not jump out all at once, but must go in certain orderings. Now, simulation after the fashion of Figure 12 functions as if the paratroopers are hooked up to a moving line which has coded tags on it which match the paratroopers' tags. When a tag on this moving line gets to the paratrooper whose tag matches, the

paratrooper is dragged along and thrown out of the plane's door. What is wanted is to conceive of cognitive structures in such a way that instead of being dragged along by some external force, they voluntarily rise and leap into action at the proper time. That is, we want to have simulations which are internally dynamic.

Other criticisms of Figure 12 style of simulation are: (1) Each cognitive structure (each node) is like all others except in terms of its hierarchical positioning; (2) The method of identifying units of behavior (which correspond to the cognitive structures) is ad hoc and usually based on common sensically perceived unities rather than motivated by some notion of "natural units;" and (3) For any given portion of behavior, there would usually be several systems of actions involved, each involving multiple levels of organization and coordination with the other systems. Each of these objections could be handled in various ways by revision of the simple model (Figure 12), but the issue of achieving internal dynamics is much more difficult to accommodate.

Miller, et al., do in fact achieve something resembling internal dynamic drive by their conjoining of feedback loops with a general equilibrium notion in TOTE units. But, does this drive mechanism really accomplish internal dynamics in real-time? I believe it does not. The reason for this failure to capture full temporality lies in the fact that TOTE units are control mechanisms rather than production units, that is, they regulate sequence of actions but not durations of actions. Thus, to get real-time modelling, cognitive structures are

assigned duration values (made into time-chunks) rather than seen as inherently characterizable by their temporal properties. Nonetheless, their work is remarkable when compared to cognitive anthropologists' modelling because they fully realized the necessity of making the model move of its own accord.[12]

I know of only one mode of behavioral simulation which solves all the objections raised with respect to Figure 12. This approach was developed by Klaus Witz and others at the University of Illinois. In what follows, I shall attempt to sketch only the rudiments of their first inroads at simulation. In this A-1 Project (Witz and Hart n.d.), the concern was with "simple" actions of a very young child. Later work has addressed higher order cognitive structures, but from present purposes, the modelling of actions is sufficient as an illustration of the approach.

From the very outset, Witz's concept of a "cognitive structure" differs radically from most other's notions. There are six general characteristics of Witz's conception of "cognitive structure" (Witz and Easley 1975:5-13).[13]

1. A cognitive structure (c.s.) is an object of our (analysts') imagination; it is a theoretical hypothetical entity.

2. A c.s. in a given person is a unit of organization-in-time which is treated just like an anatomical structure

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[12]The Toy 1 and 2 Series of Chapter 3 completely lack driving force, for example. Similarly, Geoghegan's, Keesing's, and other anthropological modelling typically are content to let the flow of real-time move the system.

[13]My presentations of these six characteristics are very close paraphrases of Witz and Easley's, sometimes even verbatim quoting, but without the accompanying expositions.

in as far as it is imputed to be part of that person's make-up. It exists in that person for some period of time, and it can be said to be activated and thus underlie his behavior on some occasions or not activated during other periods.

3. A c.s. is related to observable phenomena in two ways: (a) when it typically comes into play in the presence of certain aspects of a situation, it is assimilating the aspects in question; and (b) when activated, there will typically be certain aspects of observable behavior which can be taken as evidence that the c.s. is active. The first relation is called R_1 and the second R_2. Both are regarded as highly many-many relations.

4. Intuitively, cognitive structures are regarded as dynamic formations in two senses: (a) they have characteristic variations in such dynamical aspects as intensity, effort, tension, and so forth, over time; certain inertial properties; different modes of internal, dynamical coherence; and they are tied dynamically to other structures in various ways; and (b) they have internal constitution as is necessary to produce the dynamical properties they display.

5. Cognitive structures are specified as mathematical objects of some kind, whether that be static or dynamic in the mathematical sense.

6. Cognitive structures are not all of one type. Each species of cognitive structure has its own characteristic levels of dynamical coherence which are reflected in various time constants characterizing its internal operation. In addition, cognitive structures may vary with respect to their mode of dynamical coupling with other structures.

From these six generalizations about cognitive structures, I would like to focus on the significance of the R_2 relation and the emphasis upon multiple kinds of cognitive structures. Then, as an example of one kind of cognitive structure, I shall discuss their concept of a "node."

If cognitive structures were related to observable phenomena only through the R_1 relation, then there would be only a little difference

between them and Piaget's "schemes." Cognitive structures would again be regarded as constituent parts of understander systems, and not understander-producer systems. But, by saying that a given c.s. is active not only when assimilating, but also when behaving, and that it is one and the same c.s., Witz and Easley have made major progress, conceptually, in bringing about a reconciliation of knowledge and behavior. This R_2 relation is what conceptually justifies all subsequent characterization of cognitive structures in terms of their dynamical operation, i.e., intensity, build-up, decay, inertia (in time), etc. It reflects a full recognition that knowledge (conceived of in terms of cognitive structures) does in fact underlie and is responsible for the production of behavior, and in another sense, that it is very much part of behavior.

In allowing for many different kinds of cognitive structures, they retain the right to be empirical. The issue is to discover through detailed investigations what sorts of mathematical objects are required to capture the nature of cognition, not to see whether one can cram data into a preformulated mode of representation. And, in the course of investigations, it has indeed been the case that several species of cognitive structures have been identified (e.g., "nodes"--Witz and Hart n.d.; "prosodic field"--Witz 1974b; "S-structures"--Witz 1974a; and others--Witz 1973, 1976b). This sort of empiricism reflects an (unfortunately) unusual attitude on the part of formalists interested in social science concerns, but one which pays large dividends if followed through.[14]

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[14]Bright (1968) calls for investigation of other forms of relations (besides hierarchy and n-tuple analysis) in the study of cognition, but

As an example of one type of cognitive structure, we may take "nodes," which were originally defined as the lowest order cognitive structure for purposes of the A-1 Project (a behavior simulation project). Thus, "nodes" derive from studying sensory-motor behavior. "Mathematically, a single node a is an entity characterized by the following axioms:

> (1) At each moment of time, a is at a certain level of activation, specified by a number between 0 and 1.
> (2) Let x denote the activation level of a, and let t denote time. Then there is associated with a a family of trajectories in the (t, x) plane, called the characteristic trajectories of a, with the following property: if at some moment of time the activation level of a is raised from 0 to some level u > 0, then, in the absence of other influences, the activation level of a as a function of time will follow the characteristic trajectory that passes through point (0, u)." (Witz and Easley 1975:15; cf. Witz and Hart n.d.:27)

Among other things, these axioms describing a "node" give nodes the property of forward inertia. This is the essence of their internal dynamics, and can be interpreted in two ways. First, once a given node has been activated, it has to continue in its usual fashion for a certain period of time, that is, it does not stop until its usual trajectory of activation-in-time comes to an end. And second, if at any given moment the activation level of a given node is greater than zero, then it must have been proceeding for some time prior to that moment. Together, these mean that the true state of a given node is described not by the simple level of activation at a given time, but rather by the internal

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does not call into question these well-known types of mathematical relations with respect to appropriateness. The issue in formal work should be "what is X really like?" and not "can X be fit into this well-known formalism?".

lapse-time of it (Witz and Hart n.d.:29-32).

Nodes may be linked to one another in various ways to comprise systems of partially autonomous functionings. Such systems are held to underlie sensory-motor actions, in one sense, and to simulate such actions mathematically in another sense. This duality of interpretation is typically represented in diagrams of node systems by a "wavy line" separating cognitive structures and their behavioral, action referents. This wavy line cleaves knowledge from behavior, hence the distinction is not entirely obliviated. But, the concept of a "node" has done much to abridge this conceptual chasm because it is fundamentally grounded in the temporal functioning of actions (its behavioral referent).

The difference between conceiving of knowledge in terms of "cognitive structures" (Witz's sense) and in terms of labelled clumps with associated duration values is subtle. "Cognitive structures" are identified by their recurrent temporal patterns, thus temporal regularity is the principal ingredient defining them. But, if the method of getting full temporality is merely an assignment of duration values, then the objects to which these are assigned can be identified by other means. In this second, usual fashion, the units of knowledge may be quite arbitrary with little or no relation to natural units of behavior. Indeed, units identified in this second fashion tend to be based upon normal language categorizations, and there is no principled reason for believing that language categories segment the stream of behavior accurately with respect to recurrent temporal patterns. The R_2 relation of cognitive structures to behavior can thus be seen as the essential linkage which makes possible

characterizing knowledge as fully temporal, for it is this relation which conceptually permits using temporal functioning as a basic component of identifying units of knowledge.

In order to illustrate the differences between envisioning knowledge as composed of cognitive structures and the usual ways, let us take a statement by Goodenough (1971:22)--"The human approach to experience is categorical,"--and ask what this might mean in terms of the cognitive structures view of knowledge. Can "cognitive structures" accomplish what "categories" are supposed to do? Yes. The R_1 (assimilation) relation obtaining between a c.s. and aspects of situations or objects accomplishes synthetic formation of equivalence classes (cf. Piaget's discussions of "assimilation"), and the nature of this relation (many-many) ensures that similarities will be preserved despite the differences induced by the act of assimilation. So, cognitive structures achieve in their own way what is called "classification" in the language appropriate to the categorical view of knowledge. Therefore, we do not lose any conceptual power by replacing the notion of "category" with the notion of "cognitive structure." But, do we gain anything?

Yes. The dualism inherent in the major current images of knowledge (all categorical) is overcome, or at least greatly abridged, by conceiving of knowledge as composed of cognitive structures. This is so because cognitive structures, although located on the opposite side of the "wavy line," are saturated with full temporality. This saturation guarantees that knowledge (active) and behavior are in the same, scientific reality. The wavy line is a methodological, observational cleavage, not an in principle theoretical dichotomy.

The anthropologist reader may well be wondering by now what all of this has to do with the ways anthropologists typically describe their data and try to account for them. Is not all this discussion about the temporality of knowledge really pertinent only if one is interested in doing very detailed and relatively microscopic enthnography? What difference does it all make to the ways anthropologists go about accounting for knowledge systems (culture)?

These are quite legitimate questions at this juncture. The connection between such microscopic, nit-picky debates on how we should conceptualize knowledge and macroscopic concerns with cultural stability is not transparent. But, I believe that if a person regards knowledge as being composed of dynamical cognitive structures, then historicisms begin to have much greater appeal than structural-functionalisms as the proper mode of accounting for cultural stability. Suddenly, it is not so easy or meaningful to draw causal arrows amongst categories, rather one becomes concerned with whether knowledge structures are active together, how often, and with what intensity and regularity. How do cognitive structures develop and become lodged in memory such that they persist from the past into the present? What is the time structure of an individual's knowledge, and how does its modes of functioning become jelled in memory?

Before returning to some of these problems in Section 5.6, I would like to digress for a while and discuss some of the insights on temporality arising from the study of biological rhythms.

5.5 Parallels from Chronobiology

The reason for including this rather obliquely related section in this chapter is that I want to show that temporal considerations are becoming important in theorizing elsewhere in the scientific community, specifically in Biology. Sociology and Anthropology have borrowed from this discipline often in the past. The "organic analogy" has had great influence in the study of culture and society. In what follows, I do not mean to imply that we should borrow lock-stock-and-barrel from our biologically oriented colleagues, but I do think we should be aware of how even they are beginning to recognize the inadequacies of organic functionalism.

Chronobiologists are becoming increasingly prone to regard the time structure of an organism as the glue which is responsible for coherency and stability.

> Time cycles may provide the epoxy that melds the fragments of our substance into a coherent whole . . . (Luce 1971:4)
>
> Temporal structure, like the structure of matter, must be an important aspect of the anatomical and functional ability of organisms. Without temporal structure, indeed, we could not exist. (Luce 1970:14)
>
> Until recently, however, nobody considered the possibility that the entire body and brain might depend upon an integrated time structure. The human being is often treated as if he were--or should be--a constant system with homeostatic balance, capable of great flexibility in dealing with exigencies outside. There is growing evidence that all mammals are predictably changing from one hour of the day to another. (ibid.)

Biological rhythms have been observed and documented for several hundred years. Flowers, insects, birds, fish, mammals, and single cells exhibit rhythmic behaviors. There are rhythms with periods of seventeen

years (locusts), one year (migrations of birds), one month (menstrual cycle), one day (sleep/wake cycle), three hours (left and right nasal breathing), ninety minutes (certain sleep rhythms), a few seconds (heartbeat), and thousandths of a second (certain enzyme reactions). In short, rhythms abound in life systems. The question is what produces them: some external stimulants or internal functioning.

This issue--whether biological rhythms are exogenous or endogenous--has received intensive study since the 1930's. Erwin Bünning (1936) studied the light-sensitive response of flowers and found that they continued this typical rhythmic behavior even in constant conditions. As a result, he saw this circadian rhythm (around twenty-four hours) as controlled by some kind of "biological clock" which was endogenous in the plant (cf. Ward 1971:153-161). A host of other studies, all using the same basic logic,[15] have shown rather conclusively that many biological rhythms are endogenous rather than exogenous. Although a few still hold out for the view that all rhythms are exogenously controlled, notably Brown (e.g., 1969), most chronobiologists now agree that many biological rhythms are produced endogenously, that is, by biological clocks. However, the burden of proof is always laid on the proponents of biological clocks.

If a given biological rhythm is not a reflection of moment-to-moment reaction to some external stimulants, then there are only two

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[15]The logic is to observe some rhythmic behavior in an organism and then place the organism in an environment of constant conditions (constant light or darkness, constant temperature, constant pressure, etc.). If the rhythmic behavior persists, then it is either the case that it is endogenously controlled or some aspect of the environment is still fluctuating causally.

ways it could be explained: either it is learned or it is inherited (Cloudsley-Thompson 1961:199). This problem is the concern of many works, but it does not concern me so much as the problem of how to envision, or model, the result in either case. We must imagine some way in which energy can be drawn from a constant source and converted into more useful alternating cycles (Ward 1971:160; also, cf. Bateson 1972b:481-482). And, there are several opposed views on this.

One camp seeks to model biological rhythms as some form of oscillation. Here there are two options--either inertial oscillation or relaxation oscillation. If an endogenous biorhythm is produced by an inertial oscillator biological clock mechanism, then the model involves two simultaneous motions (behaviors) which are inverses of each other. A relaxation oscillatory, however, involves the notion of cumulative build-up, then release (relaxation) which returns the system to its initial state (cf. Ward 1971:105-107).

The second major camp seeks to account for endogenous biorhythms through the idea of negative feedback. Here one imagines linked sequential processes (each taking characteristic times) that regulate one another's progress.

There is no conclusive evidence arguing for one view or the other, but an experiment performed by Mills (1973:27-84) would tend to support the idea of an inertial oscillator model in at least one case.

> Most physiological functions are involved in homeostasis and hence are controlled by negative feedback mechanisms. It is a natural property of such a system to oscillate, so it is superficially tempting, for example, to ascribe the rhythmicity of the adrenals to the negative feedback

> of cortisol upon the anterior pituitary production of ACTH, or the rhythm of urinary acidity to an over-correction of a low plasma pH. Where this "pendulum" hypothesis has been tested, it has been clearly disproved: the production of ACTH remains rhythmic even when the response of the adrenals is absent . . . (ibid.:71)

Here, the linked processes were disconnected surgically, yet one of the processes continued in its characteristic rhythmic behavior.

One basic insight resulting from the study of various biorhythms is that the human body is not a biological clock; it is a clockshop. Seemingly everyone agrees with this whether or not they are in accord with one another regarding the nature of the clocks. Some would like to see a single master clock responsible for coordinating all the others,[16] but this is not at all necessary.

Low level debates aside, more and more biologists are becoming attuned to the importance of temporality in biological phenomena. Despite the numerous points of disagreement in chronobiological studies--exogenous or endogenous, inertial or relaxation oscillators or negative feedback loops, single master control clock or multiple levels of coordination achieved in some other way--timing (initiations, build-ups, and decays) is being recognized as a crucial dimension of life systems. For example, humans respond quite differently to certain drugs depending on what time of day (point in their biorhythms) the drugs are administered (Luce 1971:173-194). Instead of a simple two-dimensional textbook diagram of body parts and their functionings, what is required is

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[16] Rappaport (1971) sees "religion" as this master control system in culture.

a three-dimensional representation, that is, functional interrelations in the usual sense (two-dimensional) but stretched out in time (here, the third dimension). The temporal articulation of organic functioning is just as important as the simple, atemporal, abstracted identification of body part functions.

Thus, the "organic model," which has dominated Sociology and Anthropology for decades, is beginning to be modified even in the field of inquiry which originally produced this mode of thinking. Structural-functional thinking is modifying as temporality becomes increasingly recognized. If biological processes are regarded in such a way that their characteristic dynamical organizations are fundamentally recognized, then it seems to me that they will bear more similarity to the "cognitive structures" vision of the nature of knowledge than the standard categorical vision.

Before returning to the mainstream of this chapter, I would like to briefly illustrate two ways that temporal aspects of human life have been dealt with in the social sciences, for I am by no means the first to see the possible ramifications of biorhythms in social life.

Chapple and Coon (1942) represents an early attempt to use some features of the temporal dimension in efforts to characterize personality and social interaction. They extracted five variables which could be used to describe social interactions: (1) the amount of interaction (time sum); (2) the frequency of interaction (how often), (3) the origin-response ratio of each participant; (4) the rhythm of the interaction

(the basic interaction rate); and (5) the degree of synchronization (ibid.:51). Numbers 1, 2, and 5 will be of concern later in Section 5.6; numbers 3 and 4 are pertinent here.

The origin-response ratio is clearly a different sort of variable than the others. The other four variables are not specific in terms of what kinds of conclusions may be drawn or what subjects studied. They would apply reasonably to any human interaction whatsoever. The measuring of something called an "origin-response" ratio, however, is already geared towards the study of timidity and aggression, of dominance relations. This variable may be an appropriate operationalization for the study of dominance, but there is no reason given why this abstract concept should be given preferential treatment over other equally a priori categorizations of kinds of behavior.

The rhythm of an interaction brings to mind an immediate host of interesting problems. If it is true that each person develops in the course of his or her lifetime a characteristic tempo or cadence for interacting with others, then how is this rhythmic behavior related or grounded in more basic biological rhythms? Subjectively, this phenomenon of a basic interaction rate registers most dramatically whenever two people of very different rates try to discuss a common and immediate interest. A person with a rapid interaction rate will feel uncomfortable trying to converse with someone with a very slow rate. Often, the "faster" person will misconstrue the other's "slowness" as stupidity, and, on the other side, "fastness" is associated with lack of profundity.[17]

[17] Although changing recently, the relatively slower speech habits of southern folk in the United States has been incorporated into stereotypes (northerner's) as evidence of dimwittedness.

Other than the "origin-response ratio," I find Chapple and Coon's variables very much in line with the sort of temporal appreciation for which I have been arguing. Though not very helpful in doing behavior simulation, the variables are characterizations of the temporal ground on which human life is constructed. Generalized from interaction situations to human behavior, I think that the sheer quantity of time one does something and/or how often this something is done is largely responsible for the stability of that behavior. True, there are other considerations, but these two are potent observations. They do not of themselves lead to the "cognitive structures" vision of knowledge, but they complement such a view as part of the larger effort to come to grips with the simple fact that behavior (hence knowledge) happens in time. Chapple and Coon's focus on rhythm of interaction sees only the most obvious and striking kind of temporal phenomenon, but despite this limitation, it is an important insight.

Another but more recent attempt to study human behavior temporally is the microkinesic approach developed by Condon and his colleagues. Like Chapple and Coon, Condon is concerned primarily with interaction rates and rhythms, as opposed to durations and dynamic contours of cognitive structures regarded as entities. But, the time frame of Condon's studies is very small (the interval of interest is one to two second behavior).

The basic technique is to chop up the natural flow of behavior using a 16-mm motion picture camera. The number of pictures (frames) taken per second may vary. Several dimensions are selected for

investigation--such as head positioning, eye movement, hand positioning, phonetics, bioelectric activity, etc.--and these are arranged as the rows on a transcription grid where each frame (picture) is a column. By comparing one frame with its neighbors, the analyst identifies change points in each of the selected dimensions. For example, "reaching down to the floor" may take one or two seconds to perform, but its initiation and termination may be located in single frames. These change points are the focus of the analyst's interest.

Condon has identified two related phenomena which together may eventually provide the leverage for overturning our normal, common sensical understanding of human interaction. The first of these he calls "self-synchrony" and the second "interactional synchrony" (Condon and Ogston 1967; Condon and Sander 1974). Self-synchrony is manifest at the microkinesic level of organization insofar as the various change points in a behavior transcription tend to occur together rather than each dimension's change points acting independently. The effect of this micro-coordination is that there are eradic pauses during which no dimension registers a change (initiation or termination), but then suddenly a little flurry of changes occurs. Interactional synchrony refers to those cases where two individuals are coordinated at this microkinesic level such that their respective "pauses" (in their self-synchronizations) co-occur systematically.

Now, if self-synchrony where based on simple rhythmic behavior, then interactional synchrony could be explained in terms of entrainment. But, the pauses are eradic, sometimes taking five frames (columns, hence

time), sometimes seven, three, etc. This fact makes it very difficult to understand interactional synchrony. If rhythmic entrainment cannot be used as an explanatory concept, then we would naturally like to see interactional synchrony as the result of individuals sending, processing, and responding to messages emitted to each other (the social equivalent of Newtonian particle mechanics). But, the time scale is so small that it severely stretches one's imagination to believe that some message is sent, received, processed, and responded to in 1/32 of a second! These two phenomena, if they should indeed be phenomena rather than statistically understandable chance happenings, suggest that:

> . . . the "bond" between human beings should be studied as the expression of a participation within shared organizational forms rather than as something limited to isolated entities sending discrete messages. (Condon and Sander 1974:101)

Chapple and Coon and Condon represent ways one may go about studying human behavior temporally. Condon's work preserves the time organization of behavior much more so than Chapple and Coon's because he does not simply do frequency counts (a way of averaging occurrences over time). Rather, he studies the inherent relationships of the behavior in its naturally occurring temporal milieu. Both of these examples, however, are limited in comparison with Witz's mode of tackling temporality. They are nonetheless compatible with the cognitive structures vision of knowledge, and they are closely grounded in the substrate of biological rhythms.

5.6 Memory and the Stability of Knowledge

In the previous sections of this chapter, the focus of attention has been primarily upon the temporal quality of active knowledge, that

is, the dynamic shapes of knowledge as it is being "known." It is now time to turn to passive knowledge, or knowledge-in-memory, and explore how it might be best to think about this.

To begin, let us first try to place memory as a concept in terms of its position in some metaphysical system. For example, in a truly structuralist world, would memory function (conceptually) as it does in a world of constant becoming? No. If eternal forms existed as the constituents of "reality," then time would not be such a meaningful concept--there would be no universal flow of time--and with the dimishing conceptual power of time, memory would also become rather insignificant. Memory, here, might well become merely a pale shadow of present consciousness for the present consciousness would be "timeless" in its motion amongst the eternal forms. In our scientific cosmology, however, universal time flow[18] is independent of consciousness, and thus, memory becomes one of the crucial explanatory concepts in understanding our own awareness. Memory binds the present consciousness with its past and therefore is responsible for its continuity and stability. Rather than floating completely freely on the surface of a moving present, consciousness is bound to its past encounters and ruminations, that is, consciousness floats on a sea of memory. I mention these thoughts merely to show that "memory" is not a necessary concept, but that it is congruent with our culture's dominant cosmology. Memory is the subjective register of time. Insofar as time is held to be independently real, memory's significance as a concept increases.

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[18] See Fraser (1975:11-46) for a historical discussion of the development of this imaging of time. Relativity theory has significantly modified the universality of time flow, but within neighborhoods, time flow is rather uniform.

The conceptual function of memory is, thus, that of providing for the phenomenon of subjective continuity. Continuity implies persistence with the possibility of modification. The degree of persistence is the same as the degree of stability. Therefore, "memory" is responsible for the stability of knowledge, hence also culture.

But, "memory" is not a monolithic, internally homogeneous entity. Rather one should speak of both levels and kinds of memory. I shall only touch upon these subjects here because my interest is in what kind of general image we should have in conceiving of memory, not in the more analytic impulse of being precise about specific levels and kinds of memory.

There is a fairly common distinction between short-term and long-term memory. These are regarded as levels of memory functioning which have different characteristic properties. Some see three levels rather than just two--immediate, intermediate, and long-term memory (Pask 1970: 366). Regardless of the exact number of levels, the basic idea is that short-term memory holds data for the present consciousness to work with and upon, whereas long-term memory is where information is stored and the past really resides.

> The brain, as an active device driving the action of an organism, must monitor what is going on in its environment, to extract information relevant to its action. It also needs memory, in that past experience should allow it to better adjust its present action. We don't just act on the basis of looking at the world, categorizing what is in the here and now, each millisecond recognizing the world anew, classifying it and then trying to act. Rather, we have, over a great deal of time, arranged information into what you might call a long-term memory--an overall model of properties of the world to help determine our actions. (Arbib 1970:335)

With respect to kinds of memory, as distinct from levels, one could speak of visual versus auditory versus tactile versus imaginal versus representational and so forth (cf. Horowitz 1970:Chapter 5). But, rather than this carving based upon our sensory bases, I would call attention to Piaget's distinctions amongst several senses of "memory."

Piaget distinguishes between memory proper and general conservation of acquired knowledge and behavior. In the latter case, he suggests we speak of "conservation of schemata" whenever we are talking about "the ability to reproduce whatever can be generalized in a system of actions or operations (habitual, sensori-motor, conceptual, operational and other schemata)" (Piaget and Inhelder 1973:4). In the case of memory proper, he suggests we restrict the usage to "reactions associated with recognition (in the presence of the object) and recall (in the absence of the object)" (ibid.). He goes further to discuss the difference between recognizing or recalling a specific event, face, or episode as opposed to recognizing or recalling a generalized notion.

Only because this dissertation is in Anthropology rather than Psychology or Artificial Intelligence, I want to lump all the various senses of memory discussed above together. For detailed work, the distinctions must be recognized and dealt with appropriately, but I want to take more distance from the intricacies of this complex subject and look at the grand outlines of various ways of imaging the nature of memory. So, henceforth I shall use the term "memory" in a very wide sense with the understanding that one could be more specific, but that this would not make any difference for my purposes.

What are some basic images of memory? How do they differ?

There are three very general models or images of the nature of memory: the storage vault, the file system, and the compacted past. Let us examine each separately.

In the storage vault notion, knowledge is put into storage as it is received, and one must rumage about to find some portion. Implications of this view are that knowledge comes in chunks (complexes), that perceiving and conceiving are essentially identical, that knowing is therefore passive, and that once placed in storage knowledge remains unchanged.

The file system image seems to be the dominant one in contemporary Psychology and Artifical Intelligence (e.g., Norman 1969; Garvin, ed., 1970 Section IV; Bobrow and Collins, ed., 1975; Culbertson 1963; Newell, Shaw, and Simon 1958; Hovland 1960; etc.). By "file system," I refer to the view which sees cognition as an information processing system of operations. The implications of the file system image of memory are that perceiving and conceiving are not the same, that information (knowledge) is stored after processing, that knowing is therefore active, that after placed in storage knowledge may or may not change, and that knowledge forms in chunks in order to be stored away. The file system allows for systematic retrieval, unlike the storage vault image.

The last major image is to see memory as the compacted past of an individual's experience both with the world and himself. This view does not imply that memory is synonymous with storage. Storage implies immutability, but memory does not (cf. Von Foerster 1970:29-30). Of the three major images of memory, this last one has the strongest

intuitive appeal, is the eldest, and is the least specific. The others conjure rather striking, reasonably precise visualizations, but "compacted past" remains an unclear linquistic metaphor. Before going ahead and trying to explicate one possible way of imagining memory as a "compacted past," let us first see why it might be superior, generally speaking, to the storage vault and file system visions.

The problem with both the storage vault and file system images is that knowledge (and behavior) has to be chopped up into chunks or clumpings before "storage" in memory is possible. If one assumes that these chunks or clumpings are either pre-formed or in some sense "natural," then either of these two visions of memory makes good sense. However, if one begins with the question that has provoked this whole dissertation, that is, how can clumpings (complexes) cohere and maintain their coherency through time, then both the storage vault and file system images are inadequate for they both assume what is to explained. I believe it is crucial to recognize that knowledge does in fact tend to clump into complexes, but these clumps are not at all the same as chunks. There is no rigidity to the borders of complexes. They are quite fluid. At the same time, they have cohesive forces holding them together. It is just that these forces do not often produce sharply segmented, discrete chunks. Owing to this, such a fluid clumping may be modified slowly and evenly without total reconstruction or piecemeal substitution of sub-chunks. Therefore, what is required of a generalized image of memory (and knowledge) is sufficient flexibility and power to accommodate the frequent cases where fluidity is evident as

well as those cases which superficially appear to exhibit chunkiness. That is, memory needs to be modelled with some kind of continuous mathematics rather than with some kind of finite, state-space approach.

A generalized image of memory (in the compacted past genre) was developed in an article by Witz (1976a) and elaborated in a graduate seminar jointly held by Witz and Lathrap in the fall of 1974 at the University of Illinois. The label for this image is "deformed jelly." I should now like to discuss this general conceptualization of memory.

The problem for the seminar was how to account for cultural stability. We began with Bateson's (1935, 1967a) concept of "schismogenesis." This concept was postulated in order to account for the dramatic cleavage between male and female sexual ethos in the Iatmul of New Guinea. The essence of Bateson's argument was that the two sexes play off one another in a complementary fashion thus exaggerating the characteristic behaviors of the other. This conplementary schismogenic force pushes them apart reciprocally and makes them different from one another, yet entwined by virtue of the necessity of the other as audience.

In addition to the problems of what starts the schismogenesis in the direction it takes and what limits its extent, the concept has a more fundamental problem.

Schismogenesis arises in social interaction. Hence, certain kinds of interaction generate the cleavage in sexual ethos and are responsible for the stability of that cleavage once produced. But, such a view would necessarily entail one of the following three ideas: (1) schismogenic forces are active continuously, from moment to moment; (2) each

social interaction (of the appropriate kind) reconstructs the cleavage anew; or (3) the cleavage initiated in an interactional context must persist unaltered in between such encounters. Since it is very difficult to imagine a "schismogenic force" as an independent law of nature similar to gravity, the first choice above seems quite unlikely. And, given the individual to individual similarities in Iatmul sexual ethos in addition to the fact that each individual develops over a period of childhood years the characteristic ethos, the second choice above does not seem reasonable. So, it must be the case that the schismogenic force arises in social interaction[19] and whatever groove is cut between "male" and "female" in each interaction is cumulative until the groove has become the chasm characteristic of adult Iatmul.[20]

But, in this case, schismogenic interaction cannot account for the stability of the cleavage it has produced! Rather, the long-term memory of the social interactants is responsible for carrying the progressively constructed cleavage between the sexes. Each little schismogenic interaction makes an Iatmul different from what he or she was before, but it is their long-term memory that holds these differences in the Selves until the next interactional situation where they get even more developed. Therefore, while schismogenic interaction may initiate and exert a sustaining influence upon the cleavage, long-term memory tissue has the

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[19] It makes no difference here whether "social interaction" involves two persons or may also be dialogue with the Self (cf. Blumer 1969).

[20] I think Bateson himself would probably take this alternative (cf. 1972a:194-278).

causal force underlying stability.

This rather lengthy recounting has argued the importance of memory in accounting for cultural stability. However, it has not argued for one image of memory over others. Nonetheless, the seminar was the springboard for the "deformed jelly" image. In the seminar, we began to consider or image what schismogenesis worked upon. We wanted to imagine memory such that it involved long-term accumulations and did so without requiring chunkiness. We wanted an image of memory that did not take a meat cleaver to experience, chop it into chunks, and store the chunks away in isolation from ongoing life. We wanted to envision memory such that it did not merely bring the past to the present in storage, but instead made the past alive in the present. In addition to these considerations, memory should be imaged such that the full temporality of active knowledge is perserved and not just ordering.

In the "deformed jelly" image, each exposure to a situation resulting in human behavior or thought is imagined to "deform" volumes of the individual's long-term memory tissue (ltm, for short). Each time a behavior is performed or a thought thought, some portion of the individual's ltm "jelly" is activated. And with each activation, that portion, that volume is made more specific, deformed. This is the most general level of description, but note that even here chunkiness is obviated in favor of more fluid images. Experiences impress themselves in a continuous viscosity where crystalline structuring, though possible, is not the rule. With repeated occurrences, portions of the jelly, subvolumes of this density become progressively and characteristically clumped.

The three-dimensional image above still suffers, however, from the common failure to allow explicitly for temporality. A sub-volume, a region of deformation is still seen all at once as a single picture. To get time flow into the image, imagine that such a region of deformation, when activated, ripples along at a constant rate (the flow of real-time) in a characteristic manner. This characteristic manner would be the dynamic contour, or flow, characteristic of a particular cognitive structure (c.s.). Figure 13 illustrates this image. If the figure does not help the reader's visualization, picture an ink drop dispersing in a beaker of water.

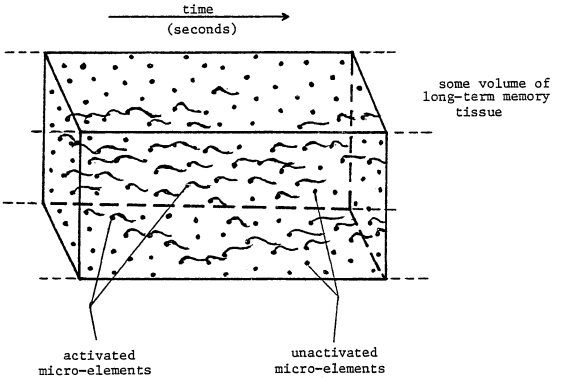

Figure 13. One schematic of the "deformed jelly" image of memory

With repetitions, a particular dynamic flow through a volume of ltm "deforms" the neighbor-neighbor relations obtaining amongst micro-elements in that volume. These micro-elements behave in such a way that once activated they continue in that state for a very short time (compared to the duration of the total dynamic flow) and then subside. But, with recurrent involvement in a particular dynamic flow, they develop particular relations (valencies) with their neighboring micro-elements. And, it is these relations amongst neighbors that reflect the "deformation" and lodge the particular action or thought in memory.

One way to mathematically model the development of "valencies" amongst micro-elements, that is, one way of modelling deformations in ltm tissue, begins by imagining that micro-elements are the nodes in a network. Each path between neighboring micro-elements has an associated probability of becoming part of a larger pathway should a micro-element on its left become activated (in Figure 13, time goes from left to right). Thus, if a given micro-element is activated by the subsidence of one of its left-hand neighbors, then it will activate some sub-set of its right-hand neighbors as it subsides. In this framework, the dynamic flow characteristic of a given cognitive structure is represented in terms of a particular pathway or pathways through this network of micro-elements. The activations of the micro-elements along this pathway(s) carry the dynamic flow along its course much like particles of water carry a wave. However, each time a particular path between neighboring micro-elements is involved in carrying a given dynamic flow, that is, each time that path is part of a pathway, the probability obtaining between those two

neighboring micro-elements increases. Thus, in the terms of this sketched model, "deformation of ltm jelly" can be translated as an altering of the probabilities in neighbor-neighbor network pathways. In common English, this means the more a person thinks a thought or acts a given way, the more that thought or action coheres as a replicable unity.

I am not necessarily advocating one way of modelling "deformation" over others. Here, I want only to illustrate that the "deformed jelly" image can be made more precise. Further conditions could be adduced and added to the above model, and it would become richer and richer. For example, one could simulate several cognitive processes by further stipulating that in vacuo the probabilities between neighboring micro-elements tend toward random distribution at a constant rate. But, such fancier aspects of a full-blown formalism would lead us too far from the major points to be made concerning the superiority of the "deformed jelly" image of memory over the file system image.

If the reader still feels that the "deformed jelly" image is all loose talk and metaphor, despite the rudimentary Bayesian modelling sketched above, then there is the following line of argumentation. Is the file system image (an information processing view of memory) really any better? I think not.

I suggest that a file system image of memory may appear more concrete owing to the fact that it has been operationalized in numerous real life contexts. For example, libraries and large wholesale stores operate after the fashion of the file system image. An order for an

item comes in, someone looks in a catalogue system to find where that item is located, and then someone goes into the storage area or warehouse, finds the item, and brings it back to the front office. This pattern of operating occurs in American society thousands of times an hour. We are intimately familiar with the basic procedure. It happens in some version when we order a Big Mac at the local McDonalds, when we ask where the cheddar cheese is in a supermarket, when we order an electric toothbrush from Sears. But, to say that because of all these situations where the file system image is actualized that the image is more real and not metaphor when applied to human memory is just poor logic. When we liken memory to such systems of storage and retrieval, we are just making an analogy about which someone like Woody Allen could make a comic movie. True, it is a very familiar analogy, but it remains nonetheless an analogy sustained by numerous metaphors.

If this "it's all metaphor" criticism may be regarded as answered, what then are the relative merits of the deformed jelly image of memory over the popular file system image? Why should we select it as our guiding general image over one with which we are more familiar?

(1) In the deformed image, the past is truly alive in the present. Rather than memory being a large storeroom in some kind of spatial limbo where each experience is indelibly placed or totally reconstructed anew each moment of life, memory is the long-term continuous accumulation of modes of acting, thinking, and feeling. Present consciousness is intimately entwined with its past from moment to moment. Each new experience shimmers in short-term memory and then sinks into the fabric

of long-term memory deformations. And conversely, current functioning involves incredible densities and volumes of long-term memory structurings. The present Self is the living, accumulated past Self.

(2) The deformed jelly image does not require chunkiness. Chunkiness is required if one imagines that memory involves placing items in storage, on shelves, or in specified locations on magnetic computer tapes. Instead, cases of apparent "chunks" are regarded as unusually rigid deformations in a normally fluid milieu. They can be handled in the deformed jelly image, but are not mandatory.

(3) The deformed jelly image embeds the temporality of active knowledge in its basic conception of memory. The temporal structure of an action, thought, or feeling, its dynamic flow is exactly what is lodged in the neighbor-neighbor deformations amongst the micro-elements of the jelly. The file system image is geared toward the view of active knowledge based on grammars, concepts, and categorical relations. The deformed jelly image is grounded in the cognitive structure (c.s.) mode of dealing with active knowledge.

(4) The deformed jelly image offers a very unstrained way to go about coping with the general problem of thought and behavioral flow. Rather than speaking of cross-indexing, code matching, or more generally "linkages," one speaks of resonances, activation levels, dynamic patternings, and simultaneous dynamic flows. The file system image has severe difficulty trying to allow for innovative associations, because, if memories are chunked parcels retrievable only by systematic matchings, how can new thoughts, feelings, or actions arise. The file system image

cannot cope with parallel processing, the polyphony of thought. But, if thought is seen as the manifestation of multiple dynamic flows resonating to varying degrees with each other, then "insight," "intuition," etc. are conceptually possible.[21]

(5) The deformed jelly image, inasmuch as it sees cognitive structures in memory lodged in the deformations in the neighbor-neighbor relations amongst micro-elements, is much more in line with general insights concerning how the nervous system appears to work than is the file system image. For in the process of deforming these relations, the micro-elements themselves are not changed. This has a loose similarity with the apparent fact that "ideas" cannot be located in the brain by specific locality. Only the pattern of synaptic firing seem to be significant, and any given synapse does not change structurally because it has fired or not. The file system image, however, by virtue of its insistence on chunkiness of memories, omits any explicit consideration of the build up of more complex patternings from elemental, inert components.

Therefore, I recommend the "deformed jelly" image of memory as the better general model for conceptualizing the rudiments of a psychological historicism. The image is deeply imbued with historicity, temporality, and fluidity. It can handle a much wider variety of cognitive phenomena than

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[21] For another viewpoint using language similar to that appropriate with the deformed jelly image, but arising in philosophy rather than science per se, see Gaston Bachelard (1969). For example, "The poetic image is a sudden salience on the surface of the psyche . . ." (ibid.:xi), and "The exurberance and depth of a poem are always phenomena of the resonance-reverberation doublet" (ibid.:xix).

the file system (and needless to say, the storage vault) image, which is geared primarily towards sequential, step-by-step operations. And while by no means a close analogue of what appears to be the way the human nervous system works, it is significantly more in line with biological models than the file system business.

But, how do we understand the source of stability in memory? That is, it is clear that memory is largely responsible for the phenomenon of subjective continuity, hence responsible for the stability of knowledge, but we have not yet outlined how this is accomplished. I see four very different modes of contemplating this.

As mentioned in connection with Bateson's schismogenesis, one way to think about stability is to assume that patterns of action and thought are reconstructed anew with each occurrence. In this understanding, there is no need whatsoever for the whole concept of "memory." Rather, one would assume that there are universally applying instantaneous laws regulating the construction of thought and action systems. For example, encounters between Iatmul males and females generate similar cleavages in sexual ethos each time because of the operation of some set of laws. Or, in a more general example, certain social laws operate in all rites of passage to produce the three phrases of the rite (cf. Van Gennep 1969). Here the apparent stability of a thought/action system derives from the transcendental laws themselves, not from the memory structures of humans. There are very few, if any, such social laws known at the present time even for such general phenomena as social "passages" let alone at the level of specifics usual in ethnographic reporting. This

does not convincingly argue against such a view, but it does show that the social law conception for explaining stability has not done very well to date. And then, of course, there is the whole bag of issues revolving around whether "system-specific laws" are really laws at all.

The second mode of comprehending the stability of memory structures is to simply claim that once formed they are permanent and immutable. Certainly, this solves the problem of stability with one fell swoop, but it would require an infinite storage capacity and would obviate the concept of "assimilation," which appears to make such good sense.

The third and fourth conceptualizations are alike in that they both firmly base stability of knowledge in memory. In both, memory is not seen as equivalent to storage, but rather seen as an active equilibrating, harmonizing of the present with its past. But, beyond this juncture, they are radically different.

The third mode's basic concept in accounting for the stability of knowledge in memory is "consistency." Consistency is most peculiar if one tries to pin down exactly what it might mean. Nonetheless, it is quite basic to many people's understanding of why something might or might not exhibit stability. For example, in addition to allowing for the sustaining effect of successful assimilation of environment by already existing structures, Boulding uses this idea of "consistency" to explain why the Image may or may not change for internal reasons.

> The stability or resistence to change of a knowledge structure also depends on its internal consistency and arrangement. There seems to be some kind of principle of minimization of internal strain at work which makes some images stable and others unstable for purely internal reasons. (1975:13)

One can easily detect the strong similarity here between consistency used as an explanatory concept for cognitive (one person's) stability and its use by anthropologists of the superorganic persuasion at the level of culture-wholes. Basically, "consistency" derives its power from some kind of structural-functionalism. However, in lowering the level of such consistency arguments from culture to the individual, the basic logic of understanding does not alter. It functions in the ways outlined in Section 4.5. One examines the content of knowledge systems in order to discuss or imagine consistency, thus "consistency" pertains to a very widespread and powerful mode of explaining and understanding which I dub content explanation.

The impulse underlying all content explanation is to account for phenomena within a given domain or level in terms of their internal-to-that-level relations. That level is a single content level. Causality, in the case of functionalisms, obtains amongst discrete content elements. Similarly, in structuralisms, logical pattern replications are observed amongst discrete content elements. If these content elements form into tightly related systems, then they are likely to exhibit relative stability.[22] As suggested in Section 4.5, this approach in the social sciences has grave difficulties overcoming the everyday notions and modes of thinking prevalent in the analyst's own culture and time. Without having to make a judgement concerning the feasibility of a sound epistemology based upon this approach, I would like to move on and expound

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[22]Of course, if content elements are "tightly related" but in a deviation amplifying manner, then the system will likely be very unstable.

upon the fourth mode of comprehending stability in memory.

The fourth way is what I call basic historicism, and it derives from the deformed jelly image of memory. The basic logic is as follows: (1) the history of an individual's functioning has resulted in the long-term continuous accumulation of deformations in a substrate (the jelly composed of micro-elements); (2) these deformations have inertia directly related to the intensity or degree to which the deformations have been effected--just as it took energy to achieve a specific deformation, it will take like energy to undo or modify those neighbor-neighbor relations amongst micro-elements; and (3) therefore, a cognitive structure resists change in direct proportion to the extent that it is lodged in long-term memory tissue, that is, to the extent of the inertia of the deformation corresponding to it.

In basic historicism, portions of knowledge (cognitive structures) have relative stability owing to their lodgement in a substrate. There is no reference to what idea, thought, feeling, action, or emotion the cognitive structure may invoke in our normal, everyday language level of description. This mode of accounting for stability is not a form of content explanation. It does not matter whether the cognitive structure be called "monotheism," "hammer," "run," "pretty," "purse seine," or so forth. Such considerations of content are irrelevant to the conceptualization of stability. Only the extent to which there is a deformation of long-term memory tissue (the substrate) matters in terms of the stability of a given cognitive structure. And this is where Chapple and Coon's ideas, about how much time one spends doing x and how often one does x, are relevant.

Generally speaking, the more time a region of long-term memory is activated in a particular fashion, the more deformed are the neighbor-neighbor relations amongst the micro-elements in that region. And, the more these relations are deformed, the more inertia that deformation has. Hence, in general, the more often one does x (performs, thinks, or feels x), the greater stability x will have, the greater its resistence to change. But, there are special situations which will deeply and strongly "deform" long-term memory in a relatively short time.

For example, traumas are by definition just such special cases, and similarly, in a less morbid vein, flashes of insight do not last very long but can be quite powerful and continue to affect one's mode of functioning for years. So, time by itself cannot be used to determine the degree of deformation; there are other variables which time only glosses. We could perhaps speak of psychic energy expended or some related notion, but the major point remains that we think of stability (relative persistence of a coherency through time) in terms of the degree of deformation in a substrate.

In basic historicism, "ideas" are seen as high level modes of functioning supported by organizations in a substrate. Ideas are not chunky entities. They are particular dynamic flows through progressively constructed deformations. Modification is seen as partial change in the neighbor-neighbor relations of micro-elements, and, with this vision, modification can be finely graduated or radical. The model is powerful enough to conceptually accommodate all degrees of continuity.

The deformed jelly image of long-term memory is, then, a very powerful and unusual conceptual basis for a psychological historicism. Rather than holding "ideas," "categories," "concepts," or "actions" themselves as the rudimentary, elemental level for the analysis of human behavior, formal objects (micro-elements) of very small scale are postulated as the level of inert matter.[23] This makes possible an alternative to content explanations. It provides the groundwork for a true historicism based on continuous models and capable of incorporating full temporality. From within this framework, anthropologists no longer must make the irrational rational in order to explain its persistence. Rather, cultural stability, as it is manifest in individuals, may be understood in all its facets.

5.7 Summary and Conclusions

In a sense, the goal of this chapter has been to introduce the reader to two related conceptual schemes: the cognitive structures vision of knowledge and the deformed jelly image of memory. While not the same things, these two go together like hand in glove. As a conceptual package, they are remarkably powerful in coping with aspects of cognitive phenomena with which other modelling schemes have difficulty or simply ignore. I have merely sketched their rudimentary notions.

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[23] Any explanation of stability in any field of inquiry will have to postulate inert matter at some level, and it is tautalogical to ask about the stability of these inert objects (Witz 1978). Since my problem has been how "ideas" and motor habits have stability, it has been necessary to use an image which sees these as organizations rather than as primitive objects. Postulating micro-elements, as defined above, achieves this end.

For readers seriously interested in them, I recommend the numerous papers by Witz and his associates in which very detailed work and applications of the general precepts bear fruit on a wide variety of subjects. Nonetheless, I hope the differences between the cognitive structures and the categorical views of knowledge and the deformed jelly and file system images of memory are now clear.

At a less specific level, my main point has been that memory is responsible for the stability of culture. "Culture" is "knowing how to behave" where behave can mean act, think, feel, emote, etc. If one wants more restricted definitions of culture, adding criteria such as "socially transmitted" or "shared," then culture is some subset of "knowing how to behave." But, the point remains that culture is knowledge. Since memory is responsible for the stability of knowledge, memory is responsible for the stability of culture. Memory, as a concept, is an attempt to solve the problem posed by subjective continuity. There are other ways, but the mode of memory is a good one.

The focus of this chapter has been upon developing a mode of explaining stability and doing so differently from content explanations. Thus, memory is the substrate of knowledge-culture which gives knowledge-culture stability from moment-to-moment and day-to-day. The causal force is from one whole content level to another rather than the usual structural-functional causality amongst content elements within the same level. This mode of argumentation is a bit peculiar or unfamiliar in the social sciences, although it is common in the physical sciences. However, as a consequence of this mode of explanation, one is left with

only very abstract relationships between the content levels, not specific cross-level content-element mappings. That is, a given cognitive structure lodged in deformed jelly is completely anonymous from the point of view of my stability argument; it makes no difference at all what normal language label we might hang on it. Rather, its stability is a function of the inertia of its deformation in the jelly substrate. Thus, stability of culture is explained without reference to what specifically (which content element) resists change. Therefore, at this point, the whole issue of what produces cultural partibility remains only obliquely answered. We have a way of explaining the persistence of cultural complexes once formed, but not much to say about how cultural partibility happens in the first place. This must be addressed.

The questions at the beginning of Chapter 4 have been partially answered here in Chapter 5. We now have a conceptual system for explaining stability of culture complexes (clumpings of knowledge-culture). And, this is a significant step forward. But, part of the problems surrounding culture complexes concern how they form as entities, their becoming. It would be nice if a single conceptual system could handle both this becoming and the being (stability) of culture complexes. How might the mode of memory accomplish this dual function? I have already argued that memory is necessary to account for the being of culture complexes, and this remains, but can the mode of memory also at least partially explain the becoming of culture complexes?

As a first comment, the answer seems negative--memory pertains to individuals. To the extent that culture complexes exist both within and amongst individuals, the mode of memory can be only partially involved in an understanding of culture complex formation. That is to say, memory pertains first and foremost to a psychological historicism and not to a sociological historicism.

The deformed jelly image of memory does, however, make statements about how a culture complex (a clumping of knowledge-behavior) develops within an individual. Deformation of neighbor-neighbor relations amongst micro-elements in a region of long-term memory jelly is a process. The more often (gloss for more psychic energy) a region of jelly is activated in closely similar ways, the more deformed it becomes, thus the more locked-in that same pattern of activation becomes. It becomes progressively more rigid as well as progressively more stable. Jumping to the language appropriate for description of culture, this means that the more often one performs in (acts, thinks, feels) or experiences a given situation as a continuous happening, the more that episode will cohere as its experiential performance deforms the memory substrate. That is, the more often a configuration of content elements is experienced[24] as a similar temporal episode, the more likely it will cohere in memory. Except for this line of thinking, I must confess that what causes specific culture complexes to form remains a mystery to me. Basically, I feel the explanation lies in the simultaneous operation of multiple cognitive

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[24]"Experience" is used here to mean cogitation and/or action as well as in its usual sense. One's own thoughts and actions are experienced reflexively.

structures, that is, in the temporal articulations and resonances of cognitive dynamics, but exactly how this might work is not clear. Models could be devised, but I would not feel empirically justified in selecting one over another at this point.

In conclusion, the stability of culture complexes is understandable in terms of the inertia of deformations in the long-term memory tissues of individuals. The process of deforming this tissue is surely related to the problem of culture complexes' formations, but the precise relationship has yet to be worked out.

CHAPTER 6

SUMMARY AND CONCLUSIONS

6.1 Introduction

As a whole, this dissertation has been motivated by the desire to reconcile a cognitive perspective with discussions of culture. I want to get people back into the study of culture. I feel that Anthropology is the study of man, not the study of culture. This general impulse does not, of course, account for the specific way of thinking advanced in the last few hundred pages. But, it is nonetheless responsible for the general direction of my work, which I think could be called an exploration into human nature.

Finding the knot which binds the two halves of this dissertation together is problematic. Chapters 2 and 3 were concerned with salmon purse seining in Southeast Alaska. Chapters 4 and 5, on the other hand, have hardly mentioned purse seining, but instead have dealt with the more general and theoretical issues involved in seeing human culture as constituted in and of culture complexes. Compounding this integration of the two halves is the fact that what passes as an "unbiased ethnographic reporting" style of written language is in fact very slanted toward the functionalist paradigm. Thus, in writing Chapter 2, and to a lesser extent 3, I may appear to contradict my conclusions in Chapters 4 and 5. This is not, however, a real contradiction in my thinking, but rather an artifact of natural language description.

In writing at some length about salmon purse seining, I hope to have instilled in the reader some of the quality and feel of the work

as I know it having been there. Then, after developing in the reader an appreciation of a thing called salmon purse seining, I ask how can I, any other seiner, or the reader feel that there is such a thing. Furthermore, how can this complex amalgam cohere as a unity through time in our perceptions of the world. These questions presume the Boasian requirement of psychological unity in order to define and identify a culture complex. But, beyond this, I ask how can there be a psychological unity and how can it persist from day-to-day and year-to-year. Chapter 5 gives my thinking on this matter.

Thus, there is a thread of thought linking the two halves of this work. If one follows the implications of what constitutes a proper description of a given culture complex, then one must consider the issues raised in Chapter 4. In its conclusion, I chose to follow through some of the implications of taking an historicism as the basic explanatory paradigm, and this choice leads to Chapter 5. So, if the reader will reflect on how it is possible that you now think you know something about a thing called "salmon purse seining," you will also see the problem that binds together that particular example and the theoretical arguments which may seem so far removed from the ethnographic data. Hence, the title of this dissertation places the connective concept (memory) in between the example (fishing) and the real subject matter (cultural stability).

In the remaining few pages, I should like first to recapitulate some of the points I have made earlier and then discussed bigger issues. This is necessary because I have not followed through in developing an

historicism powerful enough to account for culture history. Rather, I have delved into the fundaments for a psychological historicism to the neglect of a truly sociological historicism. On the other hand, as pointed out in Section 4.5, to do otherwise would be to put the cart before the horse.

6.2 Salmon Purse Seining as a Culture Complex

Salmon purse seining is an excellent example of a culture complex. It is a "systemic pattern" in Kroeber's terminology, as opposed to a "stylistic pattern." It consists of a tightly knit collection of thoughts, feelings, and actions smaller in scale than a culture-whole yet exhibiting remarkable cohesion.

Even from an outsider's vantage point, purse seining for salmon has several special features that differentiate it from other more famous culture complexes such as the East African Cattle Complex, the kula ring, outrigger canoes, etc. (1) Purse seining is a seasonal activity. It is alive and happening for only about three months or so during the summer and fall, and then it ceases being actualized and lies dormant all winter and spring. (2) A large proportion of the people who partake of and in the complex are transients who travel hundreds of miles just to work for the three months of the seining season. Many seinermen are also engaged in other fisheries, but many are not and they work in other jobs except while seining. Also, it is a frequent case that college students seine as a summer job, returning to college in the fall. (3) The human fodder of the complex are not babies or young children, but post-adolescent young men and women. Hence, very frequently, the

enculturation period is rapid and experiential rather than a long-term familiarity and preparation. (4) Many of the people who purse seine do it for only a few years, then settle into other careers. As an occupation, salmon purse seining today is typically a young man's job and not a lifetime undertaking. This is not so true of skippers, nor is it true of all crew members, but it holds as a generality. The implication of this is that each year personnel move out of and into the culture complex and must leart it anew. (5) Even amongst those who go seining every year, there is a fairly high percentage who change boats. As a guess, something on the order of ten percent of the "experienced hands" change boats from one year to the next, not to mention the alterations in crew composition effected by "rookies." And (6), purse seining is a technological complex rather than a religious or ceremonial activity. In addition, it is a very complex culture complex in the sense that it involves the products of numerous ancillary industrial complexes, such as boat architecture, tool making, rope and twine making, diesel engine factories, and on and on.

These observations are obvious even from an outsider's point of view. From this perspective, one would also note that the technology of canning salmon is tightly related to the advances in purse seining and conversely, and that this in turn is related to national food preferences and demand, and that this is related to fluctuations in the supply of other food sources, and so on. Before long, such a perspective has everything related to everything else.

While not denying the functionalist dogma of universal interconnection, I have been interested in how it is that after going seining

I have come to feel that there is a "thing" which I call salmon purse seining. If everything is related to everything else, how can I feel that salmon purse seining is one of the "things," a cohesive entity? This question requires a subjective perspective on seining.

From such a personal, or subjective, point of view, purse seining hangs together as a culture complex because purse seiners "make sets." This central operation of putting a seine in the water, pursing up the bottom, and then hauling in the gear and fish is what "purse seining" is all about. Making a set is the clearly discernable core of the complex. It is an often repeated episode, a clearly demarcated temporal happening. Being able to build a seine, steer a boat, fillet salmon, etc. does not make one a salmon seiner. Rather, fishing (making sets) with a purse seine is what gives this entitlement. There is no substitute.

But, in addition to this core, this vortex of the culture complex, there are numerous associated thoughts, feelings, and actions which in combination with the core comprise the personally felt entity of salmon purse seining. This nebulous mixed bag of associated knowledge and behavior is peculiarly and variously related to the core. In many cases, the connection is apparently functional, for example, repairing holes in the seine ("sewing seine"). In many other case, the relation is functionally and structurally opaque, for example, drinking about a half gallon of coffee per day while fishing.[1] And in yet other cases,

[1] I drink only about two or three cups per day in the winter.

there is an association based on simple temporal co-occurrences, for example, I now think of seining and Alaska every time I see slate-colored skies and drizzly, misty rain. These peripheral constituents of the culture complex are apparent only from an insider's (emic) point of view. They are variable from one individual to another, but overlapping. They are one reason the complex seems unbounded and amorphous; they give it a fluidity of character.

Altogether, the fluidity of the complex is evidenced in three slightly different ways. First, as mentioned, the psychologically peripheral areas are variable across individuals and not specific to purse seining only. Second, on different times, or days, or months, different areas of the peripheries pertain more strikingly than others. That is, sometimes all the work associated with "hanging seine" stands out strongly in conjunction with "making sets," but other times, I fixate on weather patterns, colors, coffee and tobacco, etc. And, third, even in the core itself, if one looks at it in detail there are many variations. There are different styles of making sets, different crew organizations for doing the same style, different contingency actions from one set to another, and on and on. Every set is the same, but each one is unique. In all these ways, purse seining does not stand out in my mind as a simple monolithic chunkiness, a packaged constellation of crystalline parts. Quite the contrary, it involves muddied interminglings of specific events and regular recurrences with gradations of clarity and sharpness. Some aspects are cleanly distinct and discrete, but these are relatively rare. No wonder it is so difficult to define

a culture complex; its nature is not suited to clear demarcation.

These are the general features of salmon purse seining as a culture complex. Culture complexes are highly personal constructions, yet at the same time interpersonal to the extent that many people will have similar constructions. In the case of salmon seining, this interpersonal dimension is greatly enhanced by virtue of the fact that making sets (the core of the complex) involves clearly visible actions which can be simultaneously observed by numerous participants. Purse seining is put into action (as opposed to believing a religious concept), and that action is publicly perceivable. But, despite this public dimension, a culture complex is an individually constructed clumping of culture. This is clear if one does not attempt to define a complex, but rather explores one in concrete detail. Thus, culture complexes (and therefore culture) arise in and from individuals as individuals, not from the "superorganic." Inter-individual resemblances--the usual grounding for speaking about culture as a transcendental level of reality--do indeed exist, but they are resemblances and not replications of the same thing. Culture complexes are constructed within the individual, not imposed upon him.

6.3 Psychological Historicism and Cultural Stability

I am by no means the first to recognize the importance of the individual with respect to culture. The Boasians' concern with what they called the "psychological" aspect of a culture trait was similar to the man-in-culture perspective which I am advocating. Once one insists that the insider's point of view is crucial data as regards

the constitution of a culture complex, the doors are open for seeing the basic significance of the human being for culture. If one average[2] human being cannot learn a given culture complex and make it part of his life, then that complex will not persist. This is the incredibly simple yet important fact that many authors of a culturological stance ignore. This is one of the functions of the great constant underlying cultural variation--human nature--which culturologists like to take for granted and disregard. Being able to learn and remember, that is, maintain subjective continuity with oneself from moment-to-moment and day-to-day, is part of human nature. Herein is the mundane source of cultural stability, as Kroeber came to see.

From a detailed exploration of a given culture complex, one comes to appreciate the empirical nuances of "culture's" manifestation in the human being. And, in so doing, one also becomes aware of how much that human being is responsible for "culture." The complex, seen from the inside, has all sorts of irrationality which cause no balking nor bafflement to the human beings who existentially embody the complex. These nebulous associations are very difficult to account for with a functionalist or structuralist paradigm, yet they are part and parcel of the empirical realization of the complex in human beings.

Therefore, I concluded Chapter 4 with the claim that cultural stability should be understood through an historicism as opposed to structural-functional thinking. There is an a priori argument favoring

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[2]"Average" in the sense of physically healthy and typical of the people who already partake of and support the complex in question.

this conclusion as well as the matter of ability to cope with the empirical details. The deductive argument follows from the initial question of what happens to culture when it is not currently active in human behavior. The answer to this will usually arrive at "human memory" (though as we have seen elsewhere, this is not necessarily so). The empirical details which flounder structural-functional thinking are the irrational, nebulous, peripheral components of a culture complex, that is, its fluid character. In yet a different vein, historicism has supporting evidence in the form of "survivals," "selective borrowing," and numerous cases where one wants to impute something like "cultural inertia" or "the forces of history" to buttress crumbling structural-functional explanations. These, then, are the sorts of considerations which favor an historicism as the explanatory paradigm for cultural stability. They do not argue conclusively, that is, constitute a proof, but they supply a grounding for a decision.

Chapter 4 went this far. But, after saying that a psychological historicism is essential for comprehending cultural partibility (coherencies with stabilities), one should go ahead and present at least the rudiments of what one would be like. This was the task of Chapter 5.

Without simply recapitulating its arguments and evaluations amongst alternatives, the best summary of Chapter 5 is that there I presented two separate but intimately related conceptual schemes, one for thinking about active knowledge and the other for passive knowledge (or knowledge-in-memory). The significant feature which both the "cognitive

structures" view of knowledge and the "deformed jelly" image of memory share in contradistinction to most all other cognitive modelling is that both fundamentally incorporate the full temporality of knowledge. Both represent scientific modes of studying cognitive processes because they both place knowledge in the same space-time frame as behavior. They do not foster a dualistic philosophy in which behavior transpires in the material world while knowledge exists in a mental world. This is not to say that they are reductionist; neither tries to locate "ideas" in neurophysiological patterns. Rather, they are modes of modelling which preserve the basic properties of their data base (space and time referents), but do not attempt a hasty claim to biological reality. Nonetheless, they are more in line with biological models of nervous activity than the cognitive models whose fundamental notions are "categories," "concepts," or "ideas." Also, they show abstractly what I claimed was wrong with my Toy 1 and Toy 2 Series in Chapter 3, which were based upon ordinal-time representations using rigidly discrete action-events ("jobs").

The resultant product of Chapter 5 was a very general model or image of memory underlying both current functioning and passive preservation. In its terms, my principal thesis (that memory is the source of cultural stability) is restated as follows: the stability of a cognitive structure is equal to the inertia of the deformations obtaining amongst the neighbor-neighbor relations of inert micro-elements. This general image has, then, "dissolved" a content level (culture) by providing a means of describing that level's content in terms of formal

relations obtaining at a lower level. In this respect, it is a "substrate explanation" rather than a structural-functional "content-mapping explanation."

Thus, I have offered a way to conceptualize cultural stability by focusing upon how some portion of culture is lodged in the psychological make-up of an individual rather than appealing to its place in some system of causal interlinkages or structural isomorphisms. I have tried to account for the stability of culture by delving into human nature rather than deriving stability from the operation of some cultural system, that is, by focusing on people rather than superorganic systems.

6.4 Implications of the Thesis

Although this dissertation has stopped short of a comprehensive paradigm for understanding culture history, it does have implications for this larger undertaking. Instead of focusing my attention just upon the fact that people seem to exhibit similarities (they resemble one another in terms of their respective cognitive structures) and taking this important phenomenon as my basic problem, I have concentrated mainly upon how it is possible that any given person can exhibit relative stabilities with respect to their acquired behaviors and thoughts. After one has conceptualized this stability problem at the level of single individuals, then one has a basis for approaching the problem posed by the existence of cultural similarities. This two-phase conceptualization is what I have been referring to as the difference between a psychological historicism and a sociological historicism. And, I should like to sketch the relationship between these two levels in what follows.

It is the task of a psychological historicism to account for the development of cognitive structures and their persistence within the individual. For this, one assumes the existence of an external reality (a reality apart from the subjectivity being studied) and wonders how the subjectivity constructs knowledge and maintains its internal continuity. In this dissertation, the concept of long-term memory bears this burden, and the emphasis has been upon what underlies the persistence of cognitive structures rather than their processes of construction, although this too was discussed.

A sociological historicism, however, takes all of the conceptualizations of a psychological historicism as its grounding and focuses upon the fact that part of the "external reality" vis-à-vis any given subjectivity is composed of malleable subjectivities which also grow and form. The basic phenomena of a sociological historicism--cultural similarities or resemblances--form in the malleable, human component of the world, and hence, the plasticity, the susceptibility to deformation of human beings lies at the foundation of any sociological historicism.

Both levels of historicism are concerned with formations, constructions within human beings' constitutions. A psychological historicism is interested in the development and persistence of cognitive structures within an individual; cognitive structures are the formations of interest. A sociological historicism, however, is interested in how individuals construct similar cognitive structures; cultural similarities are the formations of interest. But, whereas some concept similar to Piaget's "equilibration" will likely be invoked to account for formations at the

level of psychological historicism, the concept of "social interaction" will be invoked for explaining sociological formations. The two fit together, however, because any given subjectivity's "interaction" with his external reality (whether it be typified as social, ecological, or spiritual) will involve the activation and/or construction of that subjectivity's cognitive structures. Hence, interaction at the social level stimulates equilibration at the psychological level.

While it is imprudent to claim that all cultural similarities arise from social interaction (human behavior and constitution are not completely free of human biology), social scientists are primarily interested in the trans-subjective cognitive similarities which can be attributed to result from social interaction. And, for this reason, social interaction is the most basic concept underlying any sociological historicism which would account for culture history through a mass effect logic.

It is conceptually possible, however, to envision a sociological historicism so powerful and embracing that the "individual" is reduced to a mere shadowy actor in the grand scheme of nature's unfolding. For example, Hegel's "absolute mind" or "God" can be worked into such potent conceptual schemes that they simply "are that they are" without being construed as mass effect concepts. This would be a different sort of sociological historicism from the sort I have been building toward throughout this dissertation. I have presumed that one should begin with the incorporated subjectivity and gradually lead up to a trans-subjective, mass effect mode of comprehending inter-individual

resemblances. Throughout, I have presumed that a psychological historicism is the first step in the eventual development of a sociological historicism. But, the fact that a sociological historicism does not necessarily require incorporated subjectivities as its theoretical cornerstone only highlights the crucial concepts for a sociological historicism which does adhere to this presumption. And, these concepts are: (1) long-term memory of individuals, (2) equilibration of a progressively constructed subjectivity with its external environment, and (3) social interaction among malleable human subjectivities.

These three concepts form together a package for comprehending culture history. Other concepts may be added, for example, one could tie in rate of equilibration with a natural selection paradigm, but these three seem to me the most basic for social theory. The long-term memory concept has been explored and imaged in Chapter 5. The equilibration concept has its roots in the ancients' "dialectic," or "balancing of opposed forces." But, social interaction, as a concept, has received less attention than it deserves.

First, it becomes apparent that social interaction will be used to explain the distribution of the activation of cognitive structures, both in space and in time. It cannot, by itself, account for the stability of any given cognitive structure (that is the work of memory), but social interaction will be used to understand why some people tend to resemble each other more than they do other people.

Second, and following from the first, social interaction is partially responsible for the becoming of cognitive structures. This will be true

insofar as cognitive structures form as precipitates of consciousness, which is, of course, the locus of subjective activity. And, to the extent that social interaction is one source of stimulation for any given subjective consciousness, social interaction will function to increase, modify, or initiate deformations in the long-term memory tissues of the participants.

And third, other than the conceptual role of social interaction in a theoretical paradigm, it is not very clear what social interaction is. This is one of the most important areas for future research in the social sciences. Condon's work on microkinesic organization of behavior both within one person and between interacting persons in most exciting in this respect. If his observations are solid, empirical findings and not statistical quirks, then we may well have to dispense with our particle mechanic notions of social interaction and develop new conceptualizations of its basic nature. For example, social interaction may sometimes result in super-individual organizations which should be modelled as some sort of "interactive bubble," that is, an interactive bubble would transcend the confines of the incorporated subjectivity. But, however one eventually comes to comprehend social interaction, it seems that the deformed jelly image of long-term memory tissue will be part of our total understanding, for this general image allows for the continuous accumulation of organization-in-time and the stability thereof. And, human culture consists of just such accumulations and constructions, that is, cultured behaviors and thoughts.

The deformed jelly image of long-term memory provides an avenue for simultaneously bridging two conceptual dichotomies plaguing Anthropology. By recognizing the fundamental temporality of human life and actions, the image casts in a new light the real differences between knowledge and behavior. This was discussed in detail in Chapter 5. And, in conjunction with a well-developed sense of the nature of social interaction, the image can serve as a solid basis for conceptualizing trans-subjective resemblances through a mass effect logic, or allow new sorts of truly social realities (e.g., interactive bubbles) as real possibilities. In this second case, the deformed jelly image helps to better understand the perhaps overdrawn distinction between Individual and Culture, for, in its terms, Culture is nothing more than the collectivity of cognitive structures which have been constructed within and by each human being.

In closing, I must reiterate that my thesis has not been proved. I have not shown conclusively that an historicism based upon the concept of long-term memory is the "right" way of comprehending cultural stability. It has been argued, but I have only decided in favor of one paradigm over others. I do not see anything amiss in this. At the level of thinking involved in the competing, or I should say alternative, modes of understanding cultural phenomena, I cannot imagine what a "proof" would look like.

In situations of conflict between or among paradigms, proofs do not make much sense. Rather, it is the duty and goal of scholars to explore and expand each mode of thinking to its respective limits, and,

if possible, this should be done in a slightly whimsical state of mind rather than polemically. I believe the correct attitude of science is wonderment at and a love for nature in all of its manifestations. We, as humans, can only probe gently into the miracles which abound around us. We exert our will on this exquisite muddle through certain psychologically powerful concepts. But, if comprehension and understanding are our aims rather than control, then a cultivated sense of wonder is indispensable. In other words, science is but a branch of natural philosophy. In these terms, I ask the reader to ponder my attempts to lay a ground for an historicism. I cannot prove that I am right while other ways are wrong, but what I have offered must be reasonable, productive of insight, and consistent, as well as related to empirical data.

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APPENDIX A

Repertoire of seining "problems" noted from the 1975 season:

(1) "purse line knotting up as the seine is playing out"*

effects: purse line will not go through the rings, thus it will drag them all off at the same time thus bunching up the seine in the water

solution: stop boat and skiff immediately, three deck hands pull on purse line until can untangle it (may even have to put boat in reverse)

possible secondary problem: seine might get hung up on propeller or rudder thus requiring varying degrees of remedial action

(2) "seine becoming unhooked"*

effects: seine will float away from boat and the fish will get out

solution: everything stops until seine is secured by the joint actions of as many of the crew as are necessary to pull it back again

(3) "forward breast line getting tangled with the rings and purse line"

effects: will cause problems in getting the rings up in an orderly way so that hauling seine can go well

solution: Sk'eg tells me and I help him by pulling the webbing up from an angle more toward the bow

(4) "Stern breast line being tangled"

effects: (same as #3)

solution: I pull up the breast line until it untangles from purse line, then have the skipper hold it so I can do my other work ("hold it" because it eventually has got to be pulled up anyway)

(5) "seine caught on rudder (or propeller)"

effects: will cause a complete shut-down until fixed, once crew

progresses to the hauling seine stage of the set

solution: I get the plunger and try to unhook the webbing by pushing it down under the rudder (someone holds my feet as I lean over the stern), this goes in rotation as each crew member tires out or seine gets free; should this procedure fail, Frank brings the skiff over and works on it from the skiff (which makes for a better angle of leverage); failing both of these, someone has to jump into the water and cut it free

(6) "log in set"

effects: will rip holes in the webbing if not removed; generally hinders hauling seine

solution: using the pike pole, one of us pushes the log over the cork line as someone else pushes the cork line down with the plunger

(7) "shark in set"*

effects: possibly it might eat some of our money, but the main problem is the crew's adversity to having a flopping eight foot shark at our feet while we're working

solution: continue hauling seine until the shark gets dumped on deck, then Frank gets his .22 caliber magnum pistol and shoots it in the head several times, whereupon we get the doubleblock and strap the shark by its tail, hoist it up, swing it over the gunwale of the side opposite the set, and throw it into the water by cutting off its tail

(8) "seine snagged on bottom"

effects: could easily rip out a huge hole in the webbing, thus making fishing rather ridiculus

solution: skipper and Frank maneuver the boat and skiff around until they can get the seine free

(9) "skiff taut line getting unfastened"

effects: Frank can no longer use the skiff to keep the boat out of the seine

solution: Frank takes care of it

(10) "debris or gilled fish in webbing"

effects: if fish are gilled in net, then we would lose money for them and the stench could become tedious; if debris is in the net, it could rip holes the next set or cause the net to tangle

solution: Darrell is supposed to see it and take care of the situation without having to stop us from hauling in the seine, but we are also responsible for getting stuff he cannot reach

(11) "purse line knotted up after we've begun pursing"*

effects: will not be able to purse up because the purse line won't be able to get through the davit pulleys; also, it causes a weakness in the purse line (which is a rather expensive rope)

solution: (took some time to figure out how to handle an actual case of this problem--which occurred only once) using the double block, strap the purse line on the set side of the knot, hoisting on the block takes the tension off of the area of the purse line where the knot is, thus the knot can be untied and thereby enable us to resume pursing

(12) "chain on power block breaking"**

effects: cannot do the normal way of hauling seine

solutions: try like hell to fix the chain (case of this problem took three hours in the rain to fix in this manner, and we had to borrow a link from a nearby boat); if chain cannot be repaired, then we worked the seine in by small sections using the brailing line as a pulley (skipper told us that this was the old way of handling bizarre problems back in the days before they had power blocks, back when they used a big roller on the stern)

(13) "skiff engine overheating"**

effects: this could result in returning to port if it keeps happening every set because the skiff costs around $10,000

solution: 'jerry-rig' the problem or while in port get a mechanic to work on it

(14) "corks too far around the bow"

effects: this happens sometimes when pursing up (I think it happened during certain specifiable tidal conditions) and could result in ripping holes in the seine once we start hauling

solution: it fell upon me to watch for this condition and take care of it by walking along the three inch board on the outside of the gunwale and pushing the corks back around the bow with the plunger

(15) "Sk'eg screwing up while hauling seine: failing to get a ring on the ring bar"**

effects: makes it impossible to thread the purse line through the rings in preparation for the next set, which in turn would either leave a gaping hole for the fish to get out through (if purse line did not go through the particular ring) or running the risk of getting the seine all fouled up (if the purse line were strung through the

ring, but not in the proper order of rings)

solution: make fun of Sk'eg; reverse haul the seine up to the point where Sk'eg messed up, then begin hauling again

There are probably many more "problems" that could be listed, but these cover the spectrum of things that tend to slow down and generally foul up an ideal set. In any given set, one more or less expects that something will go slightly wrong, but the idea is to understand the consequences of each well enough so that a person or persons can straighten it out before it brings the whole crew to a standstill.

"Problems" marked with an asterisk are rare happenings; those with two asterisks are not supposed to happen at all.

Repertoire of seining "timing problems" noted from the 1975 season:

(1) "Sk'eg catching the skiff cable"

This involved Sk'eg learning to catch a steel cable tied to the bow of the skiff as it would come flying up to him right after we cut loose at the beginning of each set. There was a lot of tension on the cable, and the release of the skiff released the cable. It was not essential that he catch it--he could lean over the bow of the skiff and retrieve it as the skiff began pulling away from the boat--but it was flashy, had style, and he thought, along with the rest of us, that it was proper.

(2) "a division of labor became necessary in the deck clean up work during the fall season"

This ultimately goes back to the prevailent beliefs that humpies just keep swimming into the net whereas dogs dive and try to get under the

net. The humpies are the summer fish and the dogs are running in the fall. During the summer, therefore, we hold our set open much longer to get more fish, but in the fall, we sometimes even made "circle sets." Thus, the work which must be done during the plunging phase of a set is done much more leisurely during the summer than in the fall. The division of labor evolved naturally, i.e., with very little verbalization. (Similar time-compacting of the work a division of labor occurred whenever we made a "reverse set.")

(3) "learning when to stop plunging"

This was a case of learning when to stop an episodically continuous task. The cue is of the 'build-up' type: the skiff is moving closer and closer back to the boat, and I had to decide when to stop plunging at the stern of the boat and stow the plunger. First, it was interesting that I was always the one who was plunging at this point in time. We all took turns at it up to this point, whereupon I would take over. Also involved in this task was learning to put the plunger away on deck instead of back on top of the cabin. This came about as I became aware that the skipper was just having to take it down again immediately.

(4) "learning to put guide line over power block in stern area instead of leaving it tied to the rigging"

This innovation I can claim credit for. It saves someone from having to go into the rigging area during a much more hectic time just before beginning to haul seine (this area is congested at that time). So, I figured out that it would be out of the way if I went ahead and made

untying it part of the plunging phase work of "getting ready." Thus, it represents a form of making the work/time density and people/area density more equitably distributed, i.e., time-compacting during a leisurely work time in order to avoid excessive rush later on. Obviously, thinking of this little time-saving item required a knowledge of what happens during a set. Once suggested by me, however, Darrell and Richard immediately saw the advantage, but by itself it is no big deal. Cumulatively, however, such little innovations do make the work easier.

(5) "catching the double block from Richard"

This was one of the more pleasurable little feats of each set. It occurred just after I walked the Canadian release on the end of the seine off the deck at the first part of each set. No sooner would I have tossed the end of the seine overboard off the stern than I would turn around with my hand outstretched and catch the block coming through the air from the cabin toward me. It was very much like completing a pass in football: the quarterback throws the ball before the end turns around, yet they each know the temporal-spatial coordinates of the pattern well enough that the ball is there when the end turns and looks for it. Well, catching the block from Richard was not nearly so complicated as a pass in football, but it did feel spiffy and gave me a pleasant sense of social coordination. Functionally, this made it easier to get the block tied around the boat tow line and hoist it up before much tension got on that line. However, I think, once again, that the main "function" of this little bit of

togetherness in the work was 'style'.

(6) "learning to coordinate galleyway passage with Sk'eg"

This bit of timing was not essential, but it was more efficient. If it failed I could always just verbalize to Sk'eg that I was coming through or tap him on the back. The situation consisted of the problem posed by me having to get through a very congested area while Sk'eg was working in that area on a rather important 'continuous' job. Smoothness in the passage also had the side-effect of making my back feel better than it would otherwise (because I had to duck under (about two feet off the deck) the purse lines and then get past Sk'eg into the galleyway), and the smoother this dance step could be done on my part, the less my back had to strain. It got so that I could really take off and as I came up from under the purse lines. Sk'eg would just take a step toward the gunwale without even looking at me or hearing me or feeling my hand on his back. After he finally got the idea, however, it was by no means certain that it would work every time, but when it did, I got the same sort of pleasure as when I would catch the double block from Richard--a spiffy thrill of social coordination--only in this case I was the moving 'part'.

(7) "knowing when skipper would be coming down the ladder"

This is very similar to what Sk'eg was doing in #6 above, only I was the one who had to know when someone else would be moving along a fixed path. This path and the timing along it was brought home to me and into my attention one time when skipper stepped on my head right after I had ducked under the purse lines as was beginning to stand up. I thought my neck was somewhere between my shoulders. For

the next few times after that particular set, I was wary of the skipper and watched his behavior at that time-period. After a while, I could fairly safely ignore him, in the sense of not paying attention to his whereabouts, because I knew just where he would be at what time. (Incidentally, this whole business was a problem only if we were making a set on starboard because that is the side of the cabin the ladder is on.)

(8) "having the skipper stand on the stern breast line if I had pulled it"

Ideally, I should never have had to pull the stern breast line, but it kept getting tangled with the purse line and rings, so I did it almost every set. (Eventually, the skipper figured out that the tangling was because our purse line was about six to ten fathoms too long.) At any rate, what happened was that I would have to pull it up in order to untangle the purse line and webbing, but I was pulling it up before it should have been pulled in the normal order of work, thus we had the option of pulling it twice, once to untangle and once to get the end of the seine ready to go over the power block, or of pulling it once but then having to "hold it." Fortunately, I noticed that the skipper was standing next to where the breast line ends up and was not doing anything much during the time-period when it would have to be "held." So, I would pull it up to untangle it, then ask skipper to stand on it (thus he could still use his hands and arms). All together, this was a satisfactory arrangement. It involved knowing that the breast line had to be pulled up eventually and that

I was pulling it at the wrong time. The cleverness came in finding a way to "hold" or freeze a situation until its proper time came along, and I found this in skipper's positioning.

(9) "knowing the place on the cork line where it would be time to throw the corks back off the stern"

This is just one example of a rather common mode of keeping time: a rough transformation of time onto length. It was amazingly accurate, however. All one did was keep pulling on a line at a rather fixed rate and pretty soon it would become apparent that you could forget about watching other people and just pull until you came to a certain spot on the rope. Generally, if everything was going right with everybody working on related time-meshed jobs, then this area on the rope would be an accurate guide to stop or change jobs. Once figured out, this sort of time-keeping and pacing gave you a lot more free time mentally. You could just be semi-aware of other people's progress.

(10) "knowing how much rain gear to have on initially and when to add how much"

This knowledge captures in the most significant way the extent to which a person knows his whole routine. There are two basic rules that come into play as parameters: (1) it is miserable inside one's rain gear, therefore, try to avoid having it on until the last moment (when we haul seine--to keep the jelly fish off); and (2) never make other people stop and wait on you, especially if it is just a matter of personal business. In order to comply with both rules, a person has to know exactly when his measures of rest are during his routine

and just how long it will take to put on how much rain gear. There is an auxiliary rule which occasionally comes into play: if something needs to be done, do it; don't just say it's not your job. This additional condition further restricts when one can take the time to don rain gear to just those times when he himself is not working and when he could not really be of much help to others who are working at that moment. All in all, what happens is that each crew member (who has different sorts of rain gear anyway) begins each set in slightly different stages of complete jellyfish protection, and during the set, each crew member progresses toward full dress at different rates. I was a two-stage dresser. Sk'eg was complete at the outset. Darrell was a two- or three-stager. Richard was a three-stager.

Again, this list could be extended indefinitely, but these ten items are representative. The sort of timing sense that is required of a good crew is one of the most enjoyable aspects of the work, provided things click. Poor timing costs in efficiency (total time to make a set therefore fewer sets made per day therefore fewer fish, all other things being equal) and hence in crew pride. It is one of the main reasons I've returned for a second year; I was hoping to experience that level of social coordination again. Becoming aware of the timing is difficult to trace because one operates in a kind of supersensitized condition, especially when a novice. Figuring out what goes on in making a set helps a lot. Crew members are supposed to think about ways of improving small-scale timing, but large-scale timing is in the hands of the skipper (e.g., one style of seining

versus another). In determining "rush times" from "slack time," a good index is the amount of talking: talking increases in slack times and decreases when busy (one must distinguish talking from jubilant verbalizations expressing enthusiasm). Also, in rush times, one doesn't generally have much consciousness of where or what other people are doing, at the time one is working, that is.

Repertoire of remembered instances of learning work routines from the 1975 season:

(1) During the week when we were "hanging seine" at the old Sunny Point cannery, prior to the opening of the seining season, we (the crew) speculated as to what the skipper would end up having each of us do. By that time, Darrell knew he was going to be "deck boss" and Sk'eg had a pretty good idea that he was going to be "jump man," but other than that, we hadn't heard. The day we finally heard our jobs assignments was the day we went over to pick up the seine at Sunny Point (which was during the first week of August). At the time, Skipper told us that Sk'eg would do leads, Richard web, and I would do corks. In the process of getting the seine aboard, I had to help it over the top of the dock, so I did not get to practice piling cork, but Sk'eg and Richard and Frank did their jobs, with Skipper helping Sk'eg with instructions and tips. I was supposed to watch Frank do the corks, but I couldn't because I was also supposed to keep the seine from catching and ripping up on the dock. This was the first actual exposure of us as a crew to the 'hauling seine' aspect of our jobs, but I did not get to participate. I was

initially disappointed that I was not jump man and piling the leads because I thought that was what my brother had done. But, the more I heard about the lead man getting the worst jelly, the less I cared about not doing it. Skipper had told me, in passing, that Sk'eg seemed lighter on his feet than I did and this was why he was to be jump man. This hurt my pride somewhat, but then, I was recognized as perhaps the best of the four young crew at tying knots, splicing, etc.

(2) The night before the August 8th opening was the first time that I heard about all of the different sorts of jobs that are involved in making a set and it was also at that time that the Skipper made his basic division of labor in the crew. I shall try to paraphrase his little speech that night:

> Well, let's see. Darrell, you and Richard will do the pursing up this year. You two (Sk'eg and I) do the other things. Suzie, you're the skiff man with Frank. Richard, you'll do the web again; John, you do corks; Suzie (Sk'eg) the leads; and Darrell you do what you did last year. (total time: 30-60 seconds)

In response to Sk'eg's and my questions as to what "other things" might consist of, Skipper would only say, "You'll see tomorrow." Skipper and Frank thereupon went about telling Darrell and Richard certain points of how they wanted them to handle "set-up" and "pursing" while Sk'eg and I watched. Most of that night was spent showing the four of us how to work figure-8 clasps, how to tie

certain specific knots (exceedingly standardized knots!), where to put the buckets of purse line, and similar "little things." Even now, I often wonder if Skipper's instructions, vague and general as they were, were sufficient to cause the resulting division of labor which evolved in great detail. It is quite possible that his initially sounding super-general job allocations are in fact specific enough to rather sharply determine what eventually results. Anyway, I found out from Darrell and Richard something of what to expect the next morning, at least up to the point where the skiff brings its end of the seine back to the boat.

(3) August 8, 1975, 6:20 AM, we "cut loose" and began our first set. I was really excited and hyped-up. Fortunately, we were not the first boat to cut loose, so I had been watching the Louann's crew at work (I knew their cork man). I had observed that as soon as the seine was off the deck and the tow line hoisted up that B. J. (the fellow I knew) began "plunging" off the stern, making the water "pop" with each stroke. Being eager to impress the Skipper that I was a "good worker" (and the rest of the crew as well), I immediately grabbed the plunger and began plunging savagely. This was after tying the double block's chain around the boat tow line. I felt this not only made me look eager to work, but also would hide my ignorance of what else was supposed to get done until I had the chance to see. It was a very good tactic, if I do say so myself. After a few minutes, Richard came over and gave me a "seminar" on proper form in plunging,

the basic principle being to expend as little energy as possible yet keep the 'popping' sound--Skipper liked to hear the 'pop'. At this time, I figured out that plunging was a relaxed sociable time, each deck hand taking turns on this job, which is usually regarded as the 'bane of deck hands' and is done mainly on older Skippers' or Indian skippers' boats. I also got to see what other sorts of jobs were done during this time. Also, I was fully garbed in my rain gear. As the skiff began to head toward us, I got rather worried about whether I was going to be able to do the little jobs I knew I was going to have to do. It is one thing to be told what to do--"put the purse line through the stern pulley on the davit and shut the clasp as soon as the purse line is untied from the niggerheads"--and quite another to actually have to do it, especially when it is supposed to be done quickly. I had a feeling very much like one has in high school just before a football game or the opening of the Junior Play. Much to my satisfaction and relief, I did do that first little job okay. From then on, however, up until it was time to begin hauling seine, I was only reacting to the Skipper's rather sparsely worded verbalizations. Sk'eg and I were running around in circles, into each other, and tripping over everything as we rushed to respond to such lucid instructions as, "Pull that line (vague pointing action)!!!!". I remember we pulled up half of the forward breast line together, then upon a yell from Skipper to pull something else, we both dropped the breast line and ran in unison to the general area of the stern, whereupon the Skipper got mad and yelled at

us to pull "the line" (breast line) again. Needless to say, we were in confusion. Somehow, we got through what I now call Segment 2. Skipper did not care who did what (between Sk'eg and me), rather he regarded us indiscriminately as individuals; he just wanted certain jobs done by whichever of us was in the vicinity at the time. Finally, we got to the point when it was time to do the only job I was adequately briefed on: hauling seine. I was burning up inside my rain gear and sweating like a fish as we began hauling. By the time my right shoulder was completely exhausted (the set was to port), I thought we must surely be near finished. Wrong. We were maybe halfway. At that point, I decided that if I could haul hay in Illinois in August, or work in the polluted 90°F air of East St. Louis for four summers digging trenches, that I could live through this, but surely there was something to do to make the work easier. By the end of that first set, I had established the following aspects of my routine: walking the Canadian release off the stern, tying the double block to the tow line, being the first and the last to plunge, putting the purse line through the stern pulley on the davit, piling corks, and pitching fish. Between the davit pulley and piling corks, everything was a maze of confusion. I also decided that anthropological observation of this learning stage was going to be quite beyond my abilities; I was going to need every bit of attention, smarts, and energy that I could muster just to learn to be a fisherman. I had no time to watch myself learn. I was rather confident, however, that once I unloaded the mental burden of trying to "do" Anthropology at the same

time, I would be able to handle this job. This, I suppose, came from my basic arrogance which tells me that if anyone can do something, then I can too. So, I rested and cooled off and waited for the next set, grateful that it would be a while.

(4) Sometime during August 8th, I began to get a general grasp of two jobs that had to been done at more or less the same time: pulling up the forward breast line and tying it to the rigging, and getting the cork line tied to the bow cleat. Since Sk'eg was standing right by the rigging (where the breast line is), he would always be pulling away on the breast line by the time I finished putting the purse line through the davit pulley. Without saying anything to each other or to anyone else, it just became the pattern that I would go on up the galleyway and begin hand-walking the cork line toward the bow while Sk'eg continued to pull the breast line. My job, here, was especially difficult because Skipper had forgotten to get a piece of rope to tie onto the cork line, so I would be leaning over the gunwale with my hands below my feet pulling like mad on the cork line (which by this time would have considerable tension on it). This is where I hurt my back but good. (By the second opening, we had obtained a piece of rope which made this job much, much easier.) When finished tying the corks to the bow, I would come back down the same galleyway and help Sk'eg with the breast line, then we would both to to the stern and begin pulling corks on the stern. We did this together in a rather comical way so that Darrell and Richard laughed and told us we looked like we were doing the 'last tango'. Eventually, Sk'eg

would stop and pull up the stern breast line.

(5) On the way back to port on August 9, 1976, we heard rumors that the Skipper thought we were doing okay and would make a good crew. (He had taken us up north to fish around Deer Island instead of over to Cholomondeley because he wanted us to have our first experience in calm water.) All the way back I was thinking to myself about the sequencing of jobs in my incipient routine. It also occurred to me that there was a major difference between the "pursing" and "hauling" phases. At that time, I was very impressed with the rhythm evidenced in hauling seine. I was also beginning to formulate what has since become one of my interests in this dissertation: how to describe work organization. At any rate, I would mentally run through the whole operation of making a set (my part in that operation), because I had noticed that often I would forget what I was supposed to do next. I would just come to a stop in my head in the middle of work and literally question myself: what am I supposed to do now? The whole "routine" at this point consisted of certain major, long-term jobs, e.g., plunging, hauling seine, and numerous "little jobs" each of which was a discrete event. After two days of fishing, I had amalgamated some "little jobs" into constellations, e.g., from the time I put the plunger down to shutting the davit pulley's clasp, but at the end of these small constellations, I would come to a full stop and wonder what was next. By mentally rehearsing my job, I came to see the sequence as a sequence, for not only is it important to get all the jobs done, they must be done in the right order. At

first, the ordering of jobs was inconsequential as I struggled to just memorize what the jobs were, but by the time we were headed home after the first opening, I was intent on ordering. It was either in these mental rehearsals after the first openning or during the second opening that I hit upon a clever plan to keep track of where I was in the set by where I was on the boat. The breakthrough came when I figured out that I could get from the bow cleat to the stern area by going around the galleyway on the opposite side of the boat from the set, thus avoiding having to get around Sk'eg and his breast line a second time. This discovery made me go in a circle around the cabin, and I did not retrace my route. By a clever mental flash, I noticed that I could use this "route" as a mnemonic for my "routine." Rather suddenly, my job became ordered and organized, also my full circle around the boat made other folks amused. That circular route was my biggest innovation of the year. It is hard to believe the consequences of that simple modification. It kept me out of two congested areas, let me be sociable to Frank and Annabelle, and generally speeded up my Segment 2. At any rate, the mental rehearsals and the spatialization of my work gave me a surge of organization temporally. Noticing this effect, I suggested practicing with Sk'eg, which we subsequently did to our mutual benefit. I don't think Sk'eg would have thought of this. The basis for this comment is that he seemed a little amazed that I should think of such a thing as 'mental practice'.

(6) Just before or during the second opening (August 14-15th), I suggested to Sk'eg that he pull the forward breast line as a regular thing and

not to expect help from me, unless required by whatever the reason (I did not know about "tangling of the breast line and purse line" at that time), and I would pull the corks on the stern. It was offered up as a simple contractual arrangement for trial. He said okay, and we decided to give it a try. We were both motivated to try ways to improve our confused situation and thereby keep Skipper from yelling at us. Both of us wanted Skipper to stop yelling at us not only for the reasons no one wants to be yelled at, but also because we wanted the Skipper's friendship, esteem, etc. This occasion is just about the only division of labor emanating from a crew member that I can recall. It is the only one I can remember in which a crew member verbally suggested a "you do this, and I'll do that" sort of arrangement. Divisions of labor emerged all the time during the first two or three openings, but they all happened non-verbally (the exception being the Skipper's initial crew organization). At the time and afterwards, this and things like it were regarded as "refinements."

(7) I don't really know when it happened or how, but by the fourth opening for sure (August 24-26th), I regarded my routine as completely mastered. I did not know what the other people were doing when, except in a very general way such as location, which I always knew, and vaguely the ultimate end of their work, such as "pursing" or "assisting with ending pursing." My routine flowed along smoothly, gliding from phase to phase without the question of what next ever arising. At this point, I began to take notice of why I was having

to do some jobs sometimes and not other times, (e.g., pulling the stern breast line), and I aided in giving verbal cues to other crew members (e.g., looking for the figure-8's to come up during pursing (they signal that the rings are just about up, hence just about ready to stop pursing). I also knew when I could look around at other boats, the crew, the mountains, etc. and not foul things up by such lapses from direct and full attention. I had my rain gear pattern developed (I knew when my rest measures were after the first opening). By now, everything but piling corks was a breeze, and I really enjoyed everything up to hauling seine. I did my "dance" around the boat each set and generally had a good time. I began to notice more and more what other people were doing, but the motivation for this was anthropological not fishing oriented. Whatever the source of my observational interests, it is appropriate for members of the crew to "pay attention" to the whole operation as best they can. You don't exactly get gold stars for doing it, but the Skipper seems to know if you're doing it and thinks better of you for it. With respect to piling corks, my major breakthroughs came in three stages: (a) when Frank finally told me (after the second opening) about getting my own slack by a trick of not grabbing the cork line itself but a couple of feet of webbing then pulling this over to the rope itself; (b) when I suddenly became able to talk, whistle, or sing (they all occurred in the same set!) while piling corks--this obviously involved huge amounts of internal organization, and I think it happened in the second or third opening; and (c) when Frank

told me that two men who have done web and corks together for a long time get entrained to each other's rhythms (this made me start trying to pay some attention to Richard's gross body motions while hauling). By the end of the fourth opening, I felt that I was pretty competent at my job. We then had a two week break before the fifth opening, the first of the fall fishing season.

(8) Between the summer and fall seasons (the fourth and the fifth openings), Skipper decided to change one aspect of our style of seining. Instead of taking the purse line out of the rings just before hauling seine, we were to leave it in. The advantages of this is faster set-up for the next set and perhaps a bit faster set. The only disadvantages that I can see are it takes a bit more deck space for the lead pile and perhaps the purse line gets a bit more twisted up and unevenly spaced through the rings. The bulk of the change in style lay upon Sk'eg's job as lead man. He had to adjust to the change in bulk relative to his space on the stern deck. Sk'eg adjusted completely by the end of the opening, for sure, but I think it only took him about five sets to get used to the change. Before the opening began, the four of us younger crew members speculated on how the new system was supposed to work, and we couldn't figure it out. The first set of the fifth opening was interesting. Everything went along with the usual speed and grace right up to the point where we had been unable to predict how we would proceed, i.e., everything went as usual until we had the rings on deck and were ready to haul seine. Then everything came to a grating halt. The Skipper couldn't remember

how to handle the purse line so as to make it go over the power block in proper spacing with the rings. He stood there gazing at the rings and trying different sorts of ideas out in his head, occasionally yelling at us to try something (as if we were mind readers), and finally, after about twenty minutes, he gave up and signalled Frank to bring the skiff on over and have a look. Frank arrived, tied up the skiff to the gunwale, got on board, examined the situation, undid one of the figure-8 clasps, pulled the purse line through up to a certain point, and did something else that I've forgotten and that was it. Skipper thereupon told us sternly to remember how to do it so we wouldn't have such a long delay next set! This freaked us out because we hadn't seen what Frank was up to--he kept making small mistakes and then changing, so we were totally unsure what he had actually done. Anyway, the whole deal was fascinating because the Skipper had to admit in front of all of us that he did not know how to perform his own suggested change in our routines; all of this was communicated when he had to call in Frank. Frank, on the other hand, had to get the job done without offending the Skipper's pride or acting big before us. Fairly often, Frank would know something that Skipper did not, but this was the only time that he had to go ahead and demonstrate an area of superior knowledge. Once before, Frank and I knew that Skipper was wrong on a certain small point, and both of us told Skipper that what he wanted us to do was going to be backwards. Frank said this once. I tried twice. We did it Skipper's way, and sure enough, it was backwards; whereupon, the

Skipper told us to do it the other way (our way). After the small incident, I expressed my puzzlement at the Skipper's actions, but Frank just told me that he had learned years ago that the Skipper was skipper, we worked for him, and we should do what he says even if we know he's wrong. I mention all of this here because it is about the only case, concrete bounded case, where any sort of formal role relationship was manifested. At any rate, after Frank came to our rescue, we finished the set. Between the first and second sets, however, we pumped Frank for information on how to do the job. This was the most striking case of learning through verbal instruction from the whole summer. To be fair, the second set of that opening, Skipper stood by to tell us how to do it; evidentally, all he needed was a little jog for his memory, which Frank supplied the first set.

(9) Another whole class of cases in which routines became established and the nature of seining became more conscious in me was every time we had some foul up and had to stop. Different sorts of foul ups stop different numbers of people and vary in whether they stop whomever they do stop immediately or eventually. By observing what sorts of problems resulted in complete and immediate full stops, I came to get a pretty good idea of which jobs were most important. On the other hand, when something would go wrong during pursing and no one except me was affected, I got an idea of the relatedness of the various jobs. Much of this sort of knowledge is reflected, not directly, in Toy 2. I learned when my job was just time-coordinate with other jobs as opposed to functionally or sequentially related. This sort of learning went on all the time.

(10) Another line of coming to understand better the operation of seining came from simply tracing tension supports in the artifactual assemblage of our boat, skiff, seine, and rigging. Here the point was just to watch how the crew worked within the machinery of the boat and used levers, pulleys, mechanical power, etc. This could also be observed on other boats near us.

(11) My general grasp of what jobs got done on the Glenda Lou was tremendously improved when I interviewed Richard, Sk'eg, and Darrell. They seemed to suffer from the same sort of specific ignorances as I did. Everybody agreed that it is rather unnecessary to the operation whether people know what other people are doing, so long as each person does his thing. Only in the case of problems does such overall knowledge come in handy. Darrell told me, however, that he thought he had been made deck boss partly because last year he had been rather observant and gotten the "big picture" in hand.

APPENDIX B

Introduction

There are two reasons for attaching this appendix to the body of the dissertation. On the one hand, it offers a more probing and accurate characterization of "making a set" than was achieved in the Toy 1 and Toy 2 Series. On the other hand, portions of this appendix, especially the sample of the Toy 3 Series, illustrate the general frame of mind advocated in Chapter 5. These two concerns intermix as I discuss first the ways we crew members segmented our job routines and second the nature of my 1975 routine's segments or phases. The basic impulse underlying this appendix is ethnographic.

Segmentation of a Set

From the descriptions of purse seining in Chapters 2 and 3, one can see that each seiner does many jobs in the process of "making a set." Some of the nature of these jobs is revealed in the Toy 1 and Toy 2 Series. What I want to do here is to describe how a rookie feels about his jobs, what sort of organizations he constructs to think about his own work, and how these change as he gets better at his work.

When first exposed to purse seining, it is rather difficult to see the big picture, the larger pattern which all the various little things people are doing carry along. This is true from the perspective of a spectator, one who just stands on the cabin and watches a crew working. And, it is painfully true for a rookie who is trying to find enough work to do to keep his skipper and the experienced hands off his back. The

initial conceptualization of the work is quite different from what eventually develops as the work becomes more familiar.

At first, rookies tend to see their work as a single-levelled array of separate tasks. Each little job is seen as a separate item which must be mastered in and of itself. Frequently, rookies in this stage of development are oblivious to what the other crew members are doing, and they are completely absorbed in their own thoughts and anxieties. They have to be watched to make sure they do not forget something they are supposed to do, do something at the wrong time, or get in the way of other crew members. And, provided the rookie has the beginnings of a good seining ethos, his inner turmoil and pressure can be seen on his confused but eager face. Speaking from my own experience, I remember feeling like I was ready to jump in any direction at the slightest suggestion or instruction from one who knew what was going on. My worries were whether I could remember everything I had somehow gotten done the previous set, whether I remembered when to do what, and whether the jobs I had done the previous set were regular occurrences or just peculiar to the situations of that set. I knew I would have help in remembering: if I did something wrong, I was sure to hear about it.

Before too long, roughly on the order of eight to sixteen sets, the rookie who is going to make it as a seiner manages to get his work responsibilities under control. He can tie lines around cleats and rigging pins quickly. He knows where the busy and congested spaces are. He knows what jellyfish on the face feels like. And, he knows what he is responsible for doing and when he should do it. Typically, this "understanding" involves a simple memorization after the fashion "first I do

job_1, then I do job_2, then I do job_3,...," that is, this level of understanding is in the form of a "string of beads."

The string of beads organization, though quite simple, is itself rather miraculous because job responsibilities are evolving during the same time range. If I had been provided with a detailed list of "my" jobs, I am quite confident I could have memorized it thoroughly before making my first set. But, no such list is provided. Furthermore, there are no rehearsals or practices in advance of the real event. It is true that skippers with several rookies on their crew will often select a location relatively free of winds and waves so that their neophytes can have the chance to familiarize themselves with the basic process in good conditions, but no one gets a chance to practice. Each set takes valuable time--time granted to the seiners by the Alaska Fish and Game people--and a novice skipper with a whole crew of rookies could make a big catch.

Despite the mass of confusion (from the rookie's vantage point), a normal human being seems capable of coping with the flux and transforming it into routines. And, this usually happens, or at least inroads are made in this direction, after as few as six or eight sets. Also, the rookie must learn how to do each of his jobs. Just being able to recite the names of one's work responsibilities in their proper ordering will not get the fish into the hold. One has to do the work.

The string of beads organization is what is reflected in Toy 1 Series. It persists as one mode of thinking about one's work even after more intricate modes have been constructed. However, if a rookie

with only a couple of sets experience were asked the Toy 1 Series question, he would most likely be unable to answer with any certainty what his jobs are. Sk'eg and I had to verbally rehearse our routines a few times before we finally got them straight, and I have observed similar periods of uncertainty in several other rookies.

It seems common that rookies think of the string of beads method of organizing their work sometime during the afternoon of their first opening (i.e., after three or four sets), and they continue thinking in this mode alone until they are confident of their accuracy. After twenty to thirty sets, even a rookie can very easily and naturally recite his job routine (in addition to doing it fluidly). But, by this time, two things have also happened: (1) he has developed additional ways of thinking about his work, and (2) he is not likely to think of himself as a "rookie" any longer.

During this whole period of rapid learning, I had the feeling that there were two levels of understanding slowly coming together. On the one hand, the jargon a rookie has heard since his arrival (e.g., "making a set," "hauling gear," "pursing," etc.) is beginning to be more than mere words. Even before stepping foot on a seine boat, they have served to loosely organize the work, but now, they have deeper meanings. On the other hand, little things, unnamed chunks of activity have riveted the rookie's attention away from the big picture and surprised, confused, and frustrated him until he has finally mastered each one of them. The large-scale overview of seining provided by the jargon does not mean very much to the beginner, and the small-scale fragments of the process

dominate one's thinking but provide no comprehension despite their vividness. Both visions have merit, but they initially remain two distinct and seemingly unrelated perspectives, in spite of the fact that both are supposedly concerned with the same process.

Slowly, the two organizations mesh and interpenetrate. When this has been achieved, the rookie is not only able to do his work, he imagines that he has begun to comprehend seining. Rather than simply reciting his memorized list of little activities, the partially experienced seiner can relate his work to the operation as a whole. This is also reflected in his increased ability to decipher another rookie's work on a different boat and in his increased awareness of what his fellow crew members are doing at any given time in a set.

In varying formulations, the high-level jargon accrues meaning, meaning beyond that appropriate simply for correct linguistic usage. For example, "hauling gear" ceases being just another perhaps colorful addition to one's vocabulary and, instead, has all sorts of connotations. These connotations resonate through muscles that flex and contract in new ways, and one conjures a mood of strenuous inner tension when reflecting about "hauling gear." These complex connotative flows become part of the meaning of the words "hauling gear." And, when this kind of feeling, this kind of inner flowing has become patterned and regular through repeated activation, the seiner thinks of "hauling gear" in a more than linguistic way. Now, it is a temporal segment, a "natural" phase of seining.

With one significant exception, every seiner I have known has

developed a segmentation of the process of making a set.[1] But, these segmentations are not simply replications of the common jargon. They are not merely words with expanded meanings, common to all seiners. Rather, each seiner constructs his own version of the "natural" phases of a set. Figure 14 illustrates the segmentations of three crew members of the Glenda Lou during the 1975 season. The reader may wish to compare their differing segmentations with their differing job routines (see Figure 4).

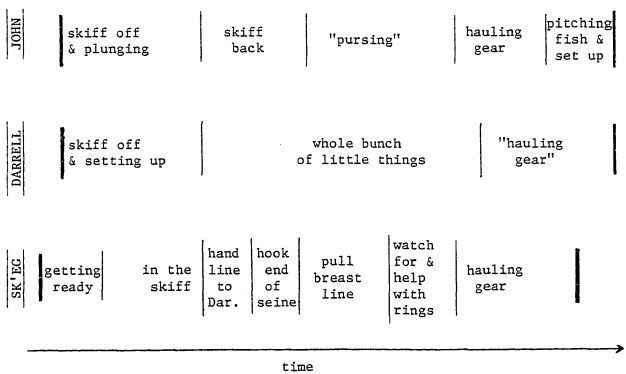

Figure 14. Varying segmentations of "making a set" by three members of the same crew

[1] Louie, from my second year, did not. I suspect that this is at least congruent with, if not partially responsible for, the fact that he never achieved a normal comprehension of seining.

With the growing literature in Anthropology on inter-individual variation, this variation among the segmentations of fellow crew members is perhaps not so surprising as it would have been a few years ago. Nonetheless, it is interesting as a phenomenon and provokes several ethnographic comments in order to better understand why a seiner constructs the segmentation he does.

Especially during one's first seining season, there is a strong element of faith on the part of the crew member that the "system" is good and not itself messed up. When fouling does occur, the good rookie blames himself and not the operation. This strong sentiment is most often verbalized as the belief that so long as each person does his work, everything will go right. Of course, this phrasing presumes that the work has been partitioned by the job responsibilities. But, my point is that the crew member's faith in the system frees him to wear blinders and fixate on just his own responsibilities. This attitude partially accounts for the variation in segmentations because it has the effect of "isolating" one's own work from the work of the crew as a whole. Thus, although one's work articulates at many points with others' (see Toy 2 Series), the cognitive organization of one's work is a private concern, so long as he gets that work done properly and does not stop the whole operation.

As a second comment, there is a functional advantage to segmenting the whole process. It is easier to remember the crucial "what do I do next?" job if one has constructed a hierarchical organization rather than reckoning in terms of the initial single-levelled ordering. This

can be shown a priori with the algebra of permutations, and it registers experientially as well. Thus, there is a functional motivation to construct a segmentation, but, of course, this motivation does not determine what that segmentation will be in specific.

These two factors--faith in the seining tradition and the functional advantage of multi-levelled cognitive organization--make it appropriate and wise, respectively, for a crew member to segment his job routine into phases. What remains odd is that a seiner feels his segmentation is "natural." Normally, if a classification is "natural," one would expect others to share it, yet as already seen, there is variation in what is perceived as "natural," even among people working together closely. To understand how the various segmentations can all appear to be the natural way of carving up seining, one must also realize that: (1) no one talks about these things in normal situations, rather they are brought to light only by inquisitive social scientists, (2) no one would feel argumentative concerning the correctness of his own version over other versions, and (3) each segmentation is "natural" in terms of the organization of a single crew member's routine, not as a statement about the structure of the operation as a whole. In other words, a segmentation of "making a set" pertains to and derives from one person's job routine, and it only partially reflects that routine's relations to the whole process. As the job routines vary, so do the segmentations. Figure 14 supports this in one way--Darrell, Sk'eg, and I did different things while making a set (see Figure 4)--and Figure 15 supports it another way.

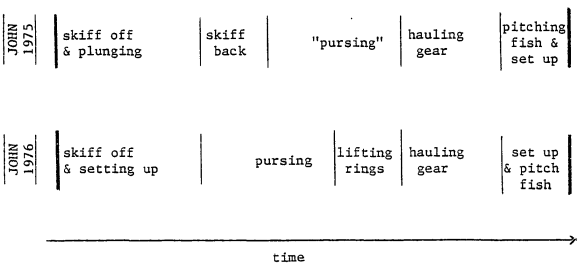

Figure 15. Varying segmentations of "making a set" by the same crew member (me) from two consecutive seining seasons on the same boat and same style of seining

My job routine my second year was rather different from my first year's. Because I was deck boss in 1976, my first segment was dominated by the responsibility to get everything set up and ready for the skiff's return rather than plunging. Since I was pursing on the inside drum, I felt that pursing began almost immediately on the skiff's arrival and that pursing was really the dominant activity in my routine. In 1975, I had many little jobs associated with the skiff's return, and although I recognized that "pursing" was the dominant job of the period, since I was not doing it, "pursing" was merely a heuristic for a segment. The other differences are, similarly, related to differences in my job responsibilities one year compared with the other.

Thus, I suggest that the appropriate understanding of a given

segmentation is to see it as the resolution between two principal factors: (1) a socially shared but vague jargon, which segments the process as a whole into high-level functional phases, and (2) the specifics of a particular job routine, which are variable both from crew member to crew member and from year to year for the same person. The products of the resolution--"natural" phases of a set--are more meaningful to their creator than just words from a common jargon, but like the jargon, they provide a more global comprehension of the process of seining than the small-scale, "jobs" way of thinking. This resolution and all of the cognitive re-organization that it involves develop during the rookie season. In subsequent seasons, there seems to be a tendency to modify prior segmentations directly, rather than repeat the whole resolution process anew. For example, when I changed boats and worked on the Jean D, I did not begin at the beginning, that is, think in terms of a string of beads. Instead, I assimilated my new work responsibilities with previous high-level cognitive structures and accomodated to discrepencies where necessary. The data in Figure 15 also illustrates this point in that the boundaries between Segments 1 and 2, 3 and 4, and 4 and 5 remained the same despite qualitative, substantive differences of the segments themselves.

Looking at segments one at a time rather than focusing upon their boundaries, there are three sources of a segment's unity.

(1) A segment has temporal continuity. It does not occur, subside, and recur in the same set. Although there are affinities between portions of different segments, a segment is a temporally continuous episode within the process of making a set.

(2) A segment has a characteristic emotional tone, or ethos, which is a regular part of its performance and which is also active, in weaker intensity, when reflecting upon that segment in the "re-living" mode. The ethos of any given segment is hard to delineate owing to our relatively small vocabulary for emotions, but part of the awareness of a segment comes from its specific, fairly regular mixture of emotions. These may be invisible to an observer. For example, I have a tendency to talk and joke both while pursing and while hauling gear, but inside of me, I feel very differently about my work during these two time periods. Thus, in saying that a segment has a characteristic ethos, I am referring to its psychological manifestation within one individual and not to what tone may be dominating crew socializing during that period.

(3) A segment has a characteristic unified action mode. Such a unified action mode, once developed, constitutes a fluid fabric in time. The person will be aware of "getting ready" to enter into such a mode and will be aware of emerging from it, but when engaged in its characteristic activities, he will be unconscious of it. All the work in a segment will appear in some significant sense to be "the same kind of work."

These, then, are the three sources of unity for the high-level segments of a set. There are intermediate and different kinds of cognitive structures ranging in scope between the segments and the jobs. Here I have concentrated on distinguishing segments, or phases, of a set as a distinct level of structuring. While I can attest to the accuracy of my criteria for my own seining experiences, I can only presume that they are accurate for other seiners. From conversations with Sk'eg and based on a couple of his spontaneous remarks, I believe my criteria describing the psychological unity of a segment would hold for him too.

To this point, I have provided data on inter-individual differences and offered some remarks as to what makes a segment cohere as a unity from the seiner's own vantage point. The bulk of the discussion was geared toward the psychological, substantive variation of segmentations.

But, in addition to the unifying, variance-reducing impact of the common jargon, there is another force toward social standardization of individuals' segmentations. Certain events in the seining process are especially important and require more than usual crew attention and cooperative work. Each of these special events is plainly visible and each involves more than one work track, that is, each of these events is truly public. These special, regular happenings are: releasing the skiff to begin a set, when the seine is completely off the stern, retrieving the skiff's end of the seine, when the rings come up and getting them on deck, commencing to haul gear, and lifting the bag of salmon out of the water. These events have the effect of synchronizing at least some of the crew members' segmentations at certain junctures. They do not necessarily induce shared meanings nor identical segments. Rather, they have the effect of aligning perhaps different segments at their temporal boundaries. For example, all three people who actually haul gear will have a "hauling gear" segment, and their segments will initiate at almost the same time, whereas whomever runs the power block is not as likely to share in this synchronization. This was the case in Figure 14, where Sk'eg and I shared a segmentation boundary (temporally synchronized), but Darrell, whose job duties during that period were not directly involved with hauling, did not initiate one of his segments with us.

My Segmentation of a Set, 1975 Season

The following descriptions are of uneven quality. The main focus is upon two special segments, Segment 2 and Segment 4. They are the emotional highpoints of the whole process, and though both are recurrent powerful

experiences, they are quite different from one another in emotional tone and in basic nature. The other segments--1,3, and 5--could, in principle, be explored in comparable detail and depth, but I have not done so. Nonetheless, rather than omitting them from discussion altogether, I shall provide short sketches of them, and thus, I take each segment in order. These descriptions are micro-ethnographies of rather short time ranges.

SEGMENT 1 --

Place in a set:

It begins when I find out that we are about to make a set and lasts until I become aware that the skiff is returning to the main boat.

Characteristic ethos:

The beginning is a pleasant anticipation and tension which subsides gradually after the little explosion when the pelican release lets the skiff go. By the time the seine is off the stern and the boat tow line hoisted up, the excitation is winding down to a jocular, social plateau. Plunging is monotonous, mindless work. My attention wanders from the scenery to home to school to conversations. I have a cup of coffee and a small cigar when Darrell or Richard take over plunging for me. I watch for jumping salmon and am happy. All the time, however, I know that now there is no alternative than to complete the whole process of making a set. This means jellyfish burning on my wrists and face. So, despite the immediate joys, I know the frantic, rushed times are ahead. My daydreaming is rudely

interrupted as either someone verbally notes the skiff heading back or as I hear its engine revving and see the extra smoke. As it drags its end of the seine back to us, I begin getting psyched up for the next phase of my routine. The rise of excitement is slower than that at the beginning of Segment 1 because I have more time to prepare myself and I can see when Segment 2 will begin. The flow of emotional tone is represented in Figure 16 below.

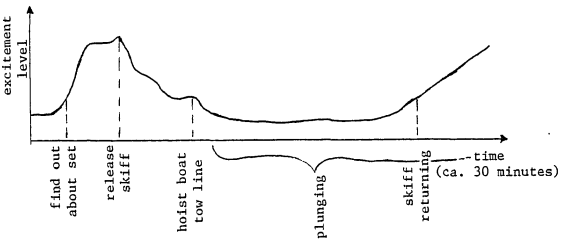

Figure 16. Crude time-graph representation of Segment 1's ethos intensity

Characteristic unified action mode:

The work is not at all strenuous. There is a lot of watching and checking to make sure things behave correctly, that lines are not fouled, that we do not let salmon escape the set between the seine and the boat, etc. The work does not require or compel undivided attention, and the tense of my thought leaps about from the present to past and future. I am "working," but the work

mixes easily with normal social behaviors, e.g., conversing (not chattering), smoking, joking, or being moody.

SEGMENT 2 --

My second segment from my 1975 season begins just about the time our skiff is at the bow of the boat. It lasts until I am pulling corks on the stern, while Darrell and Richard are pursing. Essentially, then, the segment's time range covers our retrieval of the skiff's end of the seine and the beginning of pursing.

The emotional tone of this segment is a high intensity rush. Everything is under a strong time pressure. As a crew, we are transferring enormous physical tensions from our boat's and skiff's engines to hooks and ropes. If we take too long to accomplish these transfers, the force of the tide pulling on the seine grows and could pull the seine free (hence, the salmon could escape and nullify the set). Owing to the physical forces, this is also a relatively dangerous time period, when equipment failure could cause serious injury. The various jobs of this period are subject to maximal division of labor (in the effort to reduce the total time involved). Hence, I always have the feeling that for two or three minutes, I am part of a human clockwork. So, I call the ethos of this segment a "clockwork rush."

The unified action mode demands rapid movement between and fast execution of a host of little jobs, each of which articulates temporally with other crew members' work. Once I developed a conceptualization of this segment, these little jobs were like accent marks in a steady burst of energy, like the identifiable explosions from a machine gun. My

attention is locked in to the immediacy of the here and now. I feel as if I enter a tunnel when this segment begins, and only after emerging two or three minutes later do I regain my normal time sense and awareness.

To describe this segment in greater detail, one should first note that there are different levels of cognitive dynamics. At the highest level (L3) is the segment itself--the cohesive energy expenditure and socially coordinated clockwork rush. At the lowest level (L1), one can identify discrete unities of short durations. In between these two, there are unities (L2 entities) which encompass several lowest level unities, but are themselves small compared to the whole segment. In an analogy with music, L1 entities are comparable to notes, L2 entities are similar to measures and phrases, and L3 segments correspond to the key, time signature, and tempo. This analogy is enhanced if one imagines that all music is for woodwinds and brass, because in such a case, the phrases (L2 entities) are not only aesthetically motivated, but also partially induced by breathing requirements of the performer (i.e., an external source similar to the functional advantage of multi-level organization).

In addition to the three levels, there are different types of cognitive structures. First, there are sensorimotor units underlying each "little job." Such a unit has a clear beginning, a clear ending, and a sharply bounded, homogeneous state of mind characteristic of its performance. Second, there are "rests," to stay with the music analogy. Rests have clear beginnings and clear endings, but no sensorimotor units (pertinent to seining per se) are involved. Rests are of two kinds: (a) a wait is somewhat bounded by a conscious sense of "waiting" for some

situation to occur, thus a wait implies "interactive sensitivity;" and (b) a rest is somewhat bounded by a generally sustained effort toward some goal, but that general effort is not related directly to the performance of an L1 sensorimotor unit. Sensorimotor, wait, and rest units are all L1 entities.

L2 entities form hierarchically around L1 entities, but they have their own sources of coherency. I shall call all L2 entities func units, ignoring differences among them. Basically, func units are entities having coherence-in-time which function to organize the L1 entities into short strings of action. The degree of coherence of an L2 entity is often enhanced to the extent that it is a meaningful unity in the context of reflective, functional thinking about seining. If when reflecting upon one's work, a given L2 entity acts as a unity in imagining causal chains, whether they be forged teleologically in terms of "purpose" or at smaller scale in terms of "mechanical thinking" (see page 207, above), then that L2 entity's coherence will be increased. Similarly, the degree of coherence of an L2 entity (a func unit) is increased if it has a relatively standardized social label, that is, if it has a collective representation.

The last type of cognitive structure pertinent for what follows is a waft. A waft has no clear beginning and no clear ending, but it does vary in intensity. A waft is never a unit of analytical, causal reflection on seining nor does it have a collective representation. By intent, I do not characterize wafts as discrete units because they are used in my descriptions to represent the way feelings feel. Wafts may occur simultaneously with sensorimotor units, wait units, rest units, or func units.

Figure 17 presents my graphical representation of sensorimotor, wait, and rest units, as well as wafts. Func units (L2 entities) are graphically represented simply as circles encompassing L1 entities.

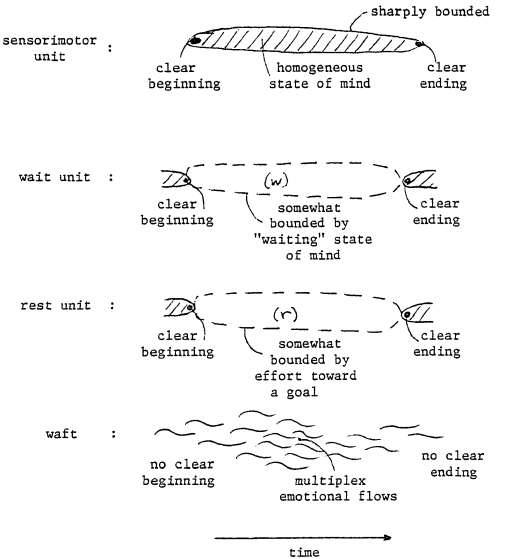

Figure 17. Forms of elemental Segment 2 entities

The Toy 3 Series builds from the discussions above concerning levels and kinds of cognitive structures. The basic idea of the series is to represent purse seining as a dynamical system. But, unlike the Toy 1 and Toy 2 Series, the Toy 3 Series is not done from an objective posture. Quite the contrary, Toy 3 attempts to represent purse seining as it happens within one seiner's job routine. Its goal as a simulation is to reveal at least some of the inner life of a human worker. Since I have easier access to my own inner life that to those of others, the Toy 3 Series schematically portrays my cognitive activities directly pertinent to purse seining in terms of the cognitive structures discussed above, that is, in terms of sensorimotor, wait, rest, and func units and wafts. These schematics enable me: (1) to represent a short time period from my 1975 Segment 2 as a model of my own cognitive structures, and (2) by extrapolating the basic forms of cognitive structures from myself to others (and as gross as these are it is unreasonable to presume that they would be individual-specific formalisms), to include in the same diagram the relevant social others as they came in and out of my primary attention. This first version of the Toy 3 Series is found in Figure 18.

A different and perhaps more advanced version of Toy 3 is illustrated in Figure 19. There, I treat sensorimotor, wait, and rest units as kinds of nodes (see pages 360-361, above). In this second version of Toy 3, my concern is to model how my cognitive structures (L1 entities only, in this case) initiate and maintain activations. For an admittedly small time range and using rather gross units, Figure 19 illustrates at least the basics of an internally dynamic system or model.

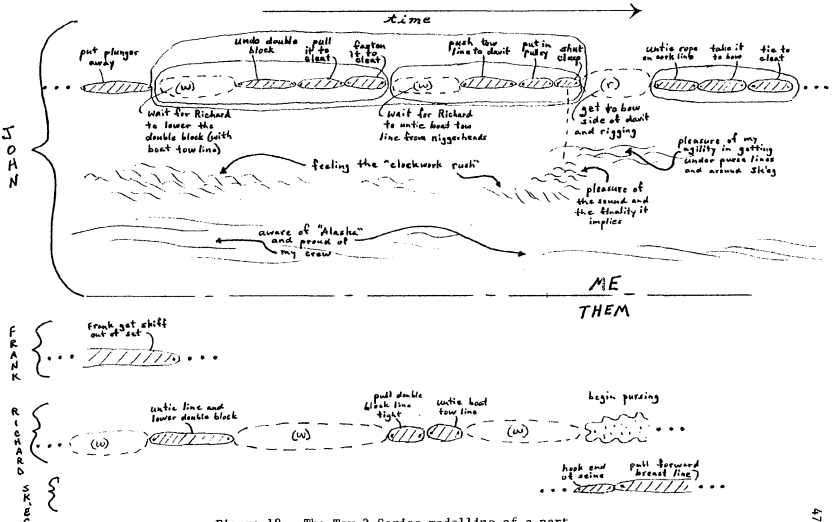

Figure 18. The Toy 3 Series modelling of a part of Segment 2

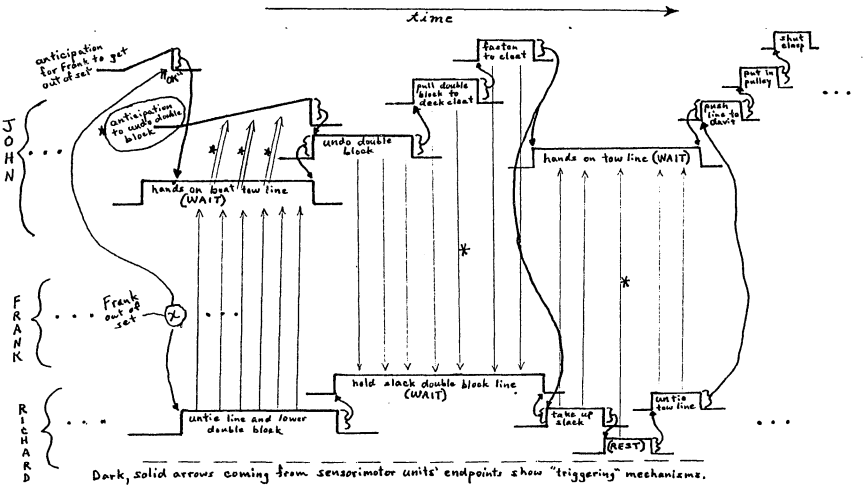

Figure 19. Advanced Toy 3 Series modelling of a part of Segment 2

Although not worked out in comparable fullness, I believe the two examples of the Toy 3 Series evidence the general superiority of this mode of thinking about purse seining over the Toy 1 and Toy 2 modes. Oddly, given that the particular data are based on introspection, I feel the Toy 3 Series is , in principle, more empirical than the "objective" characterizations from Chapter 3. It has a broader range of empirical incorporation. Furthermore, it has affinities with the cognitive structures (c.s.) vision of knowledge-behavior presented in Chapter 5, and I hope the examples have given the reader a clearer idea of the basic approach following from that vision.

The general impulse of Toy 3 is not, however, specific to the nature of Segment 2. A version could be made for each segment, in which case, a person's whole job routine would be modelled. If this were done, it would require additional types of cognitive structures in order to model things such as long-term jobs (e.g., pursing) and the wandering of attention away from purse seining activities, to name just two phenomena. Owing to its nature, Segment 2 is relatively easy to model in Toy 3 Series fashion. It requires fewer types of cognitive structures that would Segment 1, for example, and the segment is "dense" in terms of purse seining activities per amount of time. These considerations guided me to try Toy 3 thinking on Segment 2 rather than other phases, but the main motivations for the Toy 3 Series derive from its superiority as a way of modelling human behavior-in-time when compared to simple, single-levelled, externally driven flow-chart depictions of sequences in behavior.

SEGMENT 3 --

Place in a set:

It begins after I return from the bow to the stern (to pull corks), and it lasts until the rings come up and we get everything ready to haul gear. The "dominant" activity of this time range is pursing, which Darrell and Richard are doing, and I tend to think of this segment in terms of that dominant job in spite of the fact that I do other things.

Characteristic ethos:

At first, I am still calming down from the clockwork rush of Segment 2. By the time I have started pulling corks onto the stern, I am relaxed and watching how the set is going. I feel socially isolated by the fifteen to twenty feet separating me from my fellows. On deck, Skipper is the only other person not engrossed in his work. I feel that I am a marginal worker, almost an observer. I am watchful and relaxed. My thoughts wander to other boats in our area, to the fish jumping in our set, to wondering why I have to pull up these corks only to throw them back in just a few minutes. I wave to Frank in the skiff. If Sk'eg comes back to help me, we talk and I ask how many jumps he saw along the shore when he was out in the skiff. When the corks begin to pile up at my feet, I commence thinking about my cork pile and the dread I have of hauling gear. I plan how I am going to lay down my cork pile this time to make it better than last set's. So, generally, the segment's emotional

tone is a relaxed watchfulness which regularly becomes more and more anxious and self-conscious as its end draws nearer. It resembles Segment 1's relaxed period except that I am more alert to what is coming up next.

Characteristic unified action mode:

The work is physically arduous, but not at all demanding mentally. Pulling corks, the main job of this segment and the most strenuous job in seining, is a long-term, continuous task that ends only when the rings come up as a result of the pursing. Because it ends at a socially announced time, I do not have to pay much attention to my own work. When this long-term, "satellite" job does end, I begin a series of leisurely little jobs and assist others with their work. The ethos characteristic of Segment 4 begins to mobilize toward the end of my Segment 3, however, and this emotional anticipation (supported by visual cues of the impending changes) blends the end of Segment 3 with the beginning of Segment 4. Segment 3's jobs are done at my own pace, and this aspect, marks its temporal boundaries and is responsible for why I feel its jobs have a common nature.

SEGMENT 4 --

My fourth segment corresponds to the highpoint of making a set: "hauling gear." This label is common speech aboard seine boats. That it is indeed a special and remarkable clumping of activities can also be seen from the fact that seiners identify themselves to each other

according to which pile they make during this episode. These social identities are on a par with "skipper," "skiff man," and "cook." Thus, when rubbing shoulders in a bar, if two seiners want to know what each other is doing in Alaska, the order of self-identification is typically: "working on a seiner," then "I'm {cork man, lead man, web man, skiff man, skipper, cook} on the BOAT NAME." This level of identification is sufficient for most cursory social encounters.

In addition to the coherence arising from its collective representation, hauling gear is also a major functional unity. It is what a crew does after completing the entrapment of salmon (pursing). It is what must be done before the bag of fish can be brought out of the water and onto the deck.

Both of these factors--collective representation and functional unity--are auxiliary sources of the segment's coherency. They refer to and take note of the segment which has a clear unified action mode and a unique ethos. All of these sources are complementary, and the result is a very strongly-felt segment of seining.

While hauling gear (more specifically, piling corks), I am totally involved with the work. I have a heightened sensitivity of my surroundings, both spatially and socially. I am worried that I will slow the crew down. I am acutely aware of our teamwork, but it is not the same sensation as that during the clockwork rush of Segment 2. The teamwork while hauling is based upon our co-activity in closely parallel jobs, whereas the clockwork rush derives from a division of labor with rapidly interacting components. The final ingredient in this segment's emotional tone is an incredibly strong aesthetic drive to make a beautiful cork

pile. The drive has some functional basis in that my cork pile should play out smoothly the next set and it should leave room to pull up the skiff on the web pile, should that be desired. But, the principal motivation to have a well-formed cork pile hanging a graceful third over the gunwale is simple aesthetics.

The hauling ethos persists at a "psyched up" level almost to the end of the segment. Our work has a natural ending, however, and when we are left with only a "bag" of fish in the water, we stop hauling. This, of course, does not happen all of a sudden and without warning. I can tell that we are nearing the end by how far along my cork pile is and by looking to see whether Darrell has untied the cork line from the bow cleat. Segment 4 winds down while we do the things associated with lifting the fish out of the water. As I climb down from Mt. Cork (as I think of it) to help get the fish up, I am relieved that the set is drawing to its end. I know that only the anticlimatic Segment 5 remains.

As a kind of work, hauling seine is remarkable. It is a race with the machinery which does the heavy part of the job. The Puretic Power Block lifts the wet seine out of the water and drops it to the deck hands below. Their job, collectively, is to separate the seine into its three principal parts and pile them so that the seine will not tangle and foul as it plays off the stern the next set. Though the jargon uses the word "piling" for web, corks, and leads alike, perhaps for corks and leads the word "coiling" would be more accurate. The work of all three "pilers" is continuous to the extent that the seine

comes down to them at a constant rate and the seine itself is a unity. But, each of these piling jobs is internally differentiated and not strictly continuous in terms of the way a seiner makes his pile.

In the following formalism, Toy 4, I concentrate on piling corks, because that was my job in 1975. I have done all three gear piles, and I seem to be a natural web man. The point is, however, that despite the differences between corks, web, and leads, they could all be modelled in the fashion of Toy 4.

Toy 4 is a simple spatial simulation of piling corks. It begins by dividing the cork man's one third of the back deck into sub-areas. These regions are identified in terms of a particular style of piling corks, specifically the "horseshoe loop" style. Other styles, for example, the "three pile" style, would have different divisions of the deck space. Figure 20 shows the regionalization of the deck appropriate to the horseshoe loop style.

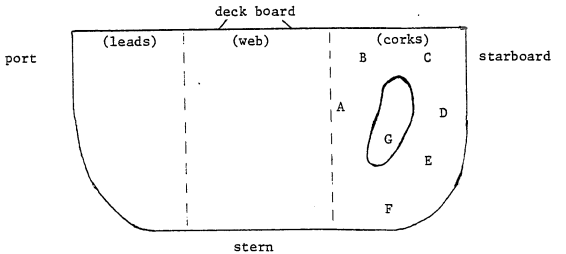

Figure 20. Plan view of the back deck of a seine boat showing the sub-regions of space appropriate for describing horseshoe loop style of piling corks

The various coiling motions of the cork line as the seiner piles it on deck can be represented as state changes in the set of spatial states {A,B,C,D,E,F,G}. In such a way, a whole cork pile (built by coiling five hundred yards of cork line) could be described as a string of symbols. Each time the cork line changed positions, that is, changed "states," the new state is added to the right end of the string. Thus, a whole cork pile would be described by a string of the spatial states such as: #BCDEFEDCBAGGGGABCDEDCB...F# . It should be clear now why the specific regionalization depicted in Figure 20 is appropriate only for a given method of making a cork pile.

With nothing more than the set of spatial states, an observer could record the manufacture of horseshoe loop cork piles. From such descriptions of real cork piles, the observer would discover that there are definite rules of motion between the various spatial states. For example, the following sub-strings, and several others, should never occur: ...EGC..., ...AD..., ...BABABA..., and ...EC... . Rather than trying to list all the unacceptable sub-strings, however, which would be to construct a "garbage filter" model, it is both easier and more in line with the impulse of simulation to write the permissible state changes and their sequences. Thus, one represents these acceptable motions between states as transformational rules in the set of spatial states. This simplest phrasings of these rules for piling corks in the style I used in 1975 are listed below in formulation (14).

(14)

A→B	B→A	A→G	G→A	G→G
B→C	C→B	B→G	G→B	
C→D	D→C	E→G	G→E	
D→E	E→D	F→G	G→F	
E→F	F→E			

The simple system of transformations in (14) does not, however, sufficiently constrain the motion of the cork line through the spatial states. Nothing in these rules prevents simply going back and forth between two adjacent states, for example, producing the unacceptable sub-string ...DEDEDEDE... . Intuitively, what is missing from (14) is any representation for the "loop" conception of the work. A seiner does not lurch like a mannequin moving the cork line from one space to another. Rather, a real seiner doing this style of corks thinks ahead of where the cork line actually is. He plans where the next "loop" should go to build the pile out and up in regions C and D and sloping down to met the web pile in regions A, G, and F. But, before describing the phases of piling a horseshoe loop cork pile, we should first remedy the absence of "loop" modelling.

(15), below, describes the empirically accurate, acceptable loops I would use while piling corks. Using the spatial states, a loop is a string of these symbols arranged in a definite order and of definite length.

(15)

ABCDEF	FEDCBA	BCDEF	FEDCB	CDEF	FEDC	G.*.G
ABCDE	EDCBA	BCDE	EDCB	EG	GE	AG GA
ABCD	DCBA	BCD	DCB	FG	GF	BG GB

There are some peculiarities in (15) that require clarification. The way cork pile construction is being modelled assumes that each time one of the spatial state symbols appears in a string, it has the same amount of cork pile deposited as all the other symbols. That is, if "D" appears in a string, it has the same amount of cork pile associated with it as does a "B," "A," or any of the others. This is reasonable because the seine comes over the power block and drops down to the deck at a constant rate. Thus, Toy 4 is a kind of temporal model to the extent that each "occupation" of a spatial state deposits the same amount of cork line. Given this general modelling condition, it is necessary to represent what a cork man thinks of as "filling in" the center of his pile as an iterative string of G's rather than as a single occurrence of G. Therefore, this "filling in" maneuver is modelled in (15) as G...G. But, this string of G's should not go on indefinitely, so I have placed an asterisk over the series symbol to indicate that the iterative accumulation of G (i.e., the cork line's continuing "occupation" of G) is limited to the order of perhaps twenty symbols.

Related to the peculiarity of G.*.G, there is a modelling problem of getting the cork line from the horseshoe loop regions (i.e., A through F) into G. There are four reasonable entries to and exits from G: A, B, E, and F. Hence, the pseudo-loops AG, GA, BG, GB, EG, GE, FG, and GF are the formal representations of this entry and exit. But, one needs a condition on the occurrence of these special pseudo-loops to prohibit them from following one another without intervening "loops" or iterative replications of G. This rule would prohibit strings such as the

following: ...AG,GA,AG,GE,EG,GB... . One way to handle this problem in the formal simulation of making a cork pile is to group AG, BG, EG, and FG under a new symbol, ENTRY, and their inverses, GA, GB, GE, and GF, under a new symbol, EXIT. The ensuing well-formedness rule on strings can now be stated simply as follows: an ENTRY and an EXIT cannot be adjacent to one another in either order.

The basic approach of this revised Toy 4 is to make sub-strings of the base states the unities out of which the simulated cork pile is constructed. The notation changes slightly, however, when this is done. Instead of getting simple strings of spatial states, we get sub-strings separated by commas. To eliminate the commas and restore the previous simplicity, we need a notational convention. The convention would work as diagrammed in (16.1)-(16.3).

(16.1) BCDEF,FEDCBA,ABCD

(16.2)
```
BCDEF
    FEDCBA
        ABCD
```

(16.3) BCDEFEDCBABCD

This operation on strings, which I have diagrammed in (16.1)-(16.3), is commonly called "concatenation" (see Wall 1971:165). But, while such operations are interesting and formalisms are intriguing, Toy 4 is built for a purpose more than to see what sort of formal game it can become. Its real function is to illustrate a basic approach to understanding a process. It is not yet sufficiently powerful to model the

various strategies a cork man uses as he plans his pile, but I suggest that these could be incorporated in a more elaborate formalism along the same general lines as Toy 4. Given the incompleteness of the current model, I hope the reader can glean from it the directions subsequent elaborations might go and how Toy 4 construes piling corks.

If one ponders the fundamental logic of Toy 4, it becomes clear that it fosters an analytical frame of mind which seeks out rigidly discrete chunks. This is the first step in its construction, and the tendency does not stop there. This frame of mind almost literally takes a meat clever to experience, and in the case of cork piling, the experience is empirically much more fluid than its Toy 4 simulation. In the simulation, the subtlety of the intuitive feeling of "loops" has been smashed like a toad run over by a truck. While it is true that the seiner feels there are nebulous sub-unities within his cork piling, the chunky-sort-click, mannequin model of cork piling does great violence to the very strong feelings of continuity in the work. The Toy 3 Series modelling, or basic approach to cognitive modelling, is much more in line with my more poetic images of the nature of cognitive processes. Whereas Toy 4 images after the fashion of rusty typewriters, automobile engines, and kaleidoscopes, the Toy 3 Series is moving in the direction of bubbling rivulets, finger-paintings, and shimmering irridescences.

SEGMENT 5 --

Place in a set:

It begins as I climb down from my cork pile to help get the fish out of the water. Its ending is variable, depending

on several factors, most notably how many fish there are and whether or not I have a little or a lot of jellyfish to wash off my fish (actually, we use paper towels for this). Basically, the end of Segment 5 is the end of a set, and I suppose that a set is really over when the fish have been pitched and the deck cleaned off. However, this varies because if there are no other boats around the fishing spot and the fishing is good, then the next set can begin before the fish have been pitched from the deck into the hold. Nonetheless, should this overlap occur, I would still regard pitching the fish as part of the first set and not part of the second set's Segment 1.

Characteristic ethos:

The key word for the ethos of this segment is "anticlimatic." The big event is over (hauling gear and seeing the catch), and from the time I walk down from my cork pile until I am sitting comfortably in the galley having coffee with no more jellyfish on my wrists and face, nothing of any real significance in my thinking happens. There is a little flurry of hurry up and get ready for the next set, but emotionally, I am tired and unresponsive. Should we have a large catch, then this energizes everyone and there is much diffuse energy expenditure and happy signalling to the Skipper that we are eager to make another set and soon. If the catch is only average, then I feel we are supposed to make a show out of saying that next time we will do better. And, should we

have a very poor catch, then I feel the social pressure even more strongly to act as if I am not disappointed. But, if this happens too often, then these sweet lemon shows of reserve enthusiasm have all the thrill of a pep rally for the debating team at a Big Ten university. In such times, the seining ethos in total comes in to ward off defeatist attitudes, and the experienced hands begin recounting previous years when they made the whole season from one big set. Usually coupled with these "good ole days" stories, one hears certain quaint expressions such as "we've got to hang tough, boys." Despite my cynical tone now, these efforts to rally a flagging spirit did have some effect, and even at the time, I admired the attempts. Summarizing the ethos of Segment 5 is a bit awkward because its specific nature any given set is so influenced by the natural variables of seining--the size of the catch. Regardless of the variations, each Segment 5 was a time of decompression, a time to re-adjust to normal, boat social life levels of behaving.

Characteristic unified action mode:

None of the jobs in my Segment 5 were difficult to perform nor especially unpleasant. Furthermore, other than a couple of my jobs, I was my own pace-maker. The time pressure which has saturated almost the whole set from the time of the skiff's return lifts only to be replaced by my own desire to hurry up with the remainder of the set so that I can get comfortable

again. There is a fuzzy division of labor which never became well-structured just as Segment 5 begins. This period of regularly occurring confusion only adds to my overall feeling that Segment 5's jobs are simply the "remainder" of the set. And, other than the so-called "set up" jobs which are very neatly allocated, pitching fish and cleaning up the deck are quite leisurely done interspersed with spontaneous and short-lived games of deck hockey (played with a small white jellyfish). The jobs in the segment are not strongly the "same kind of work," but the ethos of decompression and anti-climax is so strong that the segment coheres anyway.

VITA

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Since entering the graduate program in anthropology at the University of Illinois, John has been a teaching assistant for eight semesters. He was also a substitute lecturer in anthropology at Richland Community College. He held a University Fellowship during one of his student years, and he worked as a salesman at Buck's Pipe Shop, as a bartender at Levis Faculty Center, and as a stock boy at Foremost Liquors. He also undertook several research projects while in residence on the campus. These were: the development of relational thinking in kinship terminology of children, a quantitative attribute analysis of lithic materials, and four summers of archaeological excavation at Cahokia Mounds State Park, Illinois. He has published the following articles: "A Case Study of English-American Kinship Terminology," "'Everyday' and 'Esoteric' Knowledge," and "Deducing an "Empirical" Hypothesis." His Master's Paper was a structuralist study of the cosmology of an Indonesian people: "Some Atoni Symbolisms: A Study of the Integration of Cosmology and Everyday Life."

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