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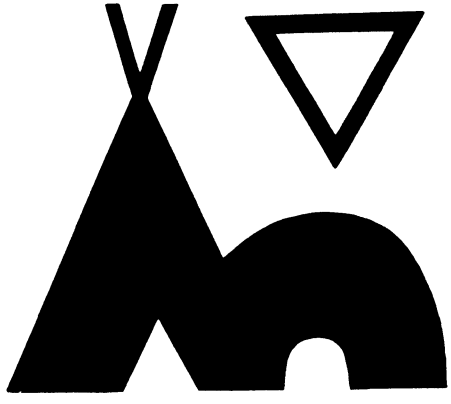

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EDITORIAL

Kathryn T. Molohon
Laurentian University

This issue of ANTHROPOLOGICA represents a substantial breakthrough in what is termed "desktop publishing." The issue was typeset entirely on IBM-compatible personal computers, and camera-ready copy for offset printing was then produced on a readily-available printer for personal computers. Our current method of production represents the third computer format which we struggled through in an effort to devise computer techniques to simplify our publishing procedures, finances, and future workload. Although the development of these techniques was tedious and sometimes discouraging, desktop publishing is the key to the future for academic journals such as ANTHROPOLOGICA. Because desktop publishing simplifies labor and costs, other academic journals in various countries are also experimenting with this procedure. Once the right coding formats and auxiliary techniques are developed, desktop publishing seems to be the only way to proceed, and all other methods appear to be counterproductive. With the continued improvement of computer technology, desktop publishing will become the standard rather than the exception for academic journals.

We would like to thank Dr. Michael Dewson, Dean of Social Sciences at Laurentian University, for his outstanding help in making this breakthrough possible. Our part-time assistants, Carolyn Malott and Lucie Saebel, as well as Ashley Thomson, Head of the Reference Department of the Laurentian University Library, and Rose-May Démoré of the Department of History at Laurentian University, also contributed heavily to this process. The entire undertaking has been a team effort centered on the cooperation and creativity of a group of no-nonsense, professional women and an exceptionally dedicated and able university Dean.

HISTORIC FERTILITY PATTERNS IN A NORTHERN ONTARIO OJIBWA COMMUNITY: THE FORT HOPE BAND

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Cet article examine le problème de la fertilité des femmes de la nation odjibwée inscrites comme membres de la bande indienne de Fort Hope. Il s'agit du groupe de celles qui avaient contracté mariage dans la période entre 1930 et 1944. La présente étude possède un caractère inhabituel en ce que les données utilises aux fins de l'enquête sur les modes de reproduction du groupe sont presque exclusivement extraites de listes annuelles d'avant le traité de 1960. On laisse entendre que les relevés du gouvernement canadien concernant les populations autochtones seraient plus utiles qu'on le pensait pour l'étude des modèles de démographie historique.

This paper describes the fertility patterns of a cohort of Ojibwa women who were registered members of the Fort Hope Indian Band and who married in the late contact traditional period between 1930 and 1944. The study on which the paper is based is unusual in that data used for reconstructing the cohort's reproductive patterns are derived almost exclusively from pre-1960 treaty annuity lists. It is suggested that Canadian government records of native populations may be more useful for studying historic demographic patterns than was previously supposed.

INTRODUCTION

Research into the historic demography of northern Algonkian populations is hampered by a lack of empirical data. Archaeological information on boreal forest populations is scant (Sieciechowicz 1982), and discussions of demographic parameters in ethnographic studies tend to be scattered, fragmentary, and organized around divergent research questions (see Dunning 1959a; Rogers 1962;

Taylor 1972; Young 1979). The situation has developed, in part, out of the general reluctance of anthropologists to collect demographic information (Morrill and Dyke 1980), and from misgivings about the information that has been collected (Turnbull 1972; Salzano 1972; Hurlich 1983). Reports that Canadian government records of vital events for native people are unreliable prior to 1960 (Romaniuk and Piché 1972; Piché and George 1973) may also have discouraged attempts to reconstruct the past demographic patterns of Algonkian peoples.

This paper contributes to the sparse demographic literature on historic northern Algonkian populations by describing the fertility experience of a cohort of Ojibwa women who were registered members of the Fort Hope Band and who married in the late contact-traditional period between 1930 and 1944. The study is unusual in that data used to reconstruct the cohort's reproductive patterns are derived almost exclusively from pre-1960 treaty annuity lists. The main thrust of this paper is to suggest that Canadian government records of native populations may be more useful for studying historic demographic patterns than was previously supposed.

THE POPULATION

The Fort Hope Band is a small, isolated, Ojibwa-speaking community living in the boreal forest region of northern Ontario (latitude 51.8o, longitude 86o; see Map 1). From about 1900 to 1950, band members lived in contact-traditional base communities as a loose confederation of seminomadic hunters, trappers, fishermen, and gatherers (Taylor 1972). The gradual transformation into permanent settlements began after World War II and coincided with the decline of the fur trade and the extension of government services to native people (Driben and Trudeau 1983). Demographic information, available for isolated points in time, indicates that the band grew slowly from about 475 people at the time of the signing of Treaty Nine in 1905 to 800 people in 1950. Previous research has shown that the development of permanent settlements was accompanied by the demographic transition from high birth and death rates from 1950 to 1964, to moderate birth and low death rates from 1965 onward (Sawchuk, Herring, and Driben 1981).

MATERIALS AND METHODS

Treaty annuity lists for the Fort Hope Band, which are held by the Membership Section of the Department of Indian Affairs and Northern Development in Ottawa, were collected in 1975. Vital events information contained in these lists was used to identify a cohort of women who entered into a first marital union between

1930 and 1944. Using standard record linkage methodology (Wrigley 1966), all reports of births, deaths, marriages, remarriages, transfers to other bands, and enfranchisements were compiled for each woman using names and family registration numbers. In this way, a life history of vital events was produced for the eighty-four Fort Hope women who married between 1930 and 1944, and for their biological families.

The use of record linkage techniques to construct historical demographic data sets improves the accuracy of information on the study population (Drake 1972). By prospectively linking vital events for the Fort Hope band and creating biological family histories, a number of errors were detected in the records that might otherwise have gone unidentified. Late registrations of births and deaths, missing birth dates, multiple marriages, and double entries were corrected. In this way, some of the known sources of distortion that plague aggregate studies based on government records were reduced (see Romaniuk and Piché 1972; Piché and George 1973; Kunitz 1974).

Of the original sample of 84 women, eight were lost to observation through enfranchisement or transfer to other bands, and are not reported on here. Another 26 women were excluded either because they lacked a date of birth, or because they failed to survive to age 45. Thus, it was necessary to discard about one-third of the original cohort in order to derive the study sample of 50 women.

Information on the reproductive histories of these women was entered into and processed by the University of Toronto IBM-360 mainframe computer via Statistical Package for the Social Sciences (SPSS) computer programs (Nie, Hull, Jenkins, Steinbrenner, and Bent 1975). Age-specific and total fertility rates were calculated according to the following formulae (Howell 1979:124):

(1) Age Specific Fertility (ASF) = $f_{i/risk}$

Where: f_i = number of births per five-year age interval

risk = number of women in the sample X 5

(2) Total Fertility (TF) = sum of ASF X 5

The accuracy of the total fertility rate, and, hence, the quality of information upon which it is based, was assessed via the United Nations Test for Underreporting (United Nations 1967:31)1. Other standard fertility measures generated for the Fort Hope Band were the mean age of mothers at first birth, the mean age of mothers at last birth, the mean length of the reproductive period,

and the mean birth space. The infant mortality rate per one thousand live births was also determined for the cohort.

Statistical testing of the findings was accomplished using the G statistic, a log-linear test for homogeneity (Sokal and Rohlf 1979). These tests were carried out using SYSTAT (Wilkinson 1986) on an IBM personal computer.

RESULTS

The preliminary results indicated that there were problems with the treaty annuity data (Table 1). First, age-specific fertility failed to peak in the 20-24 or 25-29 age groups as expected for North American Indian populations of this period (see Romaniuk and Piché 1972; Piché and George 1973; Kunitz 1974). Second, when the age-specific fertility data were subjected to the United Nations Test for Underreporting, these data yielded a higher observed than expected total fertility (TF). However, the rates should be very similar, or the expected TF should exceed the observed TF. In the 1930-1944 marriage cohort, not only did the observed TF of 7.7 exceed the expected TF of 6.1, but it did so by a wide margin of 1.6 live births per woman. Even when the effects of the small sample of births was reduced by redefining P_3 as the mean cumulative births between ages 25-30, the expected TF (7.2) remained lower than the observed TF (7.7).

To investigate time differences in the quality of information, the cohort was subdivided into two groups: (1) women who married between 1930 and 1937 (n=27); and (2) women who married between 1938 and 1944 (n=23). Age-specific fertility rates, observed and expected total fertility, and infant mortality rates were compared between these two series of women (see Tables 2 and 2a). This analysis revealed that most of the problems in the full fifteen year cohort stemmed from women who married between 1930 and 1937.

For the women who married between 1930 and 1937, the treaty annuity lists generate a late fertility peak in the 30-34 and 35-39 age groups, rather than the expected peak in the 20-24 and 25-29 age groups (see Table 2). The United Nations Test for Underreporting shows that the expected TF (7.9) continued to exceed the observed TF (6.5). As Table 3 indicates, infant mortality rates were low at 37.7 per 1000 live births, which was well below the very high levels of infant mortality (136 to 200/1000) expected for this time period (see Latulippe-Sakamoto 1971; Sawchuk, et al. 1981).

Although a small degree of underreporting was also found in the 1938 to 1944 marriage cohort, this was substantially less than in the previous cohort. The fertility peak for this cohort was more

in line with expectations, occurring in the 25-29 age group (see Table 2a). The United Nations Test for Underreporting also produced more satisfactory results with an expected TF of 8.0, which exceeded the observed TF of 7.5. Moreover, the infant mortality rate of 145/1000 live births was closer to the expected very high values. Indeed, the disparity in infant death rates between the two cohorts was highly significant ($p < 0.001$), and was clearly a major factor in the underreporting inherent in the 1930-1937 cohort.

In view of the deviations introduced by the women who married between 1930 and 1937, the 27 women making up this cohort were eliminated from the study. Using only the more reliable data for the 1938 to 1944 cohort, a number of standard fertility measures were then generated for the Fort Hope Band (see Table 4).

As Table 4 demonstrates, Fort Hope women who began reproducing in the late contact-traditional period bore, on average, 7.5 children. These births occurred at 27.4 month intervals from the time a woman began reproducing at about age 21.5, until she ceased bearing children at about the age of 38.6 years of age. Fertility peaked in the 25-29 age group which, according to DeJong (1972), is characteristic of a developed nation. Given the probability of underreporting in the early age intervals of the cohort, the maximum age-specific fertility rate probably occurred earlier.

DISCUSSION

The results of this analysis should be viewed with caution owing to the small number of women upon which they are based (n=23). The findings reveal, however, that the total fertility rate of 7.5 live births per woman in the Fort Hope 1938-1944 marriage cohort falls midway between the low value of 4.7 for the Dobe !Kung (Howell 1979) and the high value of 10.2 for the Hutterites (Eaton and Mayer 1953). Given the results of the United Nations Test, TF probably approached 8.0, the upper limit suggested by Howell (1976:33) for well-nourished hunting and gathering groups.

Other features of the reproductive pattern of the Fort Hope 1938-1944 cohort include a relatively late mean age at first birth of 21.5, and a relatively long mean reproductive period of 17.1 years, with the last birth occurring around age 39. Like the Hutterites, the Fort Hope Ojibwa women appear to have been fertile for practically their entire reproductive period, and to have borne a child about every two years.

When the Fort Hope pattern was compared to those of other northern Algonkian and Athapascan groups (Table 5), the fertility

measures of these groups proved to be remarkably similar (see Piché and Romaniuk 1968; Sawchuk 1972; Roth 1981). The birth of a first child between the ages of 20 and 22, an average of seven to eight children, and a seventeen to nineteen year reproductive span may represent a widespread, contact-traditional fertility pattern.

Perhaps of greater significance, however, is that the findings of this study were generated from information contained in pre-1960 treaty annuity lists. While it is evident that the quality of the records is not uniform, this study has demonstrated that at least for the Fort Hope Indian Band, the 1940s mark the beginning of greater reliability in the treaty annuity lists.

Improved record keeping during this period stems, in large measure, from administrative and historic processes that occurred in northern Ontario. Much of the improvement in record keeping can be attributed to a statistically-significant increase in the number of infant deaths recorded for the later cohort. However, other improvements in record keeping also occurred at about the same time. For example, it became customary to record the names of individuals who were born, died, or were married. Previously, only a cryptic "boy born" or "girl married" accompanied the family registration number. The inclusion of given names facilitated the cross-referencing of deaths or marriages to the correct individual. Furthermore, the 1952 treaty annuity list which followed upon the heels of the new 1951 Indian Act, included the names and birth dates of the living members of the biological families. This filled in many of the gaps and clarified confused entries in the Fort Hope records. As the reporting of infant deaths and other improvements in record keeping occurred, the enumeration of births also became more reliable. Thus, fertility estimates for the 1938-1944 cohort were based on a more accurate reflection of the total number of children born.

The improvement in the Fort Hope records in the 1940s was not a random occurrence, and was closely tied to the shift from the preadministrative to the postadministrative era in the region (cf. Brody 1977). This improvement coincided with the expansion of Canadian government involvement in the lives of native people of northern Ontario (Dunning 1959b), and in the lives of the Fort Hope people in particular (Taylor 1972). Increasing numbers of non-native specialists living in native communities provided an opportunity to keep closer track of vital events. A case in point is the establishment of a nursing station at Lansdowne House in 1946 as a result of the reorganization of native health services under the Department of National Health and Welfare in 1945 (Graham-Cumming 1967). The presence of a full-time nurse undoubtedly meant that there was greater administrative awareness of births and deaths in the Fort Hope Band.

The development of permanent settlements and the demise of the contact traditional lifestyle were inextricably entwined with these administrative changes. Since under-enumeration of births, deaths, and marriages is likely to occur when populations are mobile, even for relatively short periods of time (Levine 1976; Pitkanen 1977; Wrigley 1978), the trend toward increased sedentism and social nucleation in the 1950s also led to more accurate recording of vital events in the band. As a result of these interacting trends, the quality and representativeness of government records increased[2].

CONCLUSIONS

The most important finding of this study is that, under certain conditions, it may be possible to estimate demographic parameters for native Canadian populations from pre-1960 treaty annuity lists. Although the lower limit for the Fort Hope Band appears to be around 1940, the quality of record keeping undoubtedly varied widely and it may be possible to push this horizon further back for other groups. Significant influences on the quality of government records for the Fort Hope people proved to be the extent of the involvement of Canadian government and biomedical institutions in the daily life of the band, and the degree of the band's mobility.

In addition, the reproductive pattern of Fort Hope women who married between 1938 and 1944 fits well with the available evidence for other northern Algonkian and Athapascan groups. Large families of seven to nine children, with births occurring about every two years between the ages of 20 and 40, appear to have prevailed among women who began to reproduce in the contact-traditional period. While it is premature to suggest that fertility patterns in northern North American native groups may have been relatively homogeneous during this period, further studies of this nature will help to confirm or reject such speculation.

Finally, exclusive reliance on any one set of records imposes severe limitations on the accuracy of demographic estimates. This point argues strongly for the collection of as many sets of records as possible for a population. Record linking of information contained in oral histories, parish records, and government records will inevitably produce an increasingly sound basis for demographic reconstructions of northern Algonkian populations than will dependence on only one source.

NOTES

Acknowledgements. This work was supported by Grant Number 453-84-0293 from the Social Sciences and Humanities Research Council of Canada.

1. The method uses the fertility rates of women in the 20-24 and 25-29 age categories to estimate completed fertility at 45-59 according to the formula:

$$TF = (P_3)2/P_2$$

Where: TF = Total Fertility Rate

P_3 = Mean Parity at 25-29

P_2 = Mean Parity at 20-24

The observed fertility rate is then compared to the expected total fertility rate. The test assumes that fertility has been constant in the recent past; that the age pattern of fertility conforms to the model of non-contraceptive populations; that there is no fertility bias associated with maternal survivorship; and that migration has not substantially affected reported fertility histories.

The first three assumptions are likely to be met by the data, given that Canadian Indian populations have been characterized as stable or quasi-stable (Romaniuk and Piché 1972) and noncontraceptive (Romaniuk 1974) up to 1960. However, the demographic pattern of seasonal dispersion and high geographic mobility throughout the subarctic during the contact-traditional period (Dunning 1959:63; Hallowell 1955:119; Rogers 1963:71) may preclude fulfillment of the migration assumption.

2. Sieciechowicz (1982:112) criticizes population estimates derived from treaty annuity lists. On the other hand, she also suggests (1982:210) that the administrative units represented by treaty annuity lists have come to coincide with the community structure of native groups over time.

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TABLE 1: Age-Specific and Total Fertility Rates in the Fort Hope Band: 1930 to 1944 Marriage Cohort

Age Category	Parity 0	1	2	3	4	5	6	7	8	9	10	11	12	13	Sum	ASF[1]	Mean Cumulative Parity
15-19		17	7	1											25	0.10	0.50
20-24		19	24	19	6										68	0.27	1.86
25-29		8	10	16	19	16	4	2	1						76	0.30	3.38
30-34		4	4	6	12	15	19	12	7	3	1				83	0.33	5.04
35-39			3	4	5	6	9	12	12	10	9	4			74	0.30	6.52
40-44						2	4	6	5	7	4	6	7	2	43	0.17	7.38
45-49									3	2	3	5		2	15	0.06	7.68
	2	48	48	46	42	39	36	32	28	22	17	15	7	4	384	1.54	7.68

[1]Age-Specific Fertility

Risk = 250
Observed Total Fertility Rate = 7.7
Expected Total Fertility Rate if P3 = Mean Cumulative Parity 25-30 = . 2

TABLE 2: Age-Specific and Total Fertility Rates in the Fort Hope Band: 1930 to 1937 Marriage Cohort

Age Category	Parity 0	1	2	3	4	5	6	7	8	9	10	11	12	13	Sum	ASF[1]	Mean Cumulative Parity
15-19		8	3	1											12	0.09	0.44
20-24		12	16	9	2										39	0.29	1.89
25-29		2	2	9	12	9	1	1							36	0.27	3.22
30-34		3	2	3	6	8	13	6	4	2					47	0.35	4.96
35-39			2	3	3	4	5	8	7	5	6	2			45	0.33	6.63
40-44						1	2	4	2	2	2	3	4	2	22	0.16	7.44
45-49									2	2	2	4		1	11	0.08	7.85
	2	25	25	25	23	22	21	19	15	11	10	9	4	3	212	1.57	7.85

[1]Age-Specific Fertility

Risk = 135
Observed Total Fertility Rate = 7.9
Expected Total Fertility Rate if P3 = Mean Cumulative Parity 25-30 = 6.5

TABLE 2a: Age-Specific and Total Fertility Rates in the Fort Hope Band: 1938 to 1944 Marriage Cohort

Age Category	Parity 0	1	2	3	4	5	6	7	8	9	10	11	12	13	Sum	ASF[1]	Mean Cumulative Parity
15-19		9	4												13	0.11	0.57
20-24		7	8	10	4										29	0.25	1.83
25-29		6	8	7	7	7	3	1	1						40	0.35	3.57
30-34		1	2	3	6	7	6	6	3	1	1				36	0.31	5.13
35-39			1	1	2	2	4	4	5	5	3	2			29	0.25	6.39
40-44						1	2	2	3	5	2	3	3		21	0.18	7.30
45-49									1		1	1		1	4	0.03	7.48
	0	23	23	21	19	17	15	13	13	11	7	6	3	1	172	1.50	7.48

[1]Age-Specific Fertility

Risk = 115
Observed Total Fertility Rate = 7.5
Expected Total Fertility Rate if P3 = Mean Cumulative Parity 25-30 = 8.0

TABLE 3: Infant Mortality Rates by Marriage Cohort: The Fort Hope Band, 1930 to 1944

Marriage Cohort	Number of Livebirths	Number Died < 1 Year	IMR per 1,000	G-Value	P
Full Cohort:					
1930-1944	384	33	85.9		
Cohort Subdivisions:					
1930-1937	212	8	37.7		
				14.32	< 0.001
1938-1944	172	25	145.3		

df = 1

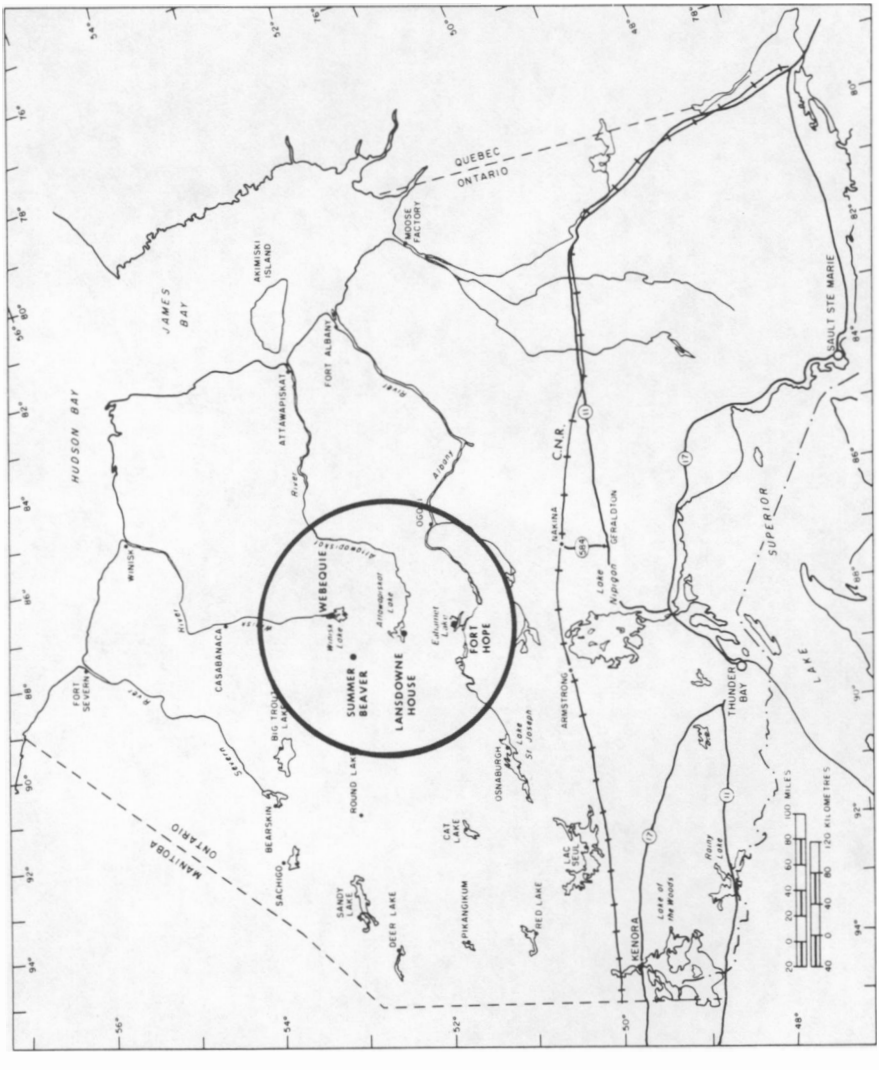
TABLE 4: Fertility in the Fort Hope Band: 1938 to 1944 Marriage Cohort Compared to the Dobe !Kung and Hutterites[1]

Population	Total Fertility Rate	Mean Age at First Birth (Years)	Mean Age at Last Birth (Years)	Mean Length of Reproductive Period (Years)	Mean Birth Space (Months)
Fort Hope Ojibwa 1938-1944 Cohort	7.5	21.5	38.6	17.1	27.4
Dobe !Kung (Howell 1979)	4.7	18.8	34.4	15.6	50.6
Hutterites (Eaton and Mayer 1953)	10.2	22.3	40.9	18.7	25.5

[1]Adapted from Roth 1981

TABLE 5: Fertility in the Fort Hope Band Compared to Other Northern Algonkian and Athapaskan Groups

Population	Total Fertility Rate	Mean Age at First Birth (Years)	Mean Age at Last Birth (Years)	Mean Length of Reproductive Period (Years)	Mean Birth Space (Months)
Fort Hope Ojibwa 1938-1944 Cohort	7.5	21.5	38.6	17.1	27.4
James Bay Cree 1968 Survey (Piché and Romaniuk 1968)	7.2	21.9	38.7	16.8	32.5
Island Lake Cree/Ojibwa 1910-1924 Cohort (Sawchuk 1972)	7.8	20.0	-	-	-
Old Crow Kutchin Post-1900 Cohort (Roth 1981)	6.6	19.8	39.0	19.2	38.9



THE FORT HOPE RESERVE, LANSDOWNE HOUSE, WEBEQUIE and SURROUNDING TERRITORY.

THE DOCTOR, THE LAWYER, AND THE MELANCHOLY WITCH

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Au cours des seizième et dix-septième siècles, un grand nombre d'individus ont été accusés, jugés, torturés et souvent mis à mort pour crime de sorcellerie. Les savants expliquent qu'une telle vogue de la sorcellerie a pu être perçue comme la conséquence du fait que les milieux professionnels, y compris la médecine, étaient incapables de donner à ce phénomène une explication "naturelle." Ce qui toutefois n'exclut pas le fait qu'on ait réussi à exprimer un point de vue différent sur des idées courantes à l'égard de la sorcellerie. En se basant sur la science médicale de son temps, le médecin Hollandais Johann Weyer (Wier) a proposé une explication logique des aveux faits volontairement par les personnes suspectées de sorcellerie. Le présent article analyse les facteurs complexes démontrant que l'acceptation de l'explication "naturelle" de Weyer posait un problème. En somme, disons que Weyer ne réussit pas à rallier les autorités à son point de vue, parce qu'il proposait une idée nouvelle à l'intérieur d'un vieux système de contraintes. Ainsi le philosophe français Jean Bodin a-t-il pu manipuler à son gré les diverses ambiguïtés et contradictions issues de la tradition médicale de Galen et, de la sorte, tirer des arguments contre ceux de Weyer.

During the sixteenth and seventeenth centuries, a great number of individuals were accused, tried, and in many cases, killed as witches. Scholars agree that the witch craze was possible because the learned professions, including medicine, could not provide a convincing "natural" explanation for witchcraft phenomena. However, dissent from the orthodox view of witchcraft did exist. The Dutch physician Johann Weyer (Wier) provided a logical explanation, based on accepted medical beliefs of the period, for the voluntary confessions of suspected witches. This paper examines a number of complex factors which made the acceptance of Weyer's "natural" explanation problematic. Stated simply, Weyer failed to convince others to accept his explanation because he presented a new idea within the constraints of an old system. This allowed the French philosopher Jean Bodin to manipulate various ambiguities and inconsistencies in the Galenic medical system, and to use these inconsistencies to counteract Weyer's argument.

INTRODUCTION

In 1486, two Dominican Inquisitors, Heinrich Kramer and Jacob Sprenger, compiled and synthesized various witch beliefs with existing notions of heresy. Their efforts resulted in the production of the *Malleus Malificarum* (Kramer and Sprenger 1970), an authoritative volume that not only identified witchcraft as a form of heresy, but also explicitly outlined how religious and secular authorities should handle the problem. Although sporadic attempts were made to control witchcraft prior to its publication, Trevor-Roper (1969:24-25) and Ben-Yehuda (1980:8-11) suggest that the *Malleus* and the *Summis Desiderates Affectibus* (1484), a Bull issued by Pope Innocent VIII authorizing Kramer and Sprenger to extirpate witchcraft, provided the basis for a more general and intensified attempt to eradicate witchcraft during the sixteenth and seventeenth centuries.

Approaching the problem from a different perspective, but nevertheless arriving at a similar conclusion, Monter 1969b:55-56) argues that the intellectual foundations for the great persecutions of 1580-1650 were firmly established because the *Malleus* received a general degree of acceptance "among the educated classes of Europe" during that period. Monter argues that none of the learned professions, including medicine, could provide a convincing "natural" explanation for witchcraft phenomena (see also Estes 1983). However, this does not mean that all intellectuals accepted the orthodox point of view. A number of prominent individuals, including the Dutch physician Johann Weyer (see Anglo 1976; Baxter 1977b; Monter 1969b; Mora 1963; Thorndike 1941; Withington 1955; Zilboorg 1967, 1969), Salazar de Frias of Spain (see Henningsen 1980), Andres de Laguna of Spain (see Friedenwald 1939), Reginald Scot of England (see Anglo 1976, 1977), Cornelius Loos of Trier in Germany (see Burr 1943), and others (see Easlea 1980; Midelfort 1972; Robbins 1977), not only disagreed with the official view of witches and witchcraft, but also engaged in active and vocal debate with other intellectuals regarding the nature and reality of witchcraft.

Early attempts to explain what was commonly believed to be supernatural phenomena in terms of natural causation can be found in the works of scholars such as Nicolas Oresme (?-1382) of France and Pietro Pompanazzi (1462-1525) of Mantua in Italy (see Anglo 1976, 1977; Thorndike 1934:398-510). For example, Pomponazzi (1948:372) states that: ". . . those possessed by demons . . . are suffering either from black bile or insanity, or from a trance or are near to death . . . whence it is that they become almost lifeless and irrational."

Within the medical field, the first major literary attack on the doctrine of witchcraft was launched by Johann Weyer (ca.

1516-1588), a Dutchman who became the personal physician of Duke William of Berg, Jülich, and Cleves in Germany. Weyer attempted to provide natural explanations for both the misfortune people attributed to witchcraft and confessions of suspected witches. He suggested that the voluntary confessions of suspected witches should not be accepted as a sign of actual guilt, but rather as a sign that the individual was suffering from delusions. Historians such as Monter (1969b), Nemec (1974), and Zilboorg (1967, 1969) agree that Weyer's ideas did not gain popular support among the intellectual classes of Europe. In fact, Weyer's ideas were severely criticized by the French philosopher Jean Bodin and other contemporary writers.

This paper will attempt to determine why the medical profession, and Weyer in particular, failed to provide a convincing natural explanation for witchcraft. To accomplish this goal, I will (1) examine the medical beliefs and practices of the period; (2) examine the criticism directed against Weyer's explanation, as well as the explanation itself; and (3) attempt to elucidate some of the factors that influenced the decision of members of the intellectual classes to reject Weyer's ideas.

I maintain that the reason courageous intellectuals such as Weyer had so little impact during the two centuries of the witch-craze was not, as generally assumed, because they were silenced. Rather, Weyer's problem was that he presented a new idea within the constraints of an old system. Medical beliefs of the period, as well as the European world view in general, were characterized by the notion of complimentarity between supernatural and natural explanations of disease causation. Weyer did not challenge this complimentarity, but merely attempted to work around it to achieve his goals. By so doing, he became easy prey for a clever ideologue such as Jean Bodin. Bodin was able to manipulate the various inconsistencies and ambiguities inherent in the Galenic medical system to give the impression that he had refuted Weyer's ideas. This, in turn, gave religious and secular authories an additional basis for rejecting Weyer's "natural" explanation.

BACKGROUND: A BRIEF REVIEW OF WITCHCRAFT LITERATURE

Although the *Malleus Malificarum* may have provided the basis for the witchcraft persecutions of 1580-1650, the importance of this document should not be overemphasized (see Cohn 1976:225; Hoak 1983; Russell 1976). Recent studies indicate that the witchcraft persecutions occurred and were maintained as a result of a number of very diverse and complex factors. These factors included:

(1) **The historical development of the witch fantasy.** For example, Norman Cohn (1976) traces the development of the negative,

antisocial image of the witch in Europe, and discusses how these beliefs became a part of the witch trials. Kieckhefer (1976) contributes to our understanding of these developments by distinguishing between folk and intellectual views of witchcraft during the fourteenth and fifteenth centuries.

(2) **The status of women.** Any study of European and British witchcraft must come to grips with the fact that women, much more often than men, were treated as witches. The following scholars explicitly address this issue: Anderson and Gordon (1978); Ben-Yehuda (1980); Ehrenreich and English (1972); Larner (1981); and Monter (1977).

(3) **The legal system.** Currie, for example, examined the differences between English and continental European legal systems, and how these systems dealt with witchcraft. He concluded that: "in both cases, witchcraft as a deviant behavior system took its character directly from the nature of the respective systems of legal control" (1968:28).

(4) **The cosmology of the period.** According to Easlea (1980), and Thomas (1971), the decline of magic and witchcraft coincided with the shift from the Aristotelian-Thomistic cosmology to that of the "mechanical" philosophy. The Aristotelian-Thomistic cosmology helped maintain magic and witchcraft beliefs because it was both "diabolicentric and geocentric" (see Easlea 1980:43).

(5) **The role of social and psychological stress.** Certain scholars (e.g., Anderson 1970; Ben-Yehuda 1980; Connor 1975; Schoeneman 1975) focus their attention on the interrelationships between witchcraft persecutions and the stressful social and cultural changes that took place in the Late Medieval/Early Renaissance and Post Reformation periods. They suggest that the witch-craze occurred in response to stress experienced at the individual and/or social level.

(6) **Witches as scapegoats.** Thomas Szasz is a leading proponent of this point of view. He maintains that: "the persecution of witches . . . is the expression of social intolerance and a search for scapegoats" (1970:112). This argument can also be found in the works of writers who focus on the "social status of women" and/or the "role of stress" (see (2) and (5) above).

(7) **Legitimation of political authority.** A number of scholars (e.g., Clark 1977; Easlea 1980; Larner 1981) stress that witchcraft persecutions served to reinforce the political philosophy of the ruling monarch or ruling class. This was particularly true in border areas where legitimacy was often at peril.

Approaching the problem of political authority from a different perspective, Marvin Harris (1974) suggests that the witch-craze was created and sustained by the governing classes to suppress peasant revolutionary movements. Witchcraft beliefs served to redirect the frustrations and tensions of peasants away from the real economic and social problems of the period. Willa Appel (1977) arrives at a similar conclusion for modern evil eye beliefs in southern Italy.

(8) **The role of "suggestion."** During the sixteenth and seventeenth centuries, certain individuals came to believe and sometimes to confess that they were witches. Ginzberg (1966), Henningsen (1980:390), and Spanos (1978:28-31) believe that this process occurred as a result of changes in the self-perception of individuals due to "suggestion." A sixteenth-century English writer named Reginald Scot (1964:29-31) arrived at a similar conclusion concerning the voluntary confessions of certain suspected witches in England.

(9) **Functional and dysfunctional aspects of witchcraft.** Many scholars continue to find Evans-Pritchard's *Witchcraft, Oracles, and Magic Among the Azande* (1937) valuable for the analysis of beliefs and behavior associated with the supernatural. Evans-Pritchard's model primarily consists of three interrelated components. He examines how Azande beliefs serve as: (a) a system of explanation; (b) a means of establishing a course of action; and (c) social control mechanisms. Both Alan Macfarlane (1970) and Keith Thomas (1970; 1971) make explicit use of one or more aspects of Evans-Pritchard's model to discuss witchcraft phenomena in England.

According to Midelfort (1972:194-196), small scale witch trials and accusations probably served a positive social function. However, Middlefort also maintains that the larger trials characteristic of continental Europe were "dysfunctional," and that the associated panics served to raise social tensions rather than to relieve them.

(10) **Nutrition and the use of hallucinogenic plants.** Caporael (1976) and Matossian (1982) suggest that ergotism, a disease caused by the ingestion of contaminated grain, was responsible for the bizarre witch craze behavior which was experienced by the residents of Salem, Massachusetts in 1692. For a critique of this point of view, see Spanos and Gottlieb (1976), and Spanos (1983).

Recently, a new nutrition-related hypothesis was proposed by Anne Zeller (1984). Zeller suggests that the symptoms experienced by certain individuals during the Salem,

Massachusetts witch scare may have occurred as a result of hypocalcemia. This argument is similar to that advanced by others to explain incidents of arctic hysteria and involuntary spirit possession (see Foulks 1972; Kehoe and Giletti 1981; Wallace 1961).

With respect to Europe, Harner (1973) notes that belladonna and other drugs which were reportedly used by suspected witches contain atrophine, a substance which can produce hallucinogenic experiences. Certain individuals may have believed themselves to be witches because they actually experienced some of the things attributed to witches while under the influence of these drugs. However, this approach is limited, and accounts for only a small portion of witchcraft cases.

By now it should be apparent that in order to understand European and British witchcraft fully, it is necessary to examine a variety of complex issues. Though it is not possible to address many of these issues in this paper explicitly, it is hoped that a discussion of the Weyer-Bodin controversy will supplement our understanding of the phenomenon.

GALENISM: A MEDICAL PHILOSOPHY

The literary works of Galen (ca. 130-200 A.D.), a Greek physician and philosopher from Perganum in Asia Minor, profoundly effected medical thinking throughout Europe, the Middle East, and North Africa. Galen's ideas were not always new; in fact, they were often based on earlier works such as the Hippocratic writings of the forth and fifth centuries B.C. (Sigerist 1933; Temkin 1973). However, Galen, was able to synthesize previous medical knowledge and contribute his own experiments, observations, and philosophical ideas. Galen's ability to compile a thorough, systematized body of knowledge enabled him to become a prominent medical authority both during and after his own lifetime.

Throughout his work, Galen consistently suggests that health depends on a condition of equilibrium. Illness occurs when internal equilibrium is disrupted. Of primary importance in this medical philosophy is the concept of the four humors and qualities attributed to these humors (see Galen 1928:201-203). The four humors are: (a) blood (hot and moist); (b) phlegm (cold and moist); (c) yellow bile (hot and dry); and (d) black bile (cold and dry). These humors are produced in the liver during digestion. Once produced, they travel as a mixture within the body, nourishing

parts and organs of similar constitution. For example, black bile nourishes the spleen, a cold and dry organ capable of transforming and excreting excess black bile (Galen 1928:205-211; see also Babb 1951:21-22 and Siegel 1968:258).

Since the four humors perform necessary functions, their production is essential for survival. Production of either an excessive or insufficient amount of one or more humors disrupts the internal equilibrium and causes physical and/or mental illness. This is particularly true of excessive black bile, a highly toxic humor (Galen 1928:205; see also Siegel 1968:220-221).

Humoral equilibrium may be disrupted by a variety of factors ranging from inappropriate diet to excessive study (see Avicenna 1930; Galen 1928; Hippocrates 1939a, 1939b).1 In order to restore natural equilibrium, it is necessary to neutralize the offending humor by stimulating the opposite reaction. For example, Galen states that:

> . . . we must cool the overheated stomach and warm the chilled one; so also we must moisten the one which has become dried up, and conversely; so too, in combinations of these conditions; if the stomach becomes at the same time warmer and drier than normally, the first principle of treatment is at once to chill and moisten it; and if it becomes colder and moister, it must be warmed and dried. . . . (1928:199)

In addition, the individual could restore natural equilibrium by decreasing the amount of the offending humor in his or her system. This could be accomplished by either blood letting, purgation, or emetics.

Although certain Galenic and Hippocratic writings were translated into Latin, the separation of the Roman Empire into Latin West and Greek East effectively isolated the Western Empire from Greek classical ideas for centuries. Meanwhile, Galenism developed and became firmly established as a medical philosophy in the East. After the conquest of Alexandria by the Arabs in 642 A.D., the Galenic system was adopted and refined by Islamic medical scholars (Temkin 1973:59-72). Translation of the medical texts of Islamic scholars into Latin by Constantine of Africa (ca. 1010-1087) and others reintroduced Greek medical and philosophical ideas to the West during the late Middle Ages (Sigerist 1933:89-92; Singer and Underwood 1962:74-77). Later, western scholars made translations of the original Greek texts.

Greek and Islamic medical/philosophical texts, including the works of Avicenna, provided the literary basis for the rise of various medical schools and universities throughout Europe. In general, these schools promoted a scholastic form of Galenism by

emphasizing medical theory rather than clinical medicine. By the mid-sixteenth century, this scholastic approach had profoundly affected medical thought. According to Cipolla:

> . . . faith in the "ancients" was blind and absolute . . . medical doctrine stood solidly on its traditional, time-honored, Galenic foundations, both unquestioned and untested, seemingly unaffected by the passing of time and ideas. . . . (1976:108)

Although the major Greek and Islamic medical philosophers had become undisputed authorities in the minds of most medical professionals, Galenism began to experience a very gradual decline during the second half of the sixteenth century. This decline continued throughout the seventeenth century. Initial criticism of the Galenic system was provided by the Swiss physician, alchemist, and chemist, Paracelsus (1493-1541) and his followers. In *The Archidoxes of Magic*, a book attributed to Paracelsus but not published during his lifetime, Paracelsus states that:

> *Aristotle, Hippocrates, Avicenna, Gallen*, and [others]. . . . grounded all their ARTS upon their own Opinions onely. And if at any time they learned any thing from Nature, they destroyed it again with their own Phantasies, Dreams, or Inventions, before they came to the end thereof; so that by them and their Followers there is nothing perfect at all to be found. (1975:B2)

The basis for Paracelsus' critical attack was the fact that many of his chemical cures could not be adequately explained within the traditional Galenic system.

The results of anatomical and physiological studies conducted by Andreas Vesalius (1514-1564), William Harvey (1578-1657), Marcello Malpighi (1628-1694), and others also demonstrated that the Galenic system was based on incomplete or faulty information (see Sigerist 1933; Temkin 1973). For example, Harvey's experimental demonstration of the circulation of the blood contradicted many of Galen's suppositions. Galenists reacted to the new information by either rejecting it outright, or maintaining that it did not affect the primary principles of the system. But as the new evidence accumulated and was corroborated by others, Galenism rapidly declined. According to Lester King (1970), the Galenic medical philosophy became obsolete by the end of the seventeenth century.

THE MELANCHOLY WITCH

The reintroduction of Hippocratic and Galenic medical ideas

into Europe during the eleventh century provided physicians with a new, more sophisticated means of explaining and treating illness. However, this medical philosophy was not the only means by which illness could be explained. People could easily implicate supernatural causes such as punishment from God, possession by the devil(s), witchcraft, or sorcery as alternative explanations. Both systems of explanation are based on authoritative sources, and both have long historical traditions. This made it difficult for one system to totally replace the other.

In *De Praestigiis Daemonum*, first published in 1563, Johann Weyer (1967, 1969) directs a concerted attack against the doctrine of witchcraft. The following excerpt from the book clearly illustrates his intention:2

> My object is chiefly of a theological nature: to set forth the artfulness of Satan according to Biblical authority and to demonstrate how one can overcome it. Next, my object is philosophical, in that I fight with natural reason against the deceptions which proceed from Satan and the mad imagination of the so-called witches. My object is also medical, in that I show that those illnesses the origin of which is attributed to witches come from natural causes. And finally, my object is legal, in that I speak of the punishment, in another than the accustomed way, of sorcerers and witches. (Weyer 1969:38-39)

Although Weyer presents a diversified attack, this paper will focus on his convictions concerning suspected witches. For a discussion of other aspects of Weyer's work, see Anglo (1976), Baxter (1977b), Walker (1958), and Zilboorg (1967, 1969).

Recognizing the fact that women accused of witchcraft confessed, often voluntarily, to various charges of performing or participating in unnatural acts, Weyer proceeds to explain how these confessions should be interpreted. First, he dismisses as invalid all confessions obtained by means of torture (see Zilboorg 1969:152-153). Second, and more important for this discussion, Weyer argues that voluntary confessions are based on false delusions experienced as a result of either a melancholic disposition, or the use of drugs such as "belladonna" (see Monter 1969b:61-62; Zilboorg 1969:142-145).3

The Melancholic Disposition

Within the Galenic medical system, the term melancholy has a variety of meanings. It can refer to the cold and dry humor known as black bile, or to a specific temperament. For example, an individual with a melancholic temperament has a natural tendency

to produce a slight excess of black bile. This tendency not only predisposes individuals to diseases caused by black bile, but also affects their personality and appearance. According to Galen, ". . . those governed by black bile are indolent, timid, ailing, and, with regard to body, swarthy and black-haired . . ." (see Klibansky et al. 1964:59). The term melancholy can also refer to a humoral disorder with psychopathological manifestations. From this point on, unless otherwise stated, the term *melancholy* will refer to this psychophysiological disease.

Galen identifies depression and anxiety as the primary psychopathological symptoms characteristic of melancholy. He also suggests that, in extreme cases, the symptoms are further complicated by hallucinatory delusions (see Jackson 1969:374-375; Siegel 1973:191-196). These effects may occur as a result of two interrelated factors:

(1) A significant excess of black bile--i.e., production of much more black bile than the spleen can transform and absorb. Excess production of black bile is influenced by a variety of factors ranging from inappropriate diet to excessive study. These factors cause an individual's constitution to become cold and dry, and thereby stimulate the production of black bile.

(2) Corruption of the humors. Production of extreme heat in the body can transform both black and yellow bile into a highly toxic substance commonly referred to as unnatural black bile (Galen 1928:211-213; see also Babb 1951:21-22; Siegel 1968:219).

In general, Galen's perception of melancholic symptoms is shared by sixteenth and early seventeenth century writers of medical treatises dealing with the subject. For example, Timothy Bright states that:

> The perturbations of melancholy are for the most parte, sadde and fearefull, and such as rise of them: as distrust, doubt, dissidence, or dispaire. . . . that melancholick humour. . . . is settled in the spleane, and with his vapours anoyeth the harte and passing up to the brayne, counterfetteth terrible obiectes to the fantasie, and . . . causeth . . . monstrous fictions . . . . (1586:102)

Almost forty years later, Robert Burton describes very similar symptoms in his *Anatomy of Melancholy* (1621):

> . . . *fear* and *sorrow* . . . according to Hippocrates' and Galen's aphorisms . . . are most assured signs, inseparable companions and characters of melancholy . . . . they are not always sad and fearful but usually so, and that without a cause. . . .(1965:134-135)

> . . . melancholics very evidently have many fantastic convictions; it would be hard to find two in all ages with the same delusion. . . . (1965:146)

The sixteenth and early seventeenth century medical perception of melancholy differs from the original Galenic view in an important way. It links melancholy, at least in certain cases, with the activity of the devil(s). This notion is found in the works of Timothy Bright (1586:237-241), Robert Burton (1965:95-99), and others (see Babb 1951:49-54). Since melancholy impairs judgement, these writers agree that the devil is attracted to melancholic individuals. The devil attempts to manipulate the victim's humors and stimulate fantastic delusions in order to lead the individual to reject God, and thereby to suffer eternal damnation.

In *De Praestigiis Daemonum*, Johann Weyer's description and discussion of melancholic symptoms is consistent with the medical views of the period. The major innovation in his work is the fact that he attempts to explain the experiences related by suspected witches in terms of melancholic delusions. Weyer suggests that the devil manipulates the humors of melancholic women, thus creating a delusion that they are performing, or participating in, an unnatural act. For the victim, the experience is so vivid that she actually believes the acts did take place and, with a little prodding, is willing to confess her involvement. In reality, the melancholic woman is merely the victim of the devil's deception. Based on this logic, Weyer concludes that suspected witches should be treated by physicians rather than mistreated by judges and jailors.

REJECTION OF WEYER'S ARGUMENT

Weyer's (1967) attack against the orthodox view of witchcraft had three primary effects. First, the Roman Catholic Church officially placed "his name on the *Index* as an *auctor primae classis*, that is, one whose opinions are so dangerous that none of his works may be read by the faithful without special permission" (Withington 1955:220-221). Second, his fellow Protestants burned copies of the book. Third, Weyer's ideas stimulated a series of

literary counterattacks which challenged each aspect of the argument. However, Weyer himself escaped punishment due to the efforts of the powerful and tolerant Duke William of Berg, Jülich, and Cleves.

In contrast to popular opinion, Weyer's ideas gained a degree of acceptance among certain British and European intellectuals (Midelfort 1972; Robbins 1977). For example:

(1) Cornelius Loos (1546-1595) of Trier, in Germany, advocated that "witchcraft was a delusion" and strongly objected to the use of torture. Unlike Weyer, he was not under the protection of a tolerant patron. Thus, Loos was imprisoned on two separate occasions and was about to be apprehended and possibly burned when he died (see Burr 1943; Robbins 1977).

(2) In the *Discoverie of Witchcraft* (1584), Reginald Scot of England (see Scott 1964) also presents an attack on the doctrine of witchcraft. Scot accepts Weyer's contention that in certain cases, both suspected witches and those who claim to be suffering from witchcraft are in reality victims of melancholy. However, Scot (1964; see also Anglo 1976, 1977) takes the argument one step further by maintaining that demons have no power in human affairs. Both Weyer and Scot's ideas were later attacked by King James I of England (1597).

(3) During their debate with John Darrel, a "Puritan Minister" from England, concerning the nature of spirit possession, Harsnett (1605), and Deacon and Walker (1601; see D.P. Walker 1981:68-73), all of whom were Anglican preachers from England, argued that the symptoms people attribute to possession often occurred as a result of melancholy. For example, Deacon and Walker stated that:

> . . . some vnskilful *Physitions*, do so rashly ascribe this *humorous disease* to the *operation of the diuell*: and that the *ignorant people* do absurdly imagine the *partie* thus *affected*, to be vndoubtedly *possessed of diuels*. (1601:160)

In the end, Weyer's convictions were rejected on philosophical, religious, medical, and legal grounds by the majority of European intellectuals.

This paper will now focus on the factors which influenced members of the European intellectual class to reject the notion that suspected witches were, in reality, suffering from melancholy.

The Weyer-Bodin Controversy

In 1580, Jean Bodin (1530-1596), a politically-minded French lawyer and social philosopher, published what was probably the most thorough refutation of Weyer's argument.4 Bodin's publication was titled *Demonomanie des Sorciers* (1580, 1969) and does not refute the notion that melancholy may produce delusions. Instead, Bodin rejects Weyer's argument by claiming that women are not prone to melancholy due to their constitution. Thus, only an ignorant (or evil) physician would attribute this condition to women. In order to support this contention, Bodin makes use of the Galenic beliefs of the period. He argues that:

(1) Production of the melancholic humor is influenced by excessive heat and dryness. Since men have a hot and dry constitution, they are susceptible to melancholy in hot and dry regions. Women, however, are naturally cold and moist; therefore their constitution is contrary to that of melancholy.

(2) According to Hippocrates and Galen, women are healthier than men. A woman's menstrual flow preserves her from various diseases because it helps eliminate excess humors, including black bile. Thus, the menstrual flow helps prevent melancholy in women.

(3) A melancholic temperament makes men "wise, serious, or contemplative"; these qualities are not compatible with women.

(4) Witches abound in cold, northern regions such as Germany and the Alps. However, northern peoples are ". . . white, green-eyed, blond-haired and slender, reddish-faced, joyous and chatter-boxes. . . ."; Their constitution is ". . . totally contrary to the melancholic humor . . . ." (Bodin 1969:50). For this reason, women accused of witchcraft in these regions should not be labeled as melancholics.

According to Sydney Anglo (1976), Bodin presents a very one-sided and selective discussion of the phenomenon. On this point, Anglo and I are in agreement. However, I would like to take this analysis one step further by contending that the Galenic medical philosophy itself was very ambiguous and inconsistent. For this reason, Bodin was able to manipulate various subtleties and inconsistencies in the system to refute Weyer's argument. Several brief examples of this point appear below.

First, within the Galenic medical tradition, heat and dryness influence the production of black bile (a cold and dry humor). However, this black bile is of the unnatural variety (see Galen 1928:211-213). On the other hand, production of natural black bile is influenced by cold and dry qualities. Following Hippocrates' lead

(1939a), Galen (1928:203; see also Siegel 1968:194) stressed that cold winds, and cold climates in general, stimulate the production of black bile in certain individuals. Since women were believed to be governed by a cold and moist constitution, they would be much more susceptible to melancholy in northern regions where the cold quality is naturally abundant.

Second, Bodin's description of the characteristics of northern peoples, and women in particular, differs sharply from the characteristics which Galen attributed to individuals with a melancholic temperament. In the Galenic tradition, a melancholic *temperament* referred to the natural constitution of certain individuals. In contrast, melancholy refers to a disease resulting from the production of a significant excess of black bile, which in extreme cases produces psychopathological effects. Although individuals with a melancholic temperament may be predisposed to melancholy, anyone, no matter what his or her natural constitution, is susceptible to the disease if the right conditions prevail.

Finally, Bodin correctly employs the Galenic medical beliefs when he suggests that the menstrual flow, like bloodletting, helps prevent melancholy by reducing the amount of black bile in the system. This argument ignores two essential facts. First, Hippocrates (1939a:77-79) states that the menstrual discharges of women exposed to cold winds "are not healthy, but are scanty and bad." Second, in his discussion of the relationship between melancholy and suspected witches, Weyer is referring to elderly women. Since the menstrual flow ceases after a woman's "change of life," it can no longer help purify her system. In 1584, Reginald Scot, a proponent of Weyer's ideas in England, explicitly linked melancholy to "menopause":

> . . . why should an old witch be thought free from . . . fantasies who . . . upon the stopping of their monethlie melancholike flux or issue of bloud, in their age must needs increase therein, as (through their weakness both of bodie and braine) the aptest person to meete with such melancholike imaginations. . . . (1964:65-66)

This aspect of Bodin's argument is, therefore, irrelevant.[5]

Based on the preceding discussion, I suggest that both Weyer and Bodin applied logical arguments to the humoral beliefs of the period. The marked differences in their opinions is possible because they refer to different aspects of the humoral theory. In this sense, the Galenic medical philosophy is ambiguous[6] enough to allow Bodin to manipulate the system, and thereby to give the impression that he has refuted Weyer's argument.

Since inconsistencies can be found in most, if not all, metaphysical systems, Bodin's ability to manipulate the system is not in itself surprising. As Edmund Leach demonstrates in his classic discussion of Kachin political behavior (1954:Chapter 9; see also Migliore 1983), myth (or tradition) is a language of argument, not a chorus of harmony. As disputants, Weyer and Bodin act like the Kachin because they use inconsistencies and ambiguities in a cultural tradition to support their arguments. Later, Weyer and Bodin themselves fell prey to direct or indirect attacks by others who manipulated the concept of melancholy. For example, when Weyer's patron, Duke William, became ill with melancholy, many people interpreted this as "a judgement upon him for his protection of Weyer and neglect of witch-burning" (Withington 1955:221). Duke William's successor made use of this interpretation to force Weyer to resign his post as court physician in 1578 (Withington 1955:221).

In contrast, Harsnett states that:

> *John Bodin* . . . who being in his younger yeeres of a most piercing, quicke, speculative wit, which grew of a light, stirring, and discursive melancholia in him, fell . . . in the midle of his age to be a pure sot . . . . For he holds that devils may transforme themselves into any shape of beasts, or similitude of men . . . and may have the act of generation with women. . . . And not only that only, but that a witch . . . may transforme herselfe into the shape of beast . . . flie in the ayre. . . . And he defends *lycanthropia*. (1605:132-133; see also Walker 1981:70)

Harsnett is using Bodin's own argument to discredit his views. More specifically, Harsnett acknowledges the value of Bodin's political and economic works by suggesting that the initial stages of melancholy made Bodin "wise, serious, and contemplative." However, since Bodin's *Demonomaine des Sorciers* (1580) was written during the later stages of the disease, this accounts for Bodin's belief in the reality of witchcraft.

What is surprising, then, is not the fact that people were able to manipulate the Galenic medical model, but rather the degree to which Bodin's argument was accepted, while Weyer's argument was generally discarded.

Reasons for the Rejection of Weyer's Argument

Reasons for the rejectin of Weyer's argument are obviously very complex issues that cannot be settled in a few pages. Various factors, including those mentioned in the above review of the witchcraft literature, must be taken into consideration in order to

understand the phenomenon. However, this paper will focus solely on a small number of factors that are directly relevant to the witchcraft/melancholy issue.

Rene Dubos (1976:319-343), a microbiologist, suggests that throughout the history of medicine, disease causation has been explained by either the "physiological" doctrine, the "ontological" doctrine, or both. The "physiological" doctrine stresses that an individual will remain healthy as long as his or her internal bodily processes function normally. Thus, disease is simply an abnormal state that occurs as a result of an internal disruption of the natural equilibrium (Dubos 1976:319). Of primary importance in this medical philosophy is the notion that disease causation is related directly to the individual's personality, bodily constitution, and mode of life. The humoral theory, as outlined in the Hippocratic and Galenic writings, is a primary example of a "physiological" doctrine.

In contrast, the "ontological" doctrine stresses that disease occurs as a result of damage caused by an external agent that either acts upon, or enters into, the human body. Mary Douglas's discussion of witchcraft provides an excellent example of the "ontological" doctrine. Douglas maintains that the dominant symbols found in witchcraft beliefs consistently ". . . build on the theme of vulnerable internal goodness attacked by external power. . . ." (Douglas 1970:xxvi). Witches are malevolent external agents capable of injuring unsuspecting others. Although they may be physical members of the society, witches are the direct embodiment of everything despised by the society (Douglas 1973:138-140).

This negative image of the witch is consistent with Norman Cohn's discussion of the European stereotype of the witch (1976:99-102). People regarded witches as normal members of the society who, by making a pact with the devil (a highly malevolent, external agent), became associated with a secret society that engaged in cannibalism, exotic orgies, and other diabolical rites. By participating in these activities, the witch effectively rejects the values of larger society. In addition, since both the devil and the witch are capable of causing misfortune by occult means, the witch becomes a potential source of suffering for members of the larger society.

Throughout the fifteenth to seventeenth centuries, both "physiological" and "ontological" belief systems were prevalent in Europe. In general, physicians based their medical decisions on the humoral theory. Occasionally, humoral principles failed to provide an adequate explanation of the nature of certain diseases. In these cases, the physician could move from the "physiological" to the "ontological" belief system to provide an acceptable explanation. An example of this movement from one system to the other can be

found in the literary works of the Italian physician Antonio Benivieni (1443-1502). Benivieni describes an ailment that he initially attempted to treat by means of the humoral theory. His treatment failed and the symptoms became more severe and very difficult to explain:

> . . . she grew more frenzied and, glaring round with wild eyes, was at last violently sick and vomited up long bent nails and brass pins, together with wax and hair mixed in a ball. . . . I saw her go through exactly the same procedure many times. . . . (1954:35-37)

At this point, Benivieni reinterprets the nature of the ailment, and suggests that the woman is possessed by an evil spirit.

The following excerpt from *The Triall of Witch-Craft* by the English physician, John Cotta, indicates that well over one hundred years later, it was still fashionable for physicians to work within both physiological and ontological explanatory systems:

> . . . when naturall remedies or meanes according unto Art and due discretion applyed . . . either lose their manifest inevitable nature, use, and operation, or else produce effects and consequences, against or above their nature. . . . doth certainly prove an infallibilitie of a superiour nature, which assuredly therefore must needs be either Divine or Diabolicall. (1616:70)

During this time period, the "physiological" and "ontological" doctrines were superimposed over one another and served as complimentary rather than conflicting systems of explanation. Johann Weyer does not challenge this complimentarity, but instead manipulates the grey area between the two systems to draw attention away from suspected witches and cast blame directly on the devil(s). This innovation attacks the specifics of the belief system(s), but leaves the main tenets intact. More specifically, Weyer does not challenge the notion that demons exist, nor does he challenge the notion that they are capable of affecting and/or interacting with human beings. As long as this ideological basis for belief in witchcraft remained intact, it was unlikely that members of the intellectual class could accept Johann Weyer's convictions (see Anglo 1976; Easlea 1980; Thomas 1971).

Throughout *De Praestigii Daemonum*, Johann Weyer (1967, 1969) attacks the "learned" conception of the witch. He insists that suspected witches do not: (1) make voluntary pacts with the devil; (2) have sexual intercourse with demons; (3) injure their neighbors

by occult means, etc. In Weyer's view, suspected witches are victims of a melancholic disease that is further aggravated by the activity of the devil. More specifically, the devil is capable of manipulating the imagination of melancholic old women.

The image of the witch that Weyer creates not only differs significantly from the view held by contemporary religious and secular authorities, but also generates certain implications. As victims of a psychopathological disease, suspected witches should not be held responsible for what they say or do. Therefore, they would not fall within the jurisdiction of lawyers, judges, and inquisitors. Melancholic individuals should be treated by competent physicians capable of easing their symptoms.

Weyer's conception of the witch also has implications for the "self-image" of both secular and religious authorities (i.e., ". . . that organization of qualities that the individual attributes to himself. . . ." (Kinch 1972:246; see also Blumer 1969). For these officials, the persecution of witches was not only justifiable, but also a duty. Various authoritative sources, ranging from the Bible to the *Malleus*, stress this point. For example, in his Bull to the Lombards in 1501, Pope Alexander VI explicitly states that:

> . . . we send to you, commit to you, and order you and your successors . . . that you may seek out diligently those people of both sexes . . . and secure and punish them through the medium of justice. . . . (see Kors and Peters 1972:190-191)

Thus, the persecution and torture of suspected witches did not adversely affect the "self-image" of various officials.

I suggest that Weyer's argument not only promoted a new concept of the witch, but also attacked the self-image of religious and secular officials. Acceptance of Weyer's argument would have transformed the positive self-image of these officials into a negative self-image. Since religious and secular officials would no longer have had a basis for justifying their actions, they would have had to reinterpret their previous actions, and to attempt to handle the psychic stress of knowing that they had tried and punished innocent people. For example, John Darrel attacks certain British writers specifically on these grounds. He states that:

> They doe not indeede deliver this in plaine tearmes, least happely they should thereby irritate the reverend Judges of the land, by making them guilty of shedding much innocent blood.7 (1602 *In* Walker 1981:71)

From the point of view of these officials, a negative "self-image" was simply not acceptable. I contend that this factor contributed to

the rejection of Weyer's argument. More specifically, the existence of alternative explanations allowed officials the luxury of choice. That is, they could choose the alternative that did the least damage to their self-image.

As mentioned previously, Jean Bodin (1580; 1969) argued that women were not melancholic. Galen did not share this view, as the following excerpt from his *On Prognosis* clearly illustrates:

> I was called in to see a woman who was stated to be sleepless at night and to lie tossing about. . . . Finding . . . no fever, I made a detailed inquiry into everything that had happened to her. . . . After leaving I came to the conclusion that she was suffering from one of two things: either from a melancholy dependent on black bile, or else trouble about something she was unwilling to confess. . . . After I . . . diagnosed that there was no bodily trouble, and that the woman was suffering from some mental uneasiness. . . . (1972:213)

Although this particular case did not warrant a diagnosis of melancholy, it demonstrates that Galen did not consider this ailment to be solely a "male" disease.

During the sixteenth and seventeenth centuries, the term melancholy had a variety of meanings. Within the Galenic medical tradition, the term could refer to a humor, a temperament, or a psychopathological disease. In addition to this Galenic view, there existed a separate philosophical discussion of melancholy. Aristotle, in his discussion of Problem XXX, Number I (1957; see also Klibansky et al. 1964:18-29) explicitly suggests that all eminent individuals are melancholic. Out of this tradition developed the notion that genius and melancholy are related. Later, Islamic scholars linked this notion to astrological beliefs, and melancholy became associated with the planet Saturn (Klibansky et al. 1964:127; Wittkower and Wittkower 1963:102-103).

Since the Aristotelian description of melancholy was very flattering, the malady became fashionable among painters, poets, and scholars in general throughout Europe and England (Babb 1951; Wittkower and Wittkower 1963). Even though painters and poets were predominantly male, individuals consistently attributed their mental state to melancholy. Saturn, which had become synonymous with melancholy, was also classified as masculine (Klibansky et al. 1964:131). At the same time, certain texts depicted women in a very negative light. For example, in the *Malleus Malificarum* (1486), Kramer and Sprenger state that:

> . . . since they are feebler both of mind and body, it is not surprising that they should come more under the spell of witchcraft. For as regards intellect or the understanding of spiritual things, they seem to be of a different nature from men. . . . Women are intellectually like children. . . . (1970:44)

I believe that these factors influenced Bodin and others to reject the notion that women, and witches in particular, were melancholic.

A notable exception was Saint Teresa of Jesus (1515-1582). In her *Book of the Foundations*, Saint Teresa (1964:36-40) stresses that certain women may experience melancholic delusions due to the hardships of convent life. She asks that these women be treated carefully, and not mistreated by confessors. According to Sarbin and Juhasz (1967), her efforts had some effect in Spain. However, her ideas were not disseminated throughout Europe during her lifetime.

Finally, Weyer's broad generalization may have contributed to the rejection of his own theory. Weyer was not satisfied to suggest that suspected witches should be examined by physicians in order to ensure that some of them were not actually melancholic. Instead, he suggested that all suspected witches who confessed voluntarily were suffering from delusions caused by either a melancholic disposition, or by the use of specific drugs. This view was not only unacceptable, but simply untrue.

A physician named Erastus who was Weyer's contemporary attacked Weyer's "natural" explanation precisely on the grounds that it was much too general (see Monter 1969b:64). Erastus pointed out that not all witches could be classified as melancholics, nor were they all women. More importantly for purposes of this discussion, Erastus rejects Weyer's argument because melancholics rarely share the same delusions, whereas witches consistently described the same, or very similar, experiences.

CONCLUSION

During the second half of the sixteenth century, Johann Weyer and Jean Bodin became involved as the main protagonists in a major intellectual debate. Weyer began the argument by refuting many of the contentions found in the *Malleus Maleficarum* which had been compiled by the Dominican Inquisitors Heinrich Kramer and Jacob Sprenger in 1486, and by suggesting that individuals who voluntarily confessed to being witches should be regarded as victims of melancholy. To support this argument, Weyer relied primarily on the Galenic medical system. Bodin countered by manipulating various subtleties and inconsistencies in the Galenic

system to give the impression that he had refuted Weyer's argument. More specifically, Bodin consciously or unconsciously made use of such ambiguities as the fact that: (1) the term melancholy had a variety of meanings during this time period; (2) the etiology of melancholy (the psychophysiological disease) could be explained in terms of either black bile or unnatural black bile; and (3) there was a juxtaposition of Galenic and Aristotelian views of melancholy.

Bodin's ability to selectively manipulate the Galenic system, and thereby to advance his own beliefs, does not by itself explain why Weyer's argument failed to gain credence among the intellectual classes of Europe at the time. I contend that in combination with a variety of other factors, Bodin's approach had a significant effect.

In summary, a variety of factors combined to neutralize Weyer's contention. Explained simply, Weyer failed because he presented a new idea within the constraints of an old system. Weyer provided natural explanations for both the phenomena people attributed to witches and the confessions of suspected witches. Yet he never questioned the notion that demons exist, and that they can affect and interact with human beings. Although his innovation did not challenge the belief that the devil played a role in human affairs, his ideas threatened the security of old beliefs. This threat then stimulated counterarguments. In this sense, as a proponent of new ideas, Weyer found himself engaged in an intellectual argument with traditional forces. At the same time, his innovation did not gain support among individuals who rejected many of the traditional beliefs. Weyer's argument followed the humoral beliefs of the period too closely to be accepted by "progressive" physicians. Thus, his explanation fell short because it was too innovative for "traditionalists," and too traditional for "progressives."

NOTES

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1. Avicenna (980-1037) is the Persian medical writer and practitioner who composed the famous *Canon of Medicine*. Although innovative in its own right, this work is based primarily on the Galenic medical system.

2. Johann Weyer's (Wier) work has not as yet been translated into English. For this reason, I will rely on the 1969 partial translation of his work for quotation.

3. As mentioned previously, the Harner (1973) article indicates that these drugs can produce hallucinogenic experiences.

4. For a discussion of Bodin's work on magic and witchcraft, see Anglo (1976), Baxter (1977a), Monter (1969a; 1969b), and Pearl (1982). Julian Franklin (1964) also presents an interesting discussion of Bodin's other works.

5. My views here are consistent with Anglo's discussion of the matter (1976:215-216).

6. In a recent article, Carol Laderman (1981:470) suggests that humoral systems may be inherently ambiguous, and thereby "subject to alteration and interpretation." This view is consistent with my findings.

7. Walker (1981:81-84) acknowledges that Darrel's insinuation was justified, and suggests that it was only when King James personally stepped in that officials began to reexamine the facts in witchcraft/possession cases (see also Kittredge 1929:319-328).

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EARLY NISHGA-EUROPEAN CONTACT TO 1860: A PEOPLE FOR "THOSE WHO TALK OF THE EFFICIENCY OF MORAL LECTURES TO SUBDUE THE OBDURACY OF THE HEART"

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Cet article présente une vue d'ensemble des rapports Nishga-Européens jusqu'à 1860. Les Nishga apparaissent dans les premiers récits comme un peuple énergique et intelligent, capable de prendre en main ses propres affaires. Les Européens ont qualifié d'arrogants et hautains leur comportement et leur attitude. Au cours du XIXe siècle, une stratégie vigoureuse, inspirée par les chefs Nishga et adaptée aux circonstances, a constitué un tremplin pour faciliter le changement culturel et a caractérisé les nouvelles relations entre ce peuple et certains représentants de la culture européenne et Euro-canadienne.

This article presents a survey of Nishga-European contact to 1860. The Nishga appear in early accounts as an energetic, intelligent people who were in charge of their own affairs. This independent behavior and attitude struck some Europeans as arrogant and aloof. The vigorous adaptive strategy of Nishga chiefs provided a basis for culture change with continuity throughout the nineteenth century, and also characterized their relationships with those segments of European and Euro-Canadian culture with which they came in contact.

INTRODUCTION

The Nishga Indians of the Nass River valley, British Columbia became the focus of national attention in the early 1970s for their role in the British Columbia Indian land protest movement. The Nishga took a leading part in asserting the validity of the land claims of British Columbia Indians by pressing their rights to ownership of the Nass River valley through both the provincial courts and the Supreme Court of Canada. Although the decision of the Supreme Court of Canada in 1973 did not accept their claims, it enhanced the Nishga case and encouraged Indian land claims in general (Berger 1981:219-254). Much less is known, however, of the early history of the Nishga. As one of four Tsimshian peoples (Coast Tsimshian, Southern Tsimshian, Gitksan, and Nishga; see

Seguin, ed. 1984:ix), description of Nishga culture is often generally subsumed under that of the Tsimshian.

In the late nineteenth century, missionaries, especially the Anglican missionary James E. McCullagh, gathered information about early Nishga history. Systematic study by anthropologists began in the 1890s with Franz Boas. Nevertheless, the Nishga have not been as thoroughly studied as the Coast Tsimshian. This may be partly the result of the accessibility of the Coast Tsimshian and interest aroused in them by another Anglican missionary, William Duncan. Duncan's Christian village of Metlakatla, founded in 1862, drew the attention of church officials, government leaders, and travelers. Metlakatla gathered most of its adherents from among the Coast Tsimshian, many of whom settled at the Hudson's Bay Company post, Fort Simpson, after its relocation in 1834. On invitation from the Nishga, Duncan was the first missionary to visit villages on the Nass River when he traveled there in April and September of 1860.

Continuous and relatively intensive contact between non-Indians and the Nishga began in 1864 with the arrival of the first resident missionary on the Nass River, the Reverend Robert R.A. Doolan. Doolan was a deacon of the (Anglican) Church of England and an agent of the Church Missionary Society. He evangelized the Nass River Nishga intermittently between 1864 and 1867 (Patterson 1981:337-344). Prior to Doolan's tour, not many non-Indians visited the Nass River and written sources for this contact are meager. The present essay is an attempt to reconstruct the broad outlines of the history of Nishga contact with non-Indians prior to the 1860s.

CONTACT AND TRADE BEFORE THE EUROPEANS

Although Tsimshian traditions tell of the migration of peoples from their various homeland locations to the Nass and Skeena Rivers, the general picture of Tsimshian cultural development derived from archaeological sources reveals a long, continued growth without sharp breaks or intrusions that would signify the introduction of major foreign influences. Archaeologists describe three horizons of culture in the Coastal Tsimshian area. The first of these horizons extends from approximately 2,000 to 500 B.C.; the second from about 500 B.C. to 500 A.D.; and the third from 500 to 1,800 A.D. (MacDonald 1970:240-254).

Archaeological artifacts indicate continuity throughout all three of these horizons and with historic Tsimshian artifacts. Geometric decorations are also found in all three horizons. In the second horizon, there is evidence of the development of woodworking, a characteristic feature of Northwest Coast Indian culture. Stone carving was also present at that time. Another

"classical" trait was the importance of marine foods in the Tsimshian diet. By the beginning of the third horizon, artifacts showing zoomorphic forms appeared. Similar findings apply to the Haida and Tlingit. This evidence supports the conclusion that there was a long tradition of sharing, interchange, and borrowing among these three Northwest Coast Indian peoples. This long tradition predated the cultural similarities noted in historic times.

Although both types of contact probably took place, myth and legend emphasize conflict rather than peaceful contact among the Tsimshian, Haida, and Tlingit. Often, kidnapped young women or their children returned "home" to their own place, where they became the progenitors of prominent leaders. Marriages took place between Haida and Tsimshian, and invading groups dispersed other peoples. After dispersing from their homeland of Temlaham on the upper Skeena River, the Tsimshian expanded southward against the Kwakiutl, to the northwest against the Tlingit, and into the interior against the Athapaskans. These movements represented local migrations of Tsimshian, Haida, or Tlingit and did not contradict the picture of the shared common culture of the three peoples.

Among scholars in the late nineteenth and early twentieth centuries, migration was a popular notion and some exaggeration of the importance of family narratives of movement may have occurred (Duff 1964). The events of the history of some Northwest Coast Indian families came to stand for the events of the ancestors of the entire population. Movements of people were associated with floods, volcanic eruptions, and mud slides as well as with kidnapping, adventure, raiding, and trade. New communities were created by hiving off from parent communities.

Movements of both individuals and groups continued throughout the nineteenth and twentieth centuries and were observed by non-Indians. Tsetsauts and Kitwancools settled on the Nass River. People moved from a flooded village site to a new site. Whether those dramatic movements ever involved major portions of the population is questionable. Rather, gradual change and small scale movement, mostly of nearby and culturally similar or identical people, seems to have been the pattern. In some cases, as with Tsetsaut or Carriers, assimilation to Nishga or Tsimshian culture took place.

The Nishga, and the Tsimshian generally, are divided into four exogamous clans: Eagle, Killer Whale, Raven, and Wolf. These clans, with their respective crests, each have subdivisions. By tradition, the Wolf and Eagle crests are the most recent arrivals in the Nass valley (Barbeau 1950:27-29). People of the Wolf and Eagle crests consider themselves to be immigrants from the Haida, Tlingit, and Tahltan (an interior Athapaskan people). These two crests are

linked as allies and commonly intermarry (Halpin 1973:62-63).

The Nishga were famous as aggressive traders and merchants. Their chiefs took the initiative in securing and maintaining the flow of trade goods and exercised control over innovations in the culture as well as over selective borrowing during contact with Europeans--all of which were important aspects of the later missionary phase of Nishga history. Among the late nineteenth century Tlingit, Wolf chiefs who illustrated this leadership role were Shakes (a Stikine from Fort Wrangell), Toyaatt (also from Fort Wrangell and one of the first Christian converts there), and Shotridge (a Chilkat from Klukwan). These men may be compared to Kinsada, Kadounaha, Claytha, Qwockshow, and Skoten among the Nishga in the last decades of the nineteenth century in their exertion of social, economic, ceremonial, political, and religious leadership. At Kuiu, Alaska, the Tlingit Wolf crest people were called "People of the Nass," further evidence of Nishga-Tlingit Wolf crest links (Swanton 1970:399). Tlingit legends state that several Tlingit Wolf and Raven clans were derived from the Tsimshian area (Swanton 1970:414). This agreed with traditional Tlingit and Tsimshian views that the Tlingit moved north as the Tsimshian expanded to the coast and then northward (Swanton 1970:476).

Tsimshian oral history tells of a migration from the interior village of Temlaham, homeland of the Tsimshian, and also from Tahltan. The latter group were members of the Wolf crest. Other Tsimshian are said to be derived from the Tlingit and Kwakiutl. Still another Nishga tradition tells of an Eagle crest clan of Tlingit origin which moved from Prince of Wales Island to the Nass River and assimilated with the Nishga (Drucker 1955:114). These were movements of small groups whose history has been merged with that of the general population.

Nishga tradition tells of a pre-flood era when they occupied a site about twenty miles from the site of the village of Gitlakdamiks. The people camped on the highest mountain nearby and waited for the flood to recede. Some Tlingit, fleeing the inundation, stayed on with the Nishga (Shotridge 1919:55ff.).

Following the eruption of a volcano in the upper Nass River in approximately 1770, two villages were founded and were named Gitlakdamiks and Gitwinsilth. According to tradition, prior to the volcanic eruption, the Nishga in the upper Nass valley had resided in one village. The eruption, which was regarded as divine punishment for the mistreatment of salmon by children, changed the course of the river and created a vast lava bed on the broad floor of the upper valley (Collison 1915:270-271; Barbeau 1950:28-29).

The Nass River takes its name from a Tlingit word meaning "stomach" or "food depot," a reference to its rich supply of food,

especially at its mouth or estuary which empties into Observatory Inlet (Walbran 1972:352; Binns 1967:245). This explanation was given by Captain Aemilius Simpson to Peter Skene Ogden, both officers in the Hudson's Bay Company at the time of the founding of the Company post at the mouth of the Nass River in 1831. Of these resources, the most important for both food and trade were eulachon (candlefish) and eulachon oil. These commodities were in demand by the Nishga and their neighbors on both the islands and on the mainland. Catching and processing eulachon was a major annual economic and social event. Surrounding peoples came to the fishery at the mouth of the Nass River (the Red Bluff area) each spring. Trade routes with interior hunting peoples (e.g., the "grease trail") carried fish, oil, and other trade items from the coast to exchange for hides, horn, and meat with people from the interior. Since these trading arrangements predated the the European fur trade, the latter may be said to have grafted itself onto the former.

A first fruits ceremony marked the spring arrival of the first run of eulachon, which was sometimes called the "savior" fish for its timeliness in providing a replenishment of food after the depletion of stores through the winter months. By March, people were living on dried berries and seaweed flavored with last year's eulachon oil. The people gathered at the fishery and called out to the fish, "You are all chiefs," to salute and honor their arrival. The eulachon were the first fish to return and were followed by others. Killer whales, porpoises, sea lions, seals, and sea birds were attracted to the area by the abundance of food. Shellfish were also available. In the spring before the snow was off the ground, people began to arrive at the fishery from up river on the Nass and the upper Skeena Rivers. The coastal Tsimshian also came, and canoes arrived from the Queen Charlotte Islands and Prince of Wales Island carrying Haidas. The Haida were trading partners and occasional enemies of the Nishga. Haida canoes were traded, and children and slaves were sometimes sold at this annual spring trade mart (Binns 1967:246ff.).

NISHGA CONTACT WITH EUROPEANS IN THE MARITIME TRADE PERIOD

By the early eighteenth century, the arrival of Russian trade goods to the north Pacific coast resulted in "widespread destabilization." Haida, Tlingit, and Tsimshian shifted northward in an effort to gain greater access to new trade goods from Siberia. Conflict resulted from one group pressing against another. Interior peoples such as the Chilcotin, Carrier, and Tsetsaut were also drawn into this "struggle over trade." Control of forts and trade routes were crucial to trade success, although contests over forts and trade routes were not primarily over territory as such (MacDonald 1984:79-80).

The earliest Nishga contact with European trade goods probably occurred through the Tlingit who traded and fought with Russian traders entering their territory by the mid-eighteenth century. The Tlingit were active traders at Sitka, Alaska, and were linked to people along the Nass River. By the 1780s, the Haida may also have initiated indirect contact with European trade goods, as they were a major trading partner of the Nishga and Coast Tsimshian, and continued to have this relationship with the Nishga and Coast Tsimshian into the nineteenth century. Haida, Tlingit, and Tsimshian recognized kinship with each other, and were often bilingual (Kraus 1956:209-214).

By the last quarter of the eighteenth century, ships from Russia, Spain, Britain, and the United States came to the Northwest Coast to explore and trade. By the 1780s, trade in sea otter pelts had become profitable, and ships arrived in late spring and summer to exchange manufactured goods (especially metal products) for sea otter pelts. In addition to a wide variety of Europeans who manned these ships, Chinese, Polynesians, Blacks, and North American Indians, including Iroquois and Mexican Indians, came to the Northwest Coast (Quimby 1945:250-251).

The Europeans introduced epidemic diseases for which the Northwest Coast Indians had neither efficacious indigenous medicine nor immunity. By the 1770s, smallpox had spread among the Tlingit and to their neighbors, including the Nishga. Jonathan S. Green, an American missionary visiting the mouth of the Nass River in 1829, reported that elderly informants told of an epidemic many years earlier (Green 1915:39ff., 64). Other factors linked to the European fur trade also contributed to a population decline, including accelerated intertribal warfare and excessive use of alcohol.

The Haida grew potatoes, and by the late 1780s were trading potatoes to European ships (Van der Brink 1974:34). In 1787, Captain George Dixon traded in Tsimshian waters and northward (Gough 1980:70). Captain Charles Duncan of the *Princess Royal* traded with Tsimshian at the mouth of the Skeena River in 1788. The Nishga were probably introduced to European foods by the late eighteenth century.

The Nishga met Captain George Vancouver in July, 1793 during his first voyage to the Northwest Coast in search of a northwest passage. Vancouver had been told of an "extensive inland navigation" by Captain William Brown, whom he met while in the area. This place, Brown had been told, was called Ewen Naas (large food depot). Though Vancouver traveled near the mouth of the Nass River, he did not find the mouth, and his expedition also failed to locate the mouth of the Skeena River. Vancouver's expedition met Indians in the waters of Alice Arm, Hastings Arm, Observatory

Inlet, and the southern end of Portland Canal. In the late nineteenth century, sites were claimed by Nishga chiefs at various places in these areas, and today all of these areas have Nishga reserves. The Nishga hold that these have been their territories from time immemorial. Tongass Tlingit lived at the mouth of Portland Canal, and some of the other Indians encountered by Captain Vancouver and his crew may also have been Tlingit.

Generally, the Indians whom Vancouver met were at first cautious and probing, but became more friendly after the initial meeting. Vancouver suspected their reticence may have been a result of Brown having fired on people in the Port Essington area with "resultant slaughter." Such an incident would have been quickly reported to neighboring peoples.

Although the pelts offered to Vancouver by the Nishga were not of high quality, trading did occur and the Indians were especially interested in acquiring blue cloth. In the previous year, the Spanish navigator, Caamano, met a Tsimshian chief who was dressed in blue calico with a floral pattern (Gunther 1972:107-108).

The only serious hostility on record took place in the Tlingit waters of Behm Canal, Alaska. During this clash, a man identified as a leader donned a mask representing a Wolf (Gunther 1972:166).

Although Captain Vancouver was invited twice by Indians, presumably Nishgas, to visit their settlements, he refused on grounds of the urgency of his main business, exploration. Thus, no occupied native villages were seen or visited in these Nishga waters. Perhaps the Indians interpreted this refusal to visit as a sign of unfriendliness or fear of attack. It is likely that they understood Vancouver's ships were not primarily interested in trading. Whether the settlements to which Vancouver was invited were temporary encampments or permanent winter villages is not clear. However, it is likely they were the former since the Nishga have traditionally and historically maintained permanent settlements on the Nass River at some distance from their far-flung fishing stations.

The first group of Nishga to contact Vancouver did so on Observatory Inlet on July 22, 1793. These people were unable or unwilling to help Vancouver locate Ewen Naas, the "extensive inland navigation" about which he had been told, and which he hoped would lead to a northwest passage. Another group sold salmon to Vancouver in exchange for mirrors and other items. On July 24, 1793, a larger party of Indians contacted Vancouver on Alice Arm. At first they were cautious about approaching the Europeans, but the atmosphere improved after an exchange of gifts. The Indian men were armed with iron daggers--an indication of direct or indirect contact--which were hung in leather sheaths from

their necks and wrists. The leaders of the party dined with Vancouver and his officers, and although each side was unfamiliar with the other's language, some communication took place through gestures and Haida words, including the first invitation to a village.

Three days later, on July 27, 1793 on Portland Canal, a party of fifteen Indians encountered Vancouver. Again, the initial contact was cautious and tentative. The men's faces were painted red, white, and black, which gave them a ferocious and horrifying appearance as far as Vancouver and his crew were concerned. This confirmed Vancouver's former opinion that these were an unusually warlike people. Vancouver also commented negatively on the appearance of a high ranking woman due to her lip labret.

Although Captain Vancouver's initial effort at friendly contact was spurned at the instigation of the party's leader, a warmer interchange was created when trade items, which were at first rejected, were accepted. Vancouver then declined a second invitation to visit their village. As trading proceeded, the encounter became still more friendly. "They began a song that continued until they came close to us, when I observed that their arms and war garments were all laid aside . . ." Like the other Indians, these men had iron daggers around their necks and their spears had iron or bone points. They wore breastplates of hide in several thicknesses with wooden slats. This suggests that they were on an expedition in which they anticipated hostility from some quarter.

Again on July 31, 1793, three Indians met Vancouver early in the day. Later, they reappeared with others and trading ensued. Vancouver and his crew were suspicious of attack, and refused to trade requested firearms and ammunition. Finally, the Indians departed issuing what Vancouver took to be insults. On this occasion, a woman was the leader of the party and the disposition of spears in the canoe for defence or attack was noted (Vancouver 1967(2):338-339).

All of these contacts create an impression of a people who were suspicious of Europeans and on their guard to defend themselves. They were willing to trade, but less so than others on the coast, and they offered only poor or mediocre pelts. These summer contacts did not reveal much about the life of the Indians or their culture. Vancouver nevertheless indicated that they were somehow different in demeanor and spirit from the other Indians he encountered.

These people, arguably Nishga, met the Europeans as potential traders or potential attackers and were prepared to respond accordingly. Directly or indirectly, they had a knowledge of trading with non-Indians. Probably further cultural traits are illustrated by

these contacts, especially those involving the etiquette of trading and raiding. Captain James Colnett met Tsimshian Indians in 1788 and noted that they were not accustomed to the maritime fur trade, and that contact with them was not satisfactory (Gunther 1972:92). Perhaps the same can be said of Vancouver's encounters with the Nishga in 1793.

Nishga-European contact became more frequent during the decades at the turn of the nineteenth century. In 1799, Russian traders established a trading post at Sitka, Alaska, and the post was reestablished in 1804 after suppression of a Tlingit uprising in the area. The Sitkans had firearms acquired from American traders. Some became trading partners of the Russians, but others resisted what they may have perceived as a threat to their trading position (Tollefson 1976:258). Lisiansky, who participated in the retaking of Sitka, observed that "rich" families among the Sitka Tlingit used metal household utensils (1814:243). In 1832, Sitka became the capital of Russian Alaska. Russian orthodox clergy were already present prior to that date, and the bishop's see or headquarters was established there in 1834.

Jonathan Green, a missionary of the American Board of Commissioners for Foreign Missions in Hawaii, toured the Northwest Coast in 1829 to assess its potential for American missionaries. The impressions he gained were probably influenced by conversations with two Russian clergy at Sitka. One of these clergy, a veteran of twelve years, told him that prospects for converting the Indians were not good. Green then reported that the time was not ripe for evangelizing. He also made several observations about the Nishga in particular, whom he visited once in early April and again in late May 1829, almost certainly at their fishery at the mouth of the Nass River. The Nishga were traders with interior Indians and had monopolized the coastal trade of these people because of superior weaponry. There were about 5,500 Nishga, although reports indicated that their population had been larger. Smallpox ("Tom Dyer" disease), war, intemperance, and infanticide were the reasons given by Green for this decrease in population. Green found the Nishga language (a Penutian language) "harsh and disagreeable"; the trade language they used was "Skidegate Haida" (presumably an Athapaskan language). On visiting the site of the Nass River fishery in April 1829, Green noted that the Hudson's Bay Company planned to build a post there (1915:61-62).

In general, Green's remarks suggest that he spent most of his time at the fishery rather than at any of the winter villages, although he made a few observations about Nishga material and social culture which would imply at least one or two visits. "They occasionally build a decent home, and erect before it a mast or log of wood of great size carved and painted fantastically." The completion of a house was celebrated with a great feast and gift

giving which bestowed honor on the giver. The Nishga took most of their slaves from more southerly peoples. They grew potatoes to sell to visiting ships. Green commented that in 1828, ships had left pigs so that the Nishga might also start raising them to sell to non-Indians (1915:51). Green believed that in most ways, non-Indian influence was "about 100% harmful," an opinion which was passed on to him by Captain Aemilius Simpson of the Hudson's Bay Company. The Nishga, Green concluded, were a powerful tribe, intelligent, fierce, and warlike. Green held some conversations with Nishga chiefs through Tlingit interpreters, and from the Tlingit gained some knowledge of the Nishga concept of God and reincarnation (1915:63ff.).

Green's comments regarding his second visit in May 1829 were briefer than his earlier ones, and stressed the aggressive trading traits attributed to the Nishga. "Here is a people," he wrote, "whom I would recommend to the attention of those who talk of the efficiency of moral lectures to subdue the obduracy of the heart" (1915:80). He noted Nishga pride, and shed light on Nishga-Haida rivalry, noting that the Nishga had acquired firearms from traders and used their military technology against the Kaigani Haida with deadly effect. Oral tradition, gathered in the late nineteenth century, also tells of war between Nishga and Tsimshian in the early nineteenth century. This was resolved by appointing champions from the two camps. Such restrained conflict may have reflected the closer ties which existed between the Nishga and Tsimshian, and the desire for more peaceful mechanisms to deal with friction.

By the 1820s and 1830s, European religious influences were probably beginning to be felt among the Nishga. Christian missionary influences penetrated perhaps from Alaska, and certainly from the interior. The Reverend Innokenti Veniaminoff, a Russian Orthodox priest who later became a bishop, took up residence at Sitka in the mid-1830s from 1834 to 1838. Later, a school and seminary were opened there. Veniaminoff knew of the Nishga, but only of the Wolf crest. This may be a reflection of the role of the Nishga as traders and merchants and their links to the Tlingit (Kraus 1956:75).

Meanwhile, Roman Catholic missionaries penetrating from the east were evangelizing as far west as the plateau area. This mission influence resulted in the career of the prophet Beni (Pene), a Carrier Indian from the Bulkley River who was active from about the 1820s to the late nineteenth century. Beni's teachings spread among the plateau peoples and on to the coastal peoples and the Queen Charlotte Islands. The Tsimshian and Nishga were influenced as his ideas spread down the Skeena and Nass Rivers. From the Nishga and the Tsimshian, this influence spread north to the Tlingit and southward to the Kwakiutl (Spier 1935:39; Jenness

1943:551-559).

Beni taught the coming of a new world order, when slaves would become chiefs and chiefs would become common men (McCullagh n.d.:20). He instituted a ceremonial ritual based on a synthesis of Indian and Roman Catholic liturgical forms. William Henry Collison, an Anglican missionary on the Nass River from 1884 to 1920, described a performance of the ritual singing, dancing, and gesturing (including the sign of the cross) as performed for him by a very old Nishga man. As will be seen below, several missionaries to the Nishga thought that the Beni movement had prepared the way for their coming.

Through direct or indirect contact between the Nishga and Indian and European traders and missionaries, there arose a conjunction of cultural influences which was often conducive to change. It may be that an atmosphere of anticipation of change was characteristic of a trading people like the Nishga. In that case, developments during the second quarter of the nineteenth century would have reinforced and heightened these preexisting conditions (Barnett 1953:47ff.).

EUROPEANS AT THEIR DOORSTEP: FORT NASS (SIMPSON)

In the early nineteenth century, an end of the maritime fur trade in the Northwest Coast followed the depletion of commercially significant quantities of sea otter pelts. The new fur trade era was based on trading posts (forts) built on both the mainland and on Vancouver Island for greater efficiency in promoting trade. The French Revolutionary and Napoleonic Wars (1790s-1815) had hurt British fur trading and given an advantage to American traders. American and British traders sold guns, ammunition, and alcohol to coastal Indians. After 1799, these goods were preferred over those of the Russians in Sitka. The amalgamation of the Hudson's Bay Company and the North West Company in 1821 placed the Hudson's Bay Company in a strong position to compete more effectively against American and Russian traders. Among other factors, the suitability of trading sites was based on access to the interior and on indications from previous contacts that trading prospects were good.

Information about Indian customs gained during the maritime fur trade period from the 1780s to the 1820s probably told of annual gatherings of thousands of people at the fishery at the mouth of the Nass River. Mid-nineteenth century estimates indicated that each spring, as many as 5,000 people gathered at this site, making it an attractive market for European traders. European awareness of indigenous trade in the area would have enhanced the locality's potential for an expanded trade based on European goods

and European control. The mouth of the Nass River seemed to be an ideal site. Dr. John McLoughlin, Hudson's Bay Company chief at Fort Vancouver, who was formerly of the North West Company and was appointed to take charge of the Columbia Department of the Hudson's Bay Company in 1823, wrote to London in October 1825 informing his superiors that more inland furs were traded at the Nass than at any other place on the coast. McLoughlin also proposed to explore the best river route to the coast with the help of interior Indians.

Based on their superior technology and cultural prestige enjoyed by the northern coastal tribes, Nishga and Coastal Tsimshian engaged in lively trade with the interior peoples adjacent to them. In the early nineteenth century, the Tsimshian and Nishga competed successfully against European traders coming overland from the east by controlling the access of hinterland tribes to the coast. By 1830, plans were underway for a fort on the Nass River which was presumed to be a gateway to the interior. This new fort was to be named Fort Nass; later its name was changed to Fort Simpson and it was moved to a second site.[1]

In the mid-1830s, Dr. John McLoughlin began collecting supplies for Fort Nass. On July 8, 1830, three ships left Fort Vancouver for the Nass River to choose a site for the new fort. This trading expedition was also intended to be large enough for defense in case the Nishga were hostile. Traditional notions of Nishga hostility and large numbers had been established by the 1830s. The Nishga were reluctant to give Captain Aemilius Simpson of the expedition information about the area, presumably anticipating a threat to their trade monopoly and their desire to make the best bargain with Europeans. They also succeeded in getting more supplies from Simpson than he had intended to give them for their skins. Simpson may have hoped to set the initial relations between the two peoples on a good footing, and thought this was worth the extra expense. The Nishga had very specific ideas about preferred trade items: ammunition, blankets, and alcohol. Trade in the latter item was justified as necessary to make the English-owned Hudson's Bay Company competitive with American traders (Rich 1941:312). Although ignorance of the Nishga language probably impeded ease of contact, trading may have been carried out by using Chinook jargon, as well as some Haida and Tlingit.

In his orders to found Fort Nass, Hudsons's Bay Company officer Peter Skene Ogden was told what was by now the conventional wisdom about the Nishga: they were "numerous and especially hostile." A delay in starting the fort occurred due to a high rate of illness among Hudson's Bay Company personnel. In May 1831, Ogden entered the Nass estuary and noted that the waters "boiled silver" with eulachon (Binns 1967:241). Despite warnings

about Nishga hostility, Ogden found them to be reasonable and friendly. They were not armed and had furs to trade. Ogden chose the east end of Fishery Bay as the fort site, and clearing began in the summer of 1831. Buildings included a dining hall, a workshop, and a store; a stockade was also erected. According to oral tradition, the location of Fort Nass was near the cemetery of the modern village of Kincolith (Collison 1915:272). Though Governor George Simpson had urged their cultivation, the ground was found to be unsuitable for growing potatoes.

Shortly after the completion of Fort Nass, Captain Aemilius Simpson died and was buried there. Ogden subsequently renamed this post Fort Simpson. When the post was relocated to the present site of Fort Simpson, British Columbia, three years later in 1834, Simpson's remains were disinterred and reburied at this second site for Fort Simpson. In the mid-nineteenth century, the Nass River was also known as Simpson River.

The last major battle between Tsimshian and Haida was said to have taken place in 1831, and it is likely that these Tsimshian were actually Nishga. Fighting broke out when a Haida woman struck a high status Tsimshian woman in the face with a fish while engaged in trading. Tradition tells us that the two were arguing over the relative merits of the two major trading commodities of their respective peoples, halibut for the Haida and eulachon oil for the Tsimshian-Nishga (Codere 1972:105-106). Peaceful relations between these two major trading peoples of the Northwest Coast greatly aided the pursuit of trading by both Indians and non-Indians.

Some indication of Indian/non-Indian relations at the first and second sites of Fort Simpson may be gained from information about two officers of the Hudson's Bay Company who were stationed there. Dr. John Frederick Kennedy served as a physician at Fort Nass (the first name and site of Fort Simpson) from 1831 to 1832. Educated in Scotland and with a medical degree from the University of Edinburgh, Kennedy served as a surgeon and physician in the area for many years and married the daughter of Chief Legaic of the Coast Tsimshian. Since the Legaics were dominant figures in the fur trade for several generations in the nineteenth century (Robinson 1978:61-87), Kennedy's marriage would have constituted an important commercial and diplomatic alliance for both Indians and Europeans. Kennedy retired from the Company in 1845 and died in 1859 (Rich 1941:346; Dee 1945:71). Another mid-nineteenth century member of the Hudson's Bay Company staff at the new Fort Simpson was Captain William McNeill, who married Neshaki, the daughter of a Nishga chief and an outstanding trader in her own right in the 1850s (see below). This marriage can also be interpreted as having diplomatic significance.

When Fort Nass was constructed, Nishga people immediately began to build houses in the vicinity (Binns 1967:243; Large, ed. 1963:283). Several Nishga, including Chief Caxetan, Red Shirt, and Jones were in close contact with the fort. Another man, Conguele, is also mentioned (Large, ed. 1963:291). Although later contact with Conguele and Jones at the new Fort Simpson was reported by fort officers, and their information suggests that Conguele (Coaguele) was in fact a Haida chief, little else is known about these figures. It is likely that all Nishga in close contact with the fort were drawn from the two Nishga tribes of the lower Nass River, and that all of these were chiefs since Nishga leaders dominated the fur trade and controlled the economic life of their people.

In September 1832, Donald Manson traveled with Chief Caxetan up the Nass River for the Hudson's Bay Company. Despite commonplace descriptions of the Nishga as hostile, Manson was greeted with cordiality throughout his trip. Caxetan's village was his first stop, and Manson estimated it to be about twenty-five kilometers above Fort Nass, which would place it in the vicinity of Gitiks or Lakkalsap (Greenville). Manson continued on upriver, passing a few houses along the way. A second village of twenty to thirty houses also warmly received him, causing him to call it the "friendly" village. The friendly village was a principal fishing place, and a chief of this village accompanied Manson on the next leg of his voyage. Manson then proceeded on to where the Nass River narrowed and flowed through a canyon with banks which were steep and rocky, but not high. During this part of his trip, he passed eight empty villages and was told that their inhabitants were hunting groundhogs in the mountains and would not return before November. Above the canyon, he stopped at one more village which contained about twenty houses. He then reversed his course and returned to the friendly village, then to Caxetan's village, and finally to Fort Nass. Throughout his trip, he reported that he met with a hospitable reception. Later that autumn, Manson visited other peoples to encourage trade (Manson 1832). This may be the earliest description of the Nass River Valley and its sequence of village sites.

The location for the original Fort Nass proved to be a disappointment to the Hudson's Bay Company, even though it drew Indians from the surrounding area. By mid-1833, plans were afoot for another fort on the Stikine River or Dundas Island (Rich 1941:111). After only three years at its first site, Fort Nass, now renamed Fort Simpson, was relocated about sixty kilometers to the southwest. A variety of reasons were given for dissatisfaction with the original site, including an inadequate supply of fresh water, lack of a harbor which made it difficult to receive ocean-going vessels, exposure to winds, the severity of winter, nearby Russian opposition, and distance from the open sea.

Two other explanations for dissatisfaction with original site for Fort Simpson require a bit more examination. Nishga hostility, which was much talked about and anticipated, did not occur even though it was also cited as a reason for removal. Apparently, relations with the Nishga were quite amicable and peaceful. One indication of this is Nishga settlement at the fort. Another explanation for the fort's relocation is suggested by the marriage of Dr. John Frederick Kennedy to Legaic's daughter. This explanation states that relocation of the fort is attributed to the influence of Chief Legaic on Dr. Kennedy. If the fort were relocated in the territory of the people for whom Legaic was a leading chief and a major trader, his influence and authority would have been more effectively at the disposal of the Hudson's Bay Company. Thus, the interests of both the Company and the Tsimshian would be jointly served.

The Nass River proved to be much less useful for access to the interior than was hoped. The Skeena River, which was nearer to the new site of Fort Simpson, would give better access to the interior because it was navigable for more than twice the distance of the Nass River. The Skeena River had also been an established trade route since precontact times. The new site for Fort Simpson on the Tsimshian Peninsula had a natural harbor and was close to a large population of Coast Tsimshian. Many of these people settled at the relocated site for Fort Simpson after 1834.

Although relations between the Nishga and non-Indians at Fort Nass/Fort Simpson had been friendly, minor violence did erupt on the last day the buildings were being dismantled in 1834. Prior to this, no sign of distress at the fort's departure had occurred. However, on the last day, twenty-five gallons of rum were provided with the result that there was much excitement and noise which lasted through the night. Although Chiefs Caxetan and Jones remained friendly and cooperative, some Indians used physical force to interfere with the process of moving portions of the dismantled fort to the waiting ship. After the non-Indians left, the Nishga searched the remains of the fort for abandoned items, especially metal objects and iron nails. These were removed and kept for future use (Binns 1967:265-268). Alarmed by all the disruptions, Kennedy held Jones and Conguele as hostages to ensure a safe withdrawal from the fort (Large, ed. 1963:291).

Three years of close contact with Fort Nass/Fort Simpson intensified Nishga use of European goods, increased the chances for transmitting European diseases, enlarged the possibility of racial admixture, and catered to the trading inclinations of the Nishga.

The presence of Fort Nass/Fort Simpson probably contributed to the spread of disease and caused a decline in the Nishga population, though this may have been offset somewhat by taking

in neighboring peoples. The fort, its personnel, and the Nishga who settled there led to more intense use of local resources. Competition among the Nishga, Tsimshian, Haida, Tlingit, and others was heightened by the quest for goods to trade to Europeans. More potlatching and competition for status probably resulted from this. Haida trade was channeled through Fort Nass/Fort Simpson, thus giving the Nishga a role in this trade. Since British goods were often preferred, some Tlingit trade was also directed through the fort.

Although Fort Nass/Fort Simpson was removed in 1834, regular and direct contact with the fort by Indian people in the area did not cease, although it was made more difficult by the fort's removal. Travel by canoe along the coast was common in aboriginal trade and communication. Fort Simpsons's new location was in the homeland of the the Coastal Tsimshian who were neighbors, trading partners, and cultural cousins of the Nishga. The same people who had gathered at Fishery Bay at the mouth of the Nass River each spring continued to do so, and then regathered at the new Fort Simpson in the summer and early autumn to trade. Although the social and commercial elements of the fort had a new location, the participants were the same as in the previous location. Relocation of the fort called for continued initiative by the Nishga. After several decades of trading, they had become accustomed to trade goods and would have been unwilling or even unable to do without them, especially guns, ammunition, and textiles.

PATTERNS OF NISHGA TRADE WITH THE NEW FORT SIMPSON

In his *History of the Oregon Territory and British North American Fur Trade* (1844), John Dunn, a Hudson's Bay Company employee at the new Fort Simpson in September 1834, observed that in addition to Tsimshian, there were Nishga, Haida, Kaigani (Haida of Prince of Wales Island), and several Tlingit tribes from Alaska, including Tongass and Stikine, at the new Fort Simpson. Entire families came to the new fort. Dunn noted that the Nishga, Haida, and Tlingit dressed alike, another indication of the close cultural similarities between these three northern peoples of the Northwest Coast culture area. The Nishga sold furs, fish, and fish oil and received blankets and other commodities in return. They also engaged in slave trading with interior tribes (Dunn 1844:283-284). In March and April, 1835, parties of Nishga also came to trade at Fort McLoughlin, a Hudson's Bay Company post founded at Bella Bella in 1833 and named for Dr. John McLoughlin. In April, one of these groups included a high ranking woman whose physical appearance due to a labret caused special comment by William Fraser Tolmie (Large, ed. 1963:308). As we have seen, chiefs, including females, were the economic, social, cultural, and political leaders of Nishga society. The various functions of their leadership

were interrelated and inseparable. By this means, controlled change could take place in contact with the Europeans. The principal chief at Fort Simpson at this time (the mid-1830s) was Legaic, father-in-law of Dr. Joseph Frederick Kennedy (Dunn 1844:282).

Hudson's Bay Company ships also continued to visit the mouth of the Nass River to trade. In the spring of 1835, John Work traded at the former site of Fort Nass before settling in as officer in charge of the new Fort Simpson. Tough bargaining ensued because of American competition there. Since the Nishga preferred American goods, Work was obliged to raise his payments to compete and this price war was to the advantage of the Nishga. Americans attempted to improve their own competitive advantage by engaging in a coastal trade in eulachon grease (Dee 1945:43).

Some of the people gathered at the new Fort Simpson were racially mixed, with red hair and fair complexions from American and Russian fathers. A few Indians knew a little English which they had picked up from American sailors. Children of mixed marriages and liaisons would, if left with their mother's people as they usually were, be accepted into her family. Among the matrilineal Nishga and Tsimshian, children became members of their mother's crest. Mixed-race children of chiefly families might assume leadership positions in their communities when they became adults, and perhaps aid friendly contacts with Europeans and European culture. The Reverend William Henry Pierce, a Methodist clergyman and missionary, and Chief Alfred Dudoward of Fort Simpson, a sometime Methodist lay missionary, were examples of this kind of leadership in the late nineteenth and early twentieth centuries.

After contact with Europeans, the annual early-autumn trading expeditions of the Haida were based on the potato. Tsimshian villages were the main purchasers of this food and were the sites of these annual "potato fairs." Some of the canoes of the forty to fifty-ship Haida potato fleets were also sold. This shift to potatoes as the staple of trade was linked to the sharp decline of the sea-otter, and the consequent decline of the fortunes of the Massett, Skidegate, Cumshewa, and other Haida communities. The great wealth created in these communities by the sea-otter facilitated the purchase of blankets, which became the standard of wealth. By the 1830s, potatoes were the means to acquire blankets through mainland tribes such as the Tsimshian and Nishga, who had in turn purchased blankets with furs (especially beaver pelts) which were traded to British and American traders. The Tsimshian and Nishga were themselves intermediaries with the interior tribes from whom the pelts came. The Nishga chief, Jones, arrived at Fort Simpson in the Spring of 1835. While there, he and his followers fought some Kaigani Haida, but were defeated and returned home (Dee 1945:31ff.).

In canoes purchased from the Haida with fish oil, the Nishga visited Fort Simpson, bringing potatoes which they had also previously acquired from the Haida. Salmon was another trade item. Sometimes the Nishga traded with local Tsimshian, ignoring the Hudson's Bay Company fort because the prices were not satisfactory and American traders were anticipated. Higher prices for pelts drew other Indians to the fort, including Stikine and Tongass Tlingit.

The most complete source of information on Nishga/non-Indian contact is the Fort Simpson journal of the Hudson's Bay Company, which contains a description of all visitations and trading that took place. From this can be constructed an outline of relations between the Nishga and Europeans (Hudson's Bay Company Archives 1832-40, 1855-1859).

The Nishga developed a pattern of trading at the new Fort Simpson in the early years of its founding. Trading was heaviest from April to early autumn. The spring trade consisted especially of eulachon ("smallfish" or "candlefish"), as well as eulachon oil or grease, and furs. The summer trade was predominantly in pelts supplemented by salmon, oil, and other fish. Some trading with the fort continued throughout the year. The bulk of the fur trade occurred in the period April to September or October. Occasionally, the fort journal mentions a Nishga trader by name. One of these traders was Chief Caxetan (Coksawtan), who had been active in trading with Fort Nass (the first Fort Simpson). Chief Caxetan came from a village on the lower Nass River. Other traders who were mentioned, especially by the end of the first decade at the new Fort Simpson, were an individual named Ross's Friend who was later identified by the Nishga name, Skotten, and another person named Ishateen.

In 1836-1837, the early pattern of trading was interrupted by a severe smallpox epidemic which caused a reduction in direct contact with the fort. This was not the first such epidemic. The American missionary, Jonathan Green, was told of an earlier epidemic when he visited the mouth of the Nass River in 1829. The epidemic of 1836-1837 raged along the Nass River, spreading from lower to upper villages, and on to the people of the interior. In September 1837, the news at Fort Simpson was that "considerable numbers" of Nishga had died and were dying. Although the epidemic contributed to a reluctance to visit the fort, a few Nishga traders reached the fort in August and September of 1837. By the spring of 1838, trade was increasing. The main commodities brought by the Nishga were beaver, bear, marten, lynx, and land otter pelts. Eulachon, eulachon oil, and fish, especially salmon, continued to be trading staples.

Many of the furs, oil, and fish traded by the Tsimshian, Haida, and others also came from the Nass River. This was

especially true during the spring when the mouth of the river became a major fishery and trade mart for north Pacific Coast peoples, including Tlingit, northern Kwakiutl, and people from the interior.

Another pattern which developed by the early 1840s was the Nishga custom of coming in small numbers. Trading parties generally consisted of one, two, or three canoes. Nishga traders usually arrived on one day, traded the next day, and left on the third day. They sometimes refrained from trading everything at once and traded the balance of their goods on the third day before returning home. Another Nishga trading pattern which was frequently noted in the Fort Simpson journal was the retention of some pelts to sell to the American ships which visited Nishga waters in the summer months. Although the Hudson's Bay Company was annoyed by American competition, and themselves sent ships to the Nass to compete, the Nishga persisted in this practice. In such circumstances, the Hudson's Bay Company tried to undersell the Americans, restoring lower prices for Nishga pelts once the Americans departed. Throughout all of this, the Nishga gained a reputation for being tough bargainers.

The Nishga also sometimes conducted forays against the Fort Simpson vegetable garden and were threatened with being shot on sight. After at least one such incident, they remained away from the fort for several weeks.

During the period from the late 1830s to the mid-1850s, the most frequently named Nishga trader was "Ross's Friend" or Skotten as he was later called.[2] Throughout the 1850s, pelts and seafood continued to be the main trade items.

In the 1850s, there was a substantial trade in pelts by a particularly outstanding Nishga trader which the Fort Simpson journal called Neshaki (Mrs. Martha McNeill). Neshaki was a highborn daughter of a Nishga chief and the second wife of Captain William H. McNeill, commandant at Fort Simpson for most of the decade from 1851-1859, and again from 1861-1863. She seems to have risen to trading prominence during her husband's tenure as the fort commandant. Neshaki was apparently able to use her prestige and rank on the Nass River to gain large supplies of beaver, bear, marten, lynx, and other pelts. She became a wealthy trader and continued to trade even after her husband retired to Victoria. Her step-daughter, Mrs. Lucy McNeill Moffett, sometimes accompanied her on trading trips to the Nass. Mrs. Moffett was the wife of Hamilton Moffett, McNeill's successor as head of Fort Simpson from 1859 to 1861, and again from 1863 to 1866.

Although the Nishga were consistently active in the trade at Fort Simpson from the time of its founding, they were not as

prominent as the Tsimshian, Haida, and Tlingit (particularly Stikine and Tongass Tlingit). While Tsimshian and Haida canoes came and went by the scores, Nishga canoes came a few at a time, although in the 1850s there was an increase in Nishga trading, perhaps due primarily to Neshaki (Mrs. Martha McNeill). By 1857, another Nishga trader named Chief Kilzarden (Killsarder, Kinsada) began to be mentioned.3

Compared to the Tsimshain and Haida, the smaller numbers of Nishga traders and quantities of goods traded may, in part, have resulted from alternative Nishga sources for trade. The Nishga had ready access to trade through Tsimshian, Haida, Tlingit, and interior peoples, and had probably carried on this trade for an extensive period prior to contact with Europeans, perhaps for centuries. The wide Nishga choice of trading partners may have had the effect of moderating the necessity or desirability of more extensive direct contact with Fort Simpson. Like their Indian trading partners, the Americans also came directly to the Nishga, as did Hudson's Bay Company ships.

The importance of the Nass fishery as an Indian food source and trading center cannot be overlooked as a factor in shaping Nishga trading activity. The impression of many Nishga neighbors and the newly arrived Europeans that the Nishga were the center of economic activities was not completely altered by the relocation of Fort Simpson. Even more important to the Nishga than any changes which might have been occurring was the element of continuity.

While traveling south to fish, trade, hunt, and attend ceremonies, the Nishga stopped at Fort Simpson to visit, and sometimes engaged in a small amount of trading. Occasionally, Hudson's Bay Company records mention trading between Indians at Fort Simpson and ceremonial activities there. A large settlement of Tsimshian had grown up around the fort during the two decades after its relocation. In mid-June 1857, two canoes of Nishga were recorded as having arrived for a "feast."

Until late April 1840, the Nishga were usually identified in the Fort Simpson Journal as Nass people, Nass traders, or Nass canoes. After that date, they were sometimes called "Nascars," suggesting improved knowledge at the fort. The Fort Simpson journal carefully distinguished between "Nass canoes" (canoes bearing Nishgas) and "canoes coming from the Nass"; that is, Tsimshian, Haida, or Tlingit traders who had arrived from the mouth of the Nass River.

Nishga trade at Fort Simpson stemmed largely from the lower Nass River villages. The first mention of upriver traders was in early April, 1840. These upriver people traded beaver, marten, and lynx pelts, as well as fish. Pelts from the upper Nass River were especially prized by the Hudson's Bay Company (Meilleur 1980:117).

By the 1850s, rice and molasses were important trade commodities. The Nishga had developed a taste for this combination, and were reported to be "mad" for rice and willing to buy as much as was for sale.

Nishga visiting Fort Simpson were involved in the increased amount of violence reported in the fort journal. The introduction of quantities of alcohol from Victoria, from ships, and elsewhere may have contributed to this condition. Rivalries for trade and resources such as competition among various Tsimshian or Haida played a part in intertribal conflict. Such conflict would be carried to Fort Simpson, and from there to tribal homelands. Nishga-Tsimshian violence occurred in the fort journal in the case of an attack on the Nishga trader, Ishateen (Hudson's Bay Company Archives, Fort Simpson Post Journal 1838-1840, July 30, 1839).

In the 1850s, gold brought non-Indian prospectors to the Nass and Skeena rivers. Rumors of gold on the Skeena attracted miners at the same time that a gold rush was occurring in the Queen Charlotte Islands. Although the Nishga reported finding various metals on the Nass River, and John Work visited the area to determine the importance of the finds, Nass River people were spared most of the impact of a gold rush (Bancroft 1887:347). Neshaki's stepson, Henry McNeill, was among the prospectors. The limited navigability of the Nass made it less attractive than the Skeena or Stikine Rivers as a trade route to the interior, thereby reducing the number of intruders. Overland access to the Nass River valley was also difficult.

The influence of the Carrier Indian prophet Beni continued to be strong in the interior of British Columbia in the late 1840s, including the "Pene Craze" of 1847-1848. As already noted, this revitalization movement spread down the Nass and Skeena Rivers to the Nishga and Tsimshian, and thence to others, including the Tlingit (Collison 1915:10; Miller 1984:142-143). Writing of this messianic and millenarian movement many years later, the Anglican missionary, Reverend William H. Collison, took a reconciling attitude. It illustrated, he thought, the religious nature of humans and their seeking for a satisfactory understanding of and relationship with the supernatural (Collison 1915; cf. Wallace 1956). The Anglican missionary, James B. McCullagh, took a similar view, seeing the movement as a precursor of Christianity (McCullagh n.d.).

It is not clear what impact the Beni religion may have had on the Nishga or how it may have affected their response to Christianity when the latter was introduced at Fort Simpson by the Anglican missionary, William Duncan, in the late 1850s. Rumley (1973:88) thinks that a coincidence of messianic and millenarian religious thought among missionaries and Indians may have aided

the spread of Christianity. During his first two visits to the Nishga in April and September of 1860, Duncan recorded remarks of his hosts in response to his evangelization which may be interpreted as showing some elements of millenarian expectations of dramatic social and cultural change. On the other hand, this may be reading too much into Duncan's translations of Nishga comments (Duncan 1860). Instead, the Nishga may have been echoing ideas expressed by Duncan's preaching at Fort Simpson.

In the 1860s, the Nishga took the initiative of inviting missionaries to the Nass River area. The first invitation was issued by Chief Kadounaha of the village of Ankida, who had heard Duncan speak at Fort Simpson. Another chief from the Nass River who had heard Duncan on his visits to Fort Simpson was Agweelakkah of Gitwinsilth, who greeted Duncan warmly on the latter's trip to the Nass. Agweelakkah was the uncle of Tacomash, Robert Doolan's first convert on the Nass. Cowcaelth (Philip Latimer), who became a Christian leader at Kincolith, and Chief Kinsada (George Kinsada), already mentioned as one of the most famous traders on the Nass, had also visited Fort Simpson and heard Duncan preach.

Between 1860 and 1864, young Nishga men continued to visit Fort Simpson and listen to Duncan's teaching. By the early 1880s, many leading Nishga chiefs, including Duncan's original hosts, had converted to Christianity and become Anglicans or Methodists. By the end of the nineteenth century, the majority of Nishga were Christians.

CONCLUSION

From their location on the Nass River, the Nishga could observe that sea life, the basis for much of their economy and subsistence, came to them. Their neighbors the Tlingit, Haida, Carrier, and Tsimshian, came to them. In the late eighteenth century, the fur traders also came to them, and in the early nineteenth century, the American missionary Jonathan Green came to them as well. In 1860, they called on the Anglican missionary William Duncan to come to them. Today, journalists, academics, industrialists, and politicians continue to come to them.

Nishga chiefs were social and political leaders among their people and were the leaders in trade. Thus, it is not surprising that the Nishga had an attitude which Europeans perceived as assertiveness, even hauteur. The Nishga were leaders; they were the people to whom other people came; they were the dominant political, spiritual, and economic figures of their own communities.

Because of their tradition of trading relationships with other

peoples, the Nishga were adaptive to outside influences. The diffusion of Nishga culture among neighboring peoples and their acceptance of cultural elements from neighbors illustrates receptivity to change.

The above quotation by Jonathan Green (1915:80) suggests that the demeanor of the Nishga as perceived by Green and others was a challenge to missionaries. Although the Nishga would be difficult to convert, success would be more rewarding because of the extra effort this had taken. In fact, assertiveness may have been a factor in Nishga character which led them to seek out non-Indian contact and European influences.

Nishga chiefs were willing to accept new technology. They were adaptive to new material culture, new trade, and complex diplomatic relations. This was part of their expertise, and a sphere of their dominance in native culture. Although Europeans sometimes portrayed the Nishga as hostile and aloof, this did not prove to be the case when they were visited by Hudson's Bay Company personnel and missionaries such as Donald Manson and the Reverend William Duncan. Perhaps outsiders' perceptions of the Nishga as hostile can be better understood as Nishga expression of determined independence, combined with an unwillingness to accept conditions and actions which seemed to threaten this independence.

Reconstruction of Nishga-European contact from the late eighteenth century to the mid-nineteenth century confirms the reiterated portrayal of the Nishga as confident, assertive, and independent. This optimistic view provides a background for continuous documentary accounts of the Nishga which were begun in the 1860s.

NOTES

1. For a reconstruction of the history of forts and trade routes of the Tsimshian in the late eighteenth century, see George F. MacDonald, "The Epic of Nekt," pp. 65-81 in Margaret Seguin, editor, *The Tsimshian: Images of the Past; Views for the Present* (1984). MacDonald's work opens new perspectives on the protohistory of the Northwest Coast.

2. Skotten's home village is not identified. The name Skotten may be the same as that of the senior Wolf Chief of Gitlakdamiks. Four or five decades later, missionary accounts mention the names Skoten, Skotain, or similar spellings. Fort officers and missionaries tended to change the spellings of Indian names as they tried to approximate English equivalents for the sounds they heard.

3. This name appears prominently in the late nineteenth century. Chief Kinsada of the Wolf clan of Ankida invited William Duncan to the Nass. Later, he became a Christian, moved to the village of Kincolith, and in 1888 became the first elected chief of Kincolith when it came under the Indian Advancement Act of 1884.

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EAGER VISITOR, RELUCTANT HOST: THE ANTHROPOLOGIST AS STRANGER

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Dans le domaine de la recherche appliquée, le rôle de l'anthropologue est ambigu par nature, qu'il s'agisse, dans notre culture, de l'analyse d'une autre culture ou de celle d'une sous-culture non familière. Face à la culture étrangère, l'anthropologue est l'étranger par excellence, tant au point de vue physique qu'au point de vue culturel. Cet article cherche à réévaluer l'expérience de contact des ethnologues avec la culture étrangère, en particulier sur le plan de leur situation de passage, compte tenu de la notion fondamentale "d'étranger" avancée par Simmel. Ici l'argument consiste à montrer que l'anthropologue lui-même, à la fois hôte et étranger, représente une donnée cruciale du problème soulevé par l'enquête, sur le plan de ses relations interpersonnelles avec les autres membres de l'équipe des chercheurs, un fait qui a des conséquences sérieuses sur les données acquises et sur leur interprétation. Comme peuvent l'illustrer les rapports ethnographiques, la situation au niveau de l'expérience sur le terrain reflète considérablement les facteurs psychosociaux, les relations interpersonnelles et les tendances cognitives résultant des problèmes d'adaptation auxquels l'étranger fait face. Malgré ses limites, la considération de ces facteurs fait surface dans les rapports des ethnographes. L'article procède donc méthodiquement à l'analyse de la masse croissante des rapports ethnographiques, en s'appliquant à mesurer la validité et la valeur de leurs conclusions sur le plan de la recherche.

The role of the anthropologist in field research is by its nature ambiguous, whether we study an alien culture or an unfamiliar subculture in our own society. In an alien culture, the anthropologist is a stranger *par excellence*, an outsider in both the physical and cultural sense. This paper reevaluates the fieldwork situation as encountered by initiate ethnographers in alien cultures, and analyzes the ethnographer's transient position in terms of Simmel's seminal notion of "the stranger." The paper argues that the anthropologist's stranger/host interpersonal relationships with members of the research community constitute a crucial datum with serious implications for the acquisition and analysis of research data. As presented in ethnographic accounts, the field situation largely reflects the influence of psychosocial

factors, interpersonal relationships, and cognitive bias due to the adaptive problems of being a stranger. However, insufficient consideration has been taken of these factors in ethnographic reports. Methodologically, this paper utilizes the growing volume of *ex post facto* ethnographic reports to question the validity and reliability of some research conclusions.

The stranger that sojourneth with you shall be unto you as the homeborn among you. . . . (Leviticus 19:3)

INTRODUCTION

From antiquity, the stranger appears to have always been the object of special treatment. Georg Simmel (1950), writing on the ambiguous position of the stranger, might well have had anthropological field-workers in mind when he portrayed the stranger's relationship with the host community as one of "nearness and remoteness." The stranger is an outsider who is nevertheless "fixed within a particular spatial group"; yet "his position in this group is determined, essentially, by the fact that he has not belonged to it from the beginning, that he imports qualities into it, which do not and cannot stem from the group itself" (1950: 402). Simmel further touches on other important elements which are characteristic of anthropologists as strangers in the field: objectivity, detachment, and involvement in relation to the host community (1950:404-405). It is this constellation of strangerhood relations on which the present discussion is focused.

This paper attempts to reevaluate one of the most crucial aspects of anthropological research: the fieldwork situation as encountered by the initiate ethnographer in an alien culture. Starting from Simmel's well-known seminal concept of "the stranger," the paper applies this notion to the position of the anthropologist in the field. The paper argues that the anthropologist in the field is at once "the stranger and an element of the group" he or she is studying; and that the concept of "stranger" as an integral factor of the ethnographer's social situation merits serious analysis for its implications for research results. The paper further suggests that the interpersonal relations of field-workers with members of a society under investigation are in their own right a *datum* that is liable to significantly influence field data and the ultimate conclusions about a society. Attention is drawn to the apparent difficulty of making objective evaluations of field reports (ethnographies) without concurrent consideration of the role of the anthropologist as a stranger in the community that was studied.

In the welcome but belated efforts by some anthropologists to explain their field methods and to provide further insight into their published ethnographies, the critical "stranger role" of the field-worker is still inadequately considered. The relationship of the researcher to the people who are the subject of ethnographic study constitutes the core of the anthropological enterprise in the field. This is because when reduced to its basic element, anthropological fieldwork is essentially a human relations situation. The role of the anthropologist as the outsider, the stranger *par excellence*, constitutes an integral part of field data that ultimately find their way into the report about the culture being investigated. It is this role that needs more attention in the research methodology of fieldwork.

One of the earliest essays on the concept of the stranger in sociological research is that of Schuetz (1944) which focuses on the psychological aspects of strangerhood and the problem of cultural orientation faced by an outsider. "The stranger," Schuetz notes, "has to face the fact that he lacks any status as a member of the social group he is about to join, and is therefore unable to get a starting point to take his bearings" (1944:504). However, Schuetz is mindful of the ineluctable link between the psychological situation of the stranger and the social environment to which he must adapt if he is to maintain meaningful interaction with his hosts. In this process of adaptation, the stranger moves from "thinking about" the host group to "acting within" such a group. As Schuetz graphically puts it, "Jumping from the stalls to the stage . . ., the former onlooker becomes a member of the cast, enters as a partner into social relations with his coactors, and participates henceforth in the action in progress" (1944:503).

This is precisely the situation that any ethnographer in the field confronts, and it is this transformation in which the observed community passes from being only a "subject matter of his thoughts" (a theoretical model) to being a "segment of the world which has to be dominated by his actions" (a social situation) (ibid.). The very moment the ethnographer leaves the stalls of passive observation and enters the stage as a participant observer, he or she becomes a datum in the corpus of data collected about "his" or "her" people. Thus, the adaptive problems of strangerhood have more to do with the psychosocial environment than with physical existence, as so many field-workers claim.

The role of the anthropologist as a stranger has been specifically analyzed by Nash (1963) and Nash and Wintrob (1972), and has been commented upon by Richardson (1975), among others. Nash (1963) pays particular attention to the problems of adaptation to the sociocultural environment which constitutes the ethnographer's field. The gist of Nash's argument is that "the ethnologist in the field is a stranger, that he faces the problem of

adapting to this role, and that the objectivity of his field report will, through the mechanisms of perception and cognition, reflect the nature of his adaptation" (1963:150; see also Nash and Wintrob 1972:532).

This paper argues that a great deal of current discussion about the "adaptive problems" of field researchers tend to focus on sociophysical factors such as culture shock, lack of privacy, material discomfort, and so forth rather than on psychosocial factors such as cognitive bias, stress, and interpersonal relations. Field situations presented in ethnographic reports are largely the result of the influence of all these factors, and reflect the subjective view of both the ethnographer and "trusted informants" who, by the evidence of most ethnographic accounts, are marginal in their own culture. This point will be discussed later in the paper.

Before we delve into the main discussion of the paper, it is essential at this stage to clarify a number of points in order to obviate any misconceptions about the paper's aim. This paper is not a denunciation of the discipline of anthropology as ethnography, nor of fieldwork as a principal method of ethnographic research; nor is this paper a critique of anthropologists as social scientists. Rather, the main thrust of the paper is directed at fieldwork problems, qua problems, and the manner in which these problems affect or influence both the acquisition of ethnographic field data and the quality of the cultural information gathered. For this reason, the paper argues that what either happens or does not happen to the anthropologist as a stranger in the field, and the manner in which the anthropologist reacts to the experience of fieldwork, constitutes a major aspect of the field data that ultimately shape an ethnographic report. As will be pointed out below, it has not been traditional for anthropologists to report on the fieldwork experience itself and the attendant methodological problems which are inevitably entailed in the stranger-host relationship in "other cultures." Consequently, the objective evaluation of ethnographic research was not always as self-evident as it might seem from the reported results. Lately, because anthropological field-workers have either included pertinent information about their field experiences or have subsequently published reflective reports of those experiences, the anthropological community and other social scientists have been able to make informed, retrospective evaluation of their endeavors (Chagnon 1968, 1974; Hicks 1976; Malinowski 1967; Middleton 1970; Rabinow 1977; Siskind 1973; Spindler, ed. 1970; see also the highly instructive collection of reflective articles in Lawless and Vinton, eds. 1983).

The changing nature of anthropological research has concomitantly resulted in a refinement of research methodology in

order to conform to the standardized canons of research. Closer attention is now being paid to much more intrusive as well as obtrusive factors which are an intrinsic hazard of fieldwork. The cardinal point of this paper is that the ethnographer is a primary factor in this process. A major catalyst in this heightened awareness of the methodological problems of fieldwork is twofold: (1) the changing nature the field situation in traditional research locales as "native" societies have become more literate and reflective about the anthropological enterprise; and (2) increasing anthropological interest in the segments of "complex" societies. While this is not intended to be a categorical detraction from the quality of the work of earlier generations of more traditional field-workers, it allows for a more realistic evaluation of the often uncritical acceptance of published field results. Pointing out problems in a discipline does not denounce the enduring contribution of its pioneers; rather, this procedure heightens awareness of the need for rectification as shown by the growing literature on anthropological research methodology, something that was painfully lacking in anthropological field preparation a generation ago. Thus, to construe the ensuing reexamination of such works as an unfilial symbolic devouring of anthropological ancestors in a ritual cannibalistic feast of atonement would be erroneous; instead, it is a reaffirmation of the humanity of the discipline whose focus is the study of humankind by humans themselves.

Until recently, it has not been in vogue for anthropological field-workers to include their frustrations and difficulties with their subjects, their failures, disillusionments, and even disenchantment with the cultures studied in their ethnographies. Rather, the tendency has been to stress the strategies which were adopted for eliciting data that enabled the researcher to present an "integrated structure" of that society. We know more about the perennial problem of bridging the conceptual boundaries between the anthropologist as a cultural stranger and his or her native informants than we know about the psychological stress produced by an uneasy and often artificial relationship between an eager visitor and reluctant hosts. Occasionally, a negative comment about some reluctant or recalcitrant informant provides insight into the anthropologist's relations with some of his or her subjects. In many cases, difficult relationships are dismissed as incidental to, or are mentioned in order to emphasize, the much touted friendly acceptance of the researcher by the "natives."

Recently, a spate of *ex post facto* reports on stress and response in fieldwork have been published in various formats in an attempt to provide insight into the tribulations of field-workers and as guides to methodology and research techniques in the field. Few, if any of these works dwell on the social and psychological impact that the stranger exerts on his hosts. Given the personal nature of

anthropological fieldwork, it is legitimate to ask: To what extent is the highly touted claim that field-workers were "fully accepted as adopted kinsmen" of their tribes reliable or verifiable in the absence of corroborative information? The apparent fact that an anthropologist was permitted to live and work among unfamiliar people and even to receive their cooperation in a venture from which they have no interest and derive no benefits, material or otherwise, does not constitute evidence that the people concerned did not harbor serious misgivings about the disturbance of their life by the intrusion of the stranger in their midst (Briggs 1978:26; Barnes 1967:198).

STRANGER-HOST FIELD RELATIONS

The role of the anthropologist in field research is always ambiguous, whether we study an alien culture or an unfamiliar subculture in our own society. In an alien culture, the anthropologist is naturally the stranger *par excellence*, an outsider in both the physical and cultural sense. Within a subcultural segment of their own society, anthropologists are enigmas to members of the subsection about whose social behavior they ask questions. Furthermore, informants may be convinced that anthropologists either ought to know the answers to these questions naturally, or have no business knowing (Spradley 1979).

Traditionally, anthropologists have tended to claim that "I was adopted and initiated as a kinsman by the tribe," or "I was accepted and treated as one of them, as a member of the tribe." The ethnographer will then refer to some traditional custom among "his people" of adopting strangers as fictive kin. For example, Barnett invokes local custom as a basis for his claim to Palauan (fictive) kinship.

> In fact, my intimate acquaintance with this family, and my entree into the community which it provided, convinced me that I could and should write something like *Being a Palauan*. Kai spoke of me as his elder brother. He treated me as such and I was so accepted by other residents of Ngara. (1970:30)

The last sentence raises an interesting epistemological question about subjectivity in fieldwork. To what extent can it be shown that the researcher's perception of, and feeling about, this relationship coincide with his hosts' own evaluation of the makeshift arrangement? In the absence of a Palauan viewpoint, it is difficult to verify the ethnographer's own assessment that:

> This was neither false nor as difficult as it might be in other societies because the establishment of fictive

> kinship is a Palauan custom. It gave me a place in their social system in addition to the one I inevitably was assigned as a prestigious outsider. (ibid.)

It should be emphasized that the ethnographer's sincerity is not in question; what is at stake is how to bridge the gap between objectivity and subjectivity in fieldwork, how the ethnographer perceives this, and what actually occurs in the ethnographer's relations with hosts.

As teachers, anthropologists often stress the overriding importance of the kinship idiom in the social relations of "primitive societies." By implication, they enculturate students into thought patterns that lead to the assumption that being "adopted as a kinsman" by the natives is a key to fieldwork success. Nothing could be further from the truth (see Chagnon 1968:4-5). Firth warns against exaggerated or sentimental claims about overcoming strangerhood on the basis of fictive kinship:

> . . . I regard with skepticism the claim of any European writer that he has "been accepted by the natives as one of themselves." . . . such a claim is usually founded upon a misapprehension of native politeness or of a momentary emotional verbal identification with themselves of a person who shares their sympathies . . . as in dancing with them and observing the etiquette of (pseudo-) kinship . . . (1957:11)

Similarly, Hart (1970:151-152) gives an interesting but skeptical account of the "fictive kinship game" in fieldwork. And Freilich cautions that "the anthropologists's role is somewhere between 'native' and 'privileged stranger'. . . Irrespective of what role he assumes, the anthropologist remains a marginal man in the community, an outsider" (1977:2). Schwab also shows an awareness of the dubiousness of the popular claim to fictive kinship: "The anthropologist under the best circumstances never becomes a fully accepted member of the culture he is studying . . ." (1977:47).

Beals provides yet another insight into the relationship problem between stranger and host when describing his and his wife's early experience in Gopalpur, where villagers "Were unaccustomed to strangers and they would not let us remain as strangers" (1970:45). After some initial adaptive adjustment "we came to feel at home in Gopalpur. . . . We were a part of the community. . . ."(1970:46). The picture presented thus far suggests nothing of the feelings and attitudes of the hosts about the strangers among them. Ethnographic accounts are by nature one-sided, although based on dyadic interaction. Most ethnographies, and lately "field method guides," provide insight into

the ethnographer's tribulations in dealing with the "natives." There is concern about the lack of privacy experienced by ethnographers in the field--that they cannot enjoy a few moments of quiet and privacy without being bothered by prying natives (Evans-Pritchard 1940:13-15; Chagnon 1968:3-6; and for a perceptive treatment, see Dentan 1970:104-105, 107). Yet one hears far less about the effects of the stranger's own intrusion into the social life of the hosts, and the possible resentment this might cause towards the ethnographer.

The fact that anthropologists in the field are strangers and are subject to all the vagaries of that role *vis-à-vis* host communities has serious implications for the acquisition and analysis of research data. The experiences of those field-workers who have paid some attention to their relationship with their hosts as a datum in their research, and reported on this, raise a host of methodological problems about data gathered from reluctant hosts. Dentan directs attention to this problem:

> We were afraid that if the people thought of us as "outsiders" they would tell us whatever they thought we wanted to hear and would conceal anything intimate or anything of which they thought we might disapprove. . . . They distrusted outsiders (mai) so much that they use a special slang to conceal what they are talking about in the presence of an outsider. . . . (1970:92)

Similarly, in his much celebrated study of the Nuer, Evans-Pritchard illustrates the tactics adopted by these people to fend off curious ethnographers. "Nuer are expert at sabotaging an inquiry and until one has resided with them for some weeks, they steadfastly stultify all efforts to elicit the simplest facts and to elucidate the most innocent practices" (1940:12). Strange as it may seem, in his analysis of Nuer society, Evans-Pritchard appears to gloss over this difficulty and does not see it as an impediment that might vitiate his data. By a curious turn of logic, he compares his field experiences among the Azande and the Nuer, and antithetical as the two situations were, he comes to the same conclusion about the validity and reliability of data from both. He writes:

> Because I had to live in such close contact with the Nuer I know them more intimately than the Azande, about whom I am able to write a much more detailed account. (1940:15)

What were the ethnographer's relations with these two societies respectively? We are told epigrammatically that:

> Azande would not allow me to live as one of themselves; Nuer would not allow me to live otherwise. Among

> Azande I was compelled to be outside the community; among Nuer I was compelled to be a member of it. Azande treated me as a superior, Nuer as an equal. (ibid.)

Yet in subsequent accounts of these respective societies, Evans-Pritchard is silent on the relationship between data and these varied conditions under which data were collected.

Malinowski was the first among the earliest ethnographers to explicitly recognize the role of the stranger in fieldwork, his impact on the host community, and the possible implications of such an impact for field relations. Malinowski observes of the Trobrianders that " . . . they knew I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished regarding me as part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco" (1928:8). I suggest that for all anthropologists in the field, this observation constitutes the limits of so-called full acceptance or adoption as fictive kinsmen in the culture they are studying. There are many ways of dealing with a "nuisance"--eliminate it, tolerate it, ignore it, or in its human form, accede to its pestilent questions to get it off your back. "Natives" often adopt the latter solution and supply the kind of information they think will get the inquisitive anthropologist off their backs. Often, an exploitative situation develops in which the "informant" not only trades off information with the anthropologist, but intentionally deceives him (Chagnon 1968).

Chagnon's self-revealing attitude towards the Yanomamo epitomizes the peculiar relationship between the stranger/ ethnographer and the native hosts. Both parties played the "bluff-upmanship" game to a fine degree:

> On another occasion I was eating a can of frankfurters and growing very weary of the demands of one of my guests for a share in my meal. When he asked me what I was eating, I replied "Beef." He then asked,"What part of the animal are you eating?" to which I replied, "Guess!" He stopped asking for a share. (1968:7)

Similarly, the Yanomamo would call Chagnon's bluff or respond with their own:

> "Give me an axe or I'll break into your hut when you are away visiting and steal one!" And so I was bombarded by such demands day after day, month on end, until I could not bear to see an Indian. (1968:8)

Apparently, this bluffing relationship continued throughout

Chagnon's fieldwork: "As I became more proficient in their language and learned more about their political tactics, I became more sophisticated in the art of bluffing" (1968:9). The consequences of all of this were reflected in falsified data that were supplied to Chagnon over a five month period (1968:10-13).

Defined in stranger/host terms, the informant-ethnographer relationship is the pivot upon which the whole anthropological enterprise in any given community revolves. Success in fieldwork may hinge precisely on the nature of this relationship. The informant-ethnographer relationship is most intimately linked with the quality in terms of veracity of the data supplied by informants, since the informant's task as imposed by the ethnographer is to "teach" this stranger all the "facts" about the society (Richardson 1975:520-521). The question of bias cannot be ignored in such a contrived social relationship. First, there are categories of information (cultural data) which are deemed good for strangers, some of which are considered appropriate for friends, and others of which are shared only among clansmen and close kin. These are universal cultural principles of information management. Second, both parties are in a manipulative situation which influences the exchange of desired "commodities" (information for gain). In addition, the respective attitudes of the participants toward each other are governed by this contrived reciprocal relationship (see Firth 1957:10-13; Beattie 1964:86; Beals 1970:47-48; Dentan 1970:108.)

It is all the more surprising that an eminent anthropologist and superb field-worker such as Evans-Pritchard would pay scant attention to the interpersonal relationship between ethnographer and informants in his methodological lecture on fieldwork. Rather, Evans-Pritchard seems to treat all of "native society" or "primitive society" much as an abstraction which constitutes the "field" for the ethnographer, and not as a vibrant community of individuals with whom the field-worker must interact on a personal basis: "The native society has to be in the anthropologist himself and not merely in his notebooks if he is to understand it, and the capacity to think and feel alternately as a savage and as a European is not easily acquired, if indeed it can be acquired at all" (1951:82). The obtrusiveness of the field-worker is also minimized: "He is not there to change their way of life but as a humble learner of it" (1951:79).

However, it may be argued that by its reliance on "key-informant interviewing," fieldwork affects changes in the community (Edgerton and Langness 1974:33-35). But informants and informant interviewing are not an integral part of "native society," for informants have to be trained in the ways of the anthropologist. At least one ethnographer has pointed out that: "The problem of finding, cultivating and changing informants is one of the most delicate facing the anthropologist" (Rabinow 1977:92).

The role of the anthropologist as a stranger limits access to certain kinds of information and encourages increasing reliance on so-called "trusted informants." But trusted informants are not altruistic conveyors of information. Instead, they are purveyors; that is, partners in an exchange relationship in which the stranger is at a disadvantage (Barnes 1967:198). Since anthropologists need certain information which is not normally accessible to strangers, they must rely on "informants" who have complete control over what information the stranger ought or ought not to have, and when and how this information will be transmitted.

Chagnon's experience among the Yanomamo is instructive. After apparently a whole year of collecting uncertain genealogies, he writes:

> . . another individual came to my aid. It was Kaobawä, the headman. . . . He visited me one day after the others had left the hut and volunteered to help me on the genealogies. He was poor he explained, and needed a machete. He would work only on the condition that I did not ask him about his own parents and other very close kinsmen who were dead. He also added that he would not lie to me as the others had done in the past. This was perhaps the most important single event in my fieldwork, for out of this meeting evolved a very warm friendship and a very profitable informant-field-worker relationship. (1968:13)

The pitfalls in this kind of relationship are obvious as we learn that the ethnographer later relied heavily on one self-ingratiating informant. "If there were things he did not know intimately, he would advise me to wait until he could check things out with someone in the village. This he would do clandestinely, giving me a report the next day" (ibid.). Reliability of data was checked against the opinions of a close kinsman of the informant, Rerebawa, who in the ethnographer's evaluation "is one of few Yanomamo that I feel I can trust . . . he is the most genuine and most devoted to his culture's ways and values. I admire him for that. . . ." (1968:16-17; see also Chagnon 1974:101-103).

Manning and Fabrega have directed attention to the role of informants and the implications of this for field research. They point out that the role of informant has become specialized and is dependent upon mutual socialization between ethnographer and informant; that is, it is a role involving the exchange of information for "prestige, money, affection, and ingratiation" (1976:44). The ramifications of this relationship lead to the development of a quasi-professional cadre of informants who are willing to serve at a stipulated rate of pay as informants on

certain types of problems by adopting a tactical stance which includes strategies of avoidance, information-management, and social control of the investigators (ibid.; see also Pelto 1970:95, 97).

Berreman's experience in an Indian village is very instructive because it dispels the prevalent notion that the field-worker's adaptive problems are effectively solved by fictive kin adoption in simple societies. In Sirkanda, strangers "are avoided or discouraged from remaining long in the vicinity. To escape such a reception a person must be able to identify himself as a member of a familiar group through kinship ties, caste (Jati) ties and/or community affiliation" (1962:4-5). It should be noted that the norm that strangers must be able to identify with local groups in order to gain acceptance is not the same thing as the assertion that acceptance will follow because a particular community has a custom of fictive kinship by which strangership may be transformed. To suggest, as many field-workers have, that a norm which obtains for a local community is *a fortiori* a justification for ultimate strangers to claim the same privilege as members of adjacent or local groups misconstrues the social customs of "simple" societies.

Berreman speaks of being constantly under suspicion:

> Nearly four months had passed before overt suspicion . . . was substantially dissipated, although . . . some people had been convinced of the innocence of our motives relatively early and others remained suspicious throughout our stay. (1962:7)

With candid honesty, Berreman states that in the eyes of the natives he remained an alien, that he was never made to feel his presence in the village was "actively desired by most of its members," and that he was "tolerated with considerable indulgence" (1962:8). Apparently this indulgence was coupled with information-management on the part of the villagers whenever they thought this was necessary, and "did not mean we therefore could learn what we wanted to learn in the village" (1962:9).

STRANGERHOOD AND CULTURAL DATA

The relationship between the eager visitor and reluctant host and its implications for the reliability of ethnographic data should never be underestimated. The adaptive problems of the anthropologist as a stranger, and the personal and cultural obstacles that may impede the acquisition of reliable data have been aptly spelled out by Nash (1963). He points to the enormous jump from one culture to another, the need to establish rapport and to acquire fairly complete data in both a limited amount of time and under extreme conditions of strangership, and pressure on

field-workers to succeed because their careers and reputations depended on successful adaptation to the field. And as Chagnon puts it, "Scientific curiosity brought me to this village and professional obligation kept me there in circumstances I did not particularly enjoy" (1974:196). In a similar fashion, the much revered field-worker, Malinowski, described his native subjects in less than flattering terms. He saw their life as "utterly devoid of interest and importance, something as remote from me as the life of a dog" (1967:167). His main interest was to "somehow document" all aspects of their lifestyle so that he would have valuable material for his project.

As many field-workers have eloquently testified, the majority of "trusted and useful informants" are marginal individuals in their cultures. This applies with even greater force to academically-trained, local informants. The possible pitfalls that the choice of academically-trained field assistants-cum-informants might pose are well-illustrated in Diamond's revealing but instructive field-method report on her work in Taiwan (1970). Among her principal field assistants-cum-interpreters were a Taiwanese "from one of the modern fishing ports on the eastern coast . . . trained in economics at Taiwan National University," while another "came from a small farming village, but had studied a number of years in Taipei. He had a Masters (*sic*) degree in political science and possessed a strong interest in sociology." Furthermore, this assistant was "Widely read in the social sciences . . . " and ". . . his academic training and reading enabled him to do a considerable amount of work on his own" (1970:132-133). The others were "two young women who taught in a city middle school"; and "One of them had taken anthropology as her major in college. . . ." Yet another key field assistant-cum-informant was "an active leader in the local Fishermen's Association and Farmer's Association, as well as being a member of the school board and temple committee." Moreover, he was "Literate, (and) `modern' in his attitudes towards technological innovations and education, and at the same time deeply concerned with the preservation of tradition . . . ." (1970:134).

Such excellent qualifications on the part of an ethnographer's field assistants or informants may be presumed to enhance the quality of the investigator's data; yet the crucial question remains: To what extent may data collected under such conditions be taken as representative of the "native culture"? Could an informant who is labelled "modern in his attitudes towards technological innovations and education" be free of bias against native culture? An even stronger objection may be raised against the utilization of, and reliance upon, trained field assistants-cum-informants from the culture under study. Since field researchers are wont to declare that their key or trusted informants were somewhat marginal to their cultures, one wonders why the information they provide should be treated as "social facts" about traditional native cultures

(see Chagnon 1974:18; den Hollander 1967:15).

Even more serious doubts may be raised with respect to the use of native informants or assistants trained in the social sciences, since such training is *a fortiori* an enculturative experience in itself. In other words, a scientifically trained native assistant is bound to observe his/her society's culture with "interpretative eyes," and is liable to respond with "analytic verbal responses" rather than with factual answers to cultural questions. This point may be more forcefully appreciated if an analogy is drawn between social scientists and other members of their own society who are *not* trained in sociology or anthropology. The social scientist's analytic perception of his/ her culture is qualitatively different from that of lay members of the same culture. Thus, it is not without a grain of truth that anthropologists in particular are often viewed, and view themselves, as marginal persons in their own societies, including the academic community in general (see Nash 1963:159). It would be academic naïveté to suggest that any student who has been enculturated into socioanthropological thought-patterns would still be capable of looking at social phenomena without tinted lenses. As Nash aptly puts it, "The novice anthropologist is recruited into and trained by a group with a particular formal and informal ideology which, to a greater or lesser extent, he absorbs and carries with him into the field" (1963:149).

A most instructive caveat against the uncritical use of informants who are enculturated in the social science milieu is poignantly illustrated by Spradley's narrative of his field encounter with a former Harvard University graduate student in anthropology who had become a skid row habitué. Initially, Spradley was "excited about the possibilities of working with Bob as a key informant," but soon discovered that Bob was responding to interview questions with:

> . . . the standard analytic categories that many social scientists use. . . . [and] tended to analyze the motives men had for drinking and other behavior, but his analysis always reflected his background in college. (1979:53)

This candid observation is a far cry from the traditional habit of most field-workers of ignoring the potential distortions of data that inevitably result from information provided by "enculturated" informants. It is well-known that the principal aim of anthropological fieldwork is the first-hand acquisition of raw data on a given research community. Paradoxically, such data are to be obtained from native informants who are knowledgeable about their culture, but is to be conveyed in idioms which are not consistent with the scientist's expectations of order, coherence, and rationality. Perhaps the insidious temptation to rely increasingly

upon "trained informants" is fueled by what den Hollander calls "a passion for neatly smoothing out a disordered social reality which thus distorts the social life of the people being reported on" (den Hollander 1967:20).

The problem of biased social perception and data collection by anthropologically trained observers is pointedly discussed by Keiser on the basis of his research on an urban subculture in his own society. As Keiser frankly puts it, "What I saw as facts and therefore recorded, was directly related to my theoretical orientation. Because of my orientation, I did not record certain things that are undoubtedly important" (1970:233). This is precisely what den Hollander so aptly terms "arranged truth," that is, truth (read data) that must look organized to be veracious (1967:25). As every anthropologist knows, this dilemma confronts all anthropological field reporting.

Philosopher Karl Popper has directed some attention to this incessant habit of the subconscious reorganization of phenomena in scientific observation, and reports:

> Our propensity to look for regularities, and to impose laws upon nature, leads to the psychological phenomenon of dogmatic thinking or, more generally, dogmatic behaviour; we expect regularities everywhere and attempt to find them even where there are none; events which do not yield to these attempts we are inclined to treat as a kind of "background noise," and we stick to our expectations even when they are inadequate and we ought to accept defeat. . . .(1963:49)

A similar poignant point is made by den Hollander to the effect that "we avoid or reduce any information that creates dissonance and welcome anything consonant with our conditioning for order and structure" (1967:20). It is thus reasonable to suspect that anthropologically trained informants would subconsciously make *a priori* deductions from perceived "social facts" and imply "structural" linkages and "functions" which are not warranted by the observed social phenomena.

Turning now to a "confession" by a practicing anthropologist, we find confirmation of this penchant for "arranged truth" in anthropological field reports. In a foreword to *Political Systems of Highland Burma* (Leach 1954:vii), Raymond Firth confides that in order to keep anthropological reporting neat, "Some of us . . . have not hesitated to tell our students in private that ethnographic facts may be irrelevant--that it does not matter so much if they get the facts wrong so long as they can argue the theories logically." This is a seriously disturbing commentary on anthropology as a science. Sacrificing empirical facts for theoretical models, for equilibrium,

for order and organization, and for style appears to be a well-entrenched practice in anthropological thinking, particularly in British anthropology as witness the works of Evans-Pritchard (1937, 1940, 1951, 1956); Fortes (1945, 1949a, 1949b, 1969); Gluckman (1955); Leach (1954); Lienhardt (1961); Radcliffe-Brown (1950, 1952); et al. Barnes (1962) has drawn attention to the British structuralist influence on researchers working on lineage/kinship systems outside of the African context. He criticizes the tendency to predicate lineage models on New Guinea societies which have been extrapolated from African ethnography.

Because fieldwork is essentially a human relations situation, ethnographic field reports sometimes inadvertently reveal the personal biases of researchers with regard to some of their informants. Individuals whom the ethnographer considers as unfriendly or disagreeable are often by-passed, or, if discussed at all, cast in negative light. Regarding the people of Takashima, Japan, Edward Norbeck writes: "At the time of my first interview with the head man of the community, I had suspected that he was generally disliked and I did not like him myself. He seemed arrogant, rude, and terribly contemptuous of his neighbors . . . . " (1970:250). Because of this negative attitude toward the headman, Norbeck "talked with him only often enough to meet the demands of etiquette. . . ." (ibid.). Similarly, Chagnon shunned a Yanomamo headman he considered "unpleasant, selfish and self-centred"; their "relationships were fairly cold and strained" and he was thus never used as an informant (1974:167). Would it not make anthropological sense to treat the alleged unfriendly or uncooperative behavior of such native individuals as a datum in itself, rather than merely as anecdotes?

Again, those individuals whom ethnographic researchers are wont to call "excellent informants" are often marginal members of their societies. Note the following observation:

> Ali was a first-rate informant. He was intelligent, quick to learn, patient, cooperative, and vivacious. . . . Ali, like several other people with whom I worked, was a marginal character in his own social world. He was not the average villager, he was far from the solid citizen stereotype of Serfrou . . . Ali was more self-reflective about his society and his place in it than most Moroccans I knew. . . . He had rejected village life. . . . Already being ostracized by large segments of the community, he would mock the bonds of social control by flaunting his freedom. (Rabinow 1977:73)

Information provided by such channels provokes serious questions of validity. Our discomfiture is further increased when the researcher reveals that Ali was a professional information

purveyor: "Ali had deliberately pursued me . . . partly because he saw the possibility of income and partly because he was relatively immune to the community's social control. He had worked with other anthropologists who had come to Serfrou; he knew the ropes . . ." (1977:75; see also Chagnon 1968:13).

The stranger/ethnographer is liable to make value-loaded judgments when comparing his favored informants who display "an imaginative ability to objectify one's own culture for a foreigner," with those who are deemed to lack this quality. Yet the so-called village idiot may provide valuable insights into the "unrationalized" aspects of his society's culture (Rabinow 1977:94-95).

Most ethnographic accounts of fieldwork experiences reveal interesting similarities in the tendency of researchers to stress the supposed good qualities of their favored informants. For example, one of Rabinow's favored Moroccan informants, Rashid, "was incredibly quick, intelligent, sensitive and overflowing with gossip and slander about almost everyone in the village" (1977:96). Yet when the anthropologist was warned of Rashid's moral character, he dismissed it as due to "simple jealousy." By the ethnographer's own account:

> The moral attacks on his character never impressed me very much. . . . In retrospect, all these charges and several more turned out to be basically true. But for some of the same reasons he was an excellent informant, he was on the fringe of community control. . . . He would say things and talk about people in a manner which the anthropologist cherished. . . . He was more than happy to tell me almost everything I wanted to know. (1977:98-99)

To what extent can such admittedly subjective information be accepted as reliable and valid data?

One suspects that anthropological field-workers tend to exaggerate their informants' capacities for comprehension of their respective cultures and/or their informants' credence. It is well-acknowledged that culture in preliterate traditional societies is "lived" and acted out rather than philosophized about (cf. Bloch 1971:86). Consequently, members of such societies cannot be assumed to have contemplated most of the cultural elements about which the ethnographer asks questions in isolation from their role in the scheme of social behavior. Similarly, the average lay person in technologically advanced societies generally has only a vague notion of the "patterning," "interdependence," or "functional relationship" of the disparate elements of his culture. Anyone who cares to try out the anthropological informant interview method on a class of undergraduates will soon perceive the truth of this

analogy.

It is thus with some hesitation that one would be inclined to accept uncritically the objectivity and validity of data sources such as the following: "In Sensuron we identified and selected a girl of sixteen for our household assistant . . . The girl had been trained as a ritual specialist . . . She served as a key informant . . . Her special ritual knowledge and her ability to give meaningful and accurate details of widely shared aspects of her culture were invaluable" (Williams 1967:29). Apart from the declared immaturity of the informant, serious doubts may be raised about the validity of information rendered by an equivalent of a medical neophyte concerning medical science and health practices in an advanced society. By what standards were her ritual knowledge, ability, and accuracy of detail measured?

It should be remembered that the role of informant is not an integral part of the structure of native society, but an additional datum in a new structural relationship that includes the ethnographer as a stranger. The place and position of the ethnographer in the society being studied determine the channels of information that are open to him or her. More importantly, "The channels of information in turn are crucial in defining the information itself" (Buechler 1969:1).

In retrospect, Berreman (1962) appears to take full cognizance of this stranger-host relationship and its implications for field data. Berreman's observations suggest a further rebuttal to the idealistic claims of some field-workers that mere entry into a research community entails acceptance as a kinsman. Berreman writes:

> In such a society the ethnographer is inevitably an outsider and never becomes otherwise. . . . The nature of his data is largely determined by his identity as seen by his subjects. Polite acceptance and even friendship do not always mean that access will be granted to confidential back stage regions of the life of those who extend [these courtesies]. (1962:21)

There are encouraging signs of an increasing awareness that all may not have been well with field data obtained under conditions of doubtful relations, at times bordering on resistance, deceit, and fabrication of information purveyed to the unsuspecting ethnographer. Retrospective revelations of field experiences by various ethnographers suggest that a hitherto unacknowledged truth may be that the enthnographer's "stranger status constitutes a major constraint" in gaining unfettered acceptance and acquiring unsullied data (Uchendu 1970:231). Subtle rejection of the stranger may be the general order rather than the exception in field relations.

By and large, anthropological field reports tend to emphasize the positive aspects of their authors' relations with members of the societies studied. These relations are often presented as a process of progression from initial problems of adaptation, through uneasy acceptance, to successful integration (the "as-one-of-them" fictive kinsman claim). There is something uncannily analogous to *rites de passage* in this process. In its mathematical sense, the initiate's successful integration becomes a function of the degree of difficulty in adaptation and acceptance; hence the tendency to exaggerate the degree of rapport with, and acceptance by, the native communities reported on. However, as Schwab rightly points out, "The anthropologist under the best circumstances never becomes a fully accepted member of the culture he is studying. . . . Usually people look upon him as outside the indigenous culture and do not expect him to follow their cultural norms" (1977:47).

I suggest that in their initial field experience, most anthropologists have tended to exaggerate their supposed adoption as "kinsman" by "their people" in the field. To begin with, kinship is not primarily a status; it is a jural relationship which by its nature implies reciprocal interaction in behavior patterns. One of the attendant obligations of a kinsman, fictive or otherwise, is mutual support in cases of need or conflict. Yet it is a truism that most anthropologists eschew any involvement in personal or intracommunal problems which are extrinsic to the researcher's immediate concerns, nor do they become involved in problems that pertain to members of the local community *vis-à-vis* officialdom. An instructive example of the divergence of interests and expectations between the field researcher and "his people" can be gleaned from Gutkind's graphic description of his encounter with the urban unemployed in a Nigerian city (1969:26-34). As an anthropologist with a strong sense of social justice, Gutkind experienced the dilemma of being confronted by a segment of the community that expected tangible benefits as reciprocity for information provided about labor conditions, while as an ethnographer, Gutkind strove to maintain a posture of scientific noninvolvement in the affairs of the people. The unemployed locals apparently construed Gutkind's scientific interest in them as sympathy for their economic condition for which he might be able to provide help; that is, to find work for them. Obviously, the native informant would consider such reluctance to reciprocate on the part of a "kinsman" as a serious breach of the "axiom of amity" (Fortes 1969:Chapter XII).

IMPLICATIONS FOR ETHNOGRAPHIC RESEARCH

The main purpose of this paper has been a critical reevaluation of ethnographic methodology by focusing on the role of the anthropological field-worker as a stranger in the field. The

paper has attempted to show, by the evidence of their own reports, that most field-workers have not paid sufficient attention to their role as a datum in the field enterprise, and that this inattention may have deleterious implications for the validity of ethnographic results. Although a spate of methodological literature on fieldwork has emerged in the last two decades, and analytic techniques have been sophisticated, the subject of informant-ethnographer relations as *cultural data* has not been treated as a focal point in the ethnographic enterprise. Yet the validity of field data cannot be any more reliable than the quality of their sources.

If anthropological reports about remote societies are to have a reasonable degree of validity and reliability, then field-workers must recognize the severe limitations of their sources of data and remain wary of distortions which will probably result from the ineluctable impress of their "disciplinary culture." In social and cultural anthropology, the field-worker confronts the academic community with "social facts" presented as data whose reliability rests on the *bona fides* of the researcher. The researcher's own acceptance of these "social facts" depends in turn on faith in "key informants." To be sure, fellow anthropologists and the scholarly community can and do subject *stated* cultural "facts" and authors' analyses of these facts to rigorous assessment according to the prevailing canons of scientific tests. Nevertheless, the scientific community cannot vouch for the validity of raw data. In order to do so adequately, they would need sufficient factual data on the ethnographic enterprise. As Winch has pointed out:

> To understand the activities of an individual scientific investigator we must take account of two sets of relations: first, his relation to the phenomena which he investigates; second, his relation to his fellow scientists. (1958:84)

Schneider states this point more pertinently by focusing on the ethnographer's process of "sifting" empirical facts:

> When we read about kinship in some society foreign to our own we have only the facts which the author chooses to present to us, and we usually have no independent source of knowledge against which we can check his facts. It is thus very hard to evaluate his theory for ordering those facts. (1968:vi)

Deleterious implications for an anthropology based on field reports of doubtful validity extend beyond the scientific community. What field-workers publish may be difficult for colleagues who are remotely removed from the communities in question to validate with any certainty. However, this is not the case for members of "native" communities who may be able to verify raw facts and

question interpretations of cultural data. Gross or even unintentional distortions of a culture may lead to the development of hostile reactions toward ethnographic research. Brislin and Holwill (1979) raise a timely question in this respect: "Why are social scientists, who have developed such fields as human relations and culture communication, disliked in many circles among the people they study?" The answer they advance is that "social scientists have not systematically examined the opinions about their writings that are held *by those written about*" (1979:65, emphasis original).

Brislin and Holwill have compiled a corpus of data based on their survey of "Indigenous Views of the Writings of Behavioral/ Social Scientists: Toward Increased Cross-Cultural Understanding" in which they sought "insiders' opinions" of standard ethnographic works. Assessments were made on the basis of the accuracy of ethnographic observation, location, information, interpretation, and reporting. A sample of native opinion on selected standard ethnographies yielded the following observations:

> I think the person in our culture who (*sic*) the anthropologist worked with only wanted money. That is why the interpretation is not accurate. People who really know should not give secrets to a stranger, only to members of their family. (1979:68-69)

Another person wrote:

> I feel that the author does not know what he is talking about because the author, in talking about Samoa, mentioned . . . work which was done in Manua. Western Samoa is very different from Manua and American Samoa. (1979:69)

For authors who complain about the habitual indirect response of natives, the following insider's comment provides a more plausible and verifiable cultural explanation than the psychological assumptions which are often suggested in the literature:

> We (Laotians) consider it polite, Westerners consider it not frank . . . Being in that kind of society with these values, we have learned to understand the message that other people are sending without it being stated in words. It is not so much what you say but the way you say it that counts. (ibid.)

A case in point is illustrated by errors made by the late Margaret Mead in her Samoan data as revealed in a review by Derek Freeman (1972:70-78). Freeman gives a detailed comparison of the ethnography of native terms and translations provided by an

informant named Mea with native explanations for these same terms and translations which were collected by further cross-checking with other informants. This exercise reveals: (a) the pitfalls that a stranger-ethnographer is likely to encounter in an alien culture; (b) that native informants may not be aware of, or able to delineate subtle linguistic nuances when interpreting terms for an ethnographer; and (c) that the symbolic cultural transformation which a linguistic term undergoes due to context may be missed.

The anthropological record is replete with evidence of misdecoded verbal and nonverbal behaviors of members of unfamiliar cultures which has resulted in unwarranted assertions and value judgments about such cultures. The clue to this dilemma may lie in our ethnocentric designation of other cultures as "simple societies," implying that they are "easy to understand." But are they? Holistic-empiricist attempts to understand simple societies suggest that the answer is a resounding "no." Anthropological ethnocentrism *vis-à-vis* research communities is all the more dangerous precisely because it often goes unchallenged or is clothed in the garb of "scientific" detachment. We paternalistically write of "my people," "my tribe," or of ourselves as the "prestigious visitor," "honored guest," and "gracious anthropologist" without conceding our veiled "superiority" over the people we study. Whether or not an ethnographer employs the subjective and pejorative terms "savage," "primitive," "backward," "nonliterate," "technologically simple," "uncivilized," "non-Western," or "small-scale" societies, all such euphemisms entail the same conceptual image of the research communities as manipulative objects to be studied at will.

Ethnographers have often confessed to being irritated by natives who do not live up to an image of the good native. The native, not the stranger and unwelcomed visitor, gets the blame:

> . . . the anthropologist may find it difficult getting honest answers to his questions, and may suffer the unwanted attentions of local snobs who see themselves as heralds of a new and progressive order. Several of these, dressed in European fashion and aggressively mouthing their Portuguese, used to crowd around us during our first visits to the hamlets. But when they realized we valued real Tetum customs above pseudo-European ones, they left us alone. (Hicks 1976:14)

Few anthropologists would honestly concede the right of a "native" community to object to being studied without the ethnographer viewing such action as a threat to his project. Hence, the often caustic comments leveled at intractable and uncooperative "informants" in ethnographies (Chagnon 1968; Foster 1979; Rabinow 1977; Turnbull 1972).

In conclusion, only an unsuccessful stranger-host relationship is likely to compel a noted anthropologist to depict a research community as "this strange and outwardly rather horrid society," whose two catechists "Both wore those indisputable signs of Christianity and progress, pants, and . . . carried rosaries to show how holy they were"; a society moreover that appears "to have disposed of virtually all the qualities . . . that differentiate us from other primates," and in whom the ethnographer could not find "one lingering trace of humanity." The ethnographer could not believe he "was studying a human society; it was rather like looking at a singularly well-ordered community of baboons" (Turnbull 1972; 114, 228-229, 234, 236). This may represent an extreme case of cathartic reporting, but is illustrative of the truism that anthropological research "makes extreme demands on imaginative intelligence," as Rodney Needham has recently pointed out, "and in the crucial setting of field research it can subject the investigator to the severest intellectual and moral test . . . of his life" (1981:29). It is thus reasonable to suspect that much to the detriment of the discipline, most of the unsatisfactory results from fieldwork are due to difficulties in stranger/host relationships.

EPILOGUE

A commonplace reaction to critical comment is to demand that the critic offer a better mousetrap. But the role of critical analysis is not to revolutionize literature or art by substituting himself as a better author or artist. As is true for the role of culture in human development, the viability of anthropological fieldwork as a principal tool in ethnographic research rests on the complementary contribution of all of its diverse practitioners, and not on the individual prescriptions of only one of its adherents who can offer no more than a pointer to some of the shortcomings in the method of data collection and presentation.

There is no panacea suggested here apart from the need for ethnographers to specify and validate their data bases in a manner that increases the reliability not only of the proffered analysis of alien cultural phenomena, but also of the raw "social facts" constituting the data base. The stuff of anthropological fieldwork are the field journal and field notes, and since monographs are extrapolations of the content of these records, it is essential that all material for clarification of otherwise unverifiable assertions ought to be adduced from these records in the absence of other "tests of goodness."

Due to imprecise presentation of field reports, we often cannot distinguish between emic normative statements and etic explanatory assertions. Fieldwork as qualitative research is not mutually exclusive of the quantitative aspect; yet data are often

vaguely presented as "native opinion holds that. . . ."; "villagers state that. . . ."; or "sacred twines represent the unity of the tribe" without any corroborative evidence as to the sample population that constitutes the basis of these assertions, the number of informants who agree or disagree over such "facts," or how many households conform to or deviate from a stated norm. Only the field-worker can supply such substantive clarificatory data, and those who take care to do so enhance the reliability of their fieldwork.

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