

Julian Steward and American Anthropology:
The Science of Colonialism

by

Marc Pinkoski
B.A., University of Alberta, 1995
M.A., University of Alberta, 1999

A Dissertation Submitted in Partial Fulfillment of the
Requirements for the Degree of

DOCTOR OF PHILOSOPHY

© Marc Pinkoski, 2006
University of Victoria

All rights reserved. This dissertation may not be reproduced in whole or in part, by
photocopying or other means, without the permission of the author.

Julian Steward and American Anthropology:
The Science of Colonialism

by

Marc Pinkoski
B.A., University of Alberta, 1995
M.A., University of Alberta, 1999

Supervisory Committee

Professor Peter Stephenson (Co-Supervisor), Anthropology

Professor Michael Asch (Co-Supervisor), Anthropology

Dr. Karena Shaw, Environmental Studies

Professor Robert Walker, Political Science

Professor Regna Darnell, Anthropology, University of Western Ontario

Supervisory Committee

Professor Peter Stephenson (Co-Supervisor), Anthropology

Professor Michael Asch (Co-Supervisor), Anthropology

Dr. Karena Shaw, Environmental Studies

Professor Robert Walker, Political Science

Professor Regna Darnell, Anthropology, University of Western Ontario

ABSTRACT

Demonstrating a lacuna within the discipline of anthropology regarding its connection to colonialism in North America, this dissertation analyses Julian Steward's *oeuvre* and theorises him in four novel ways. First, his life-work is introduced with a focus on his representations of Indigenous Peoples. Second, his life-work is contextualised with respect to American federal Indian policy. Third, Steward's evolutionary theory is shown to have been designed as an explicit counter to Boas' method, belying a Spencerian biological analogy, and placing him outside of the "Americanist tradition." Finally, the culmination of Steward's method and theory, heralded as an objective approach to understanding Indigenous Peoples social organisation and the "scientific" method of anthropology, is exposed as a programmatic of the US Department of Justice in proceedings before the Indian Claims Commission, and showing it as a colonial science. Archival material regarding Steward's involvement in the Uintah Ute, Dockets 44 & 45 before the Indian Claims Commission, forms the data for this exposition. Exposing the connection of Steward's work to US government policy begins to fill the gap regarding anthropology's connection to colonialism in North America, and prompts a serious reconsideration of the discipline's method, practice, science, history, historiography, and curriculum regarding Indigenous Peoples.

Table of Contents

Front Matter	
Supervisory Committee	ii
Abstract	iii
Table of Contents	iv
Acknowledgments	vi
Dedication	vii
Chapter One. Introduction	1
i. Introduction to the Problem	3
ii. Julian Steward and the Discipline	9
iii. The Disciplinary Lacuna	20
iv. Historical Context of Steward's Work & Outline of Dissertation	23
Chapter Two. Becoming an Anthropologist: Steward's Work pre-1935: Anti-Diffusionism, Misogyny, and the Representation of Indigenous Peoples	30
i. Steward's Early Training	30
ii. UC-Berkeley	32
iii. Odd Jobs: Universities of Michigan, Utah, and Berkeley	40
iv. Steward's Theory 1929-35	45
Chapter Three. Steward and the Bureau of American Ethnology: Representations of Indigenous peoples (1935-46)	54
i. The Bureau of Indian Affairs (BIA) and American Indian Policy	54
ii. Steward and the BIA	58
iii. <i>Basin-Plateau</i>	66
iv. Fieldwork	73
v. <i>The Handbook of South American Indians</i>	74
Chapter Four. Steward and Columbia University: anti or pre-Boasian, 1946-52.	79
i. Columbia University	80
ii. Steward and Columbia	86
iii. Theory	90
A. "Cultural Causality and Law"	91
B. "Evolution and Process"	95
C. <i>Area Research: Theory and Practice</i>	97
D. "The Levels of sociocultural Integration"	102
iv. Steward and Spencerian Social Theory	106
Chapter Five. Steward and the Indian Claims Commission, 1949-57	115
i. The ICC and US Federal Indian Policy	115
ii. Steward and the Indian Claims Commission	118
iii. Steward and Indian Claimants	123
iv. Steward's Supporting Cast	129
v. Steward and the Uintah Ute Case	132
vi. The Ute Decision	136
Chapter Six. The Paiute Cases and the University of Illinois	141
i. UIUC and Steward's Research Crew	141
ii. The Paiute Cases	144

iii. The Levels of Sociocultural Integration	152
iv. Disciplinary Response	159
Chapter Seven. Steward's Theory of Culture Change	163
i. <i>Theory of Culture Change</i>	163
ii. Steward v. Boas: Method and Theory	167
A. Aesthetic vs. Affective Impulse	170
B. Comparisons (with the age of Enlightenment) and Methods.	173
C. History and the Common Bond of Humanity	177
iii. Application of Theory and Method	178
Chapter Eight. Conclusion. American Anthropology and Colonialism	183
i. Anthropology and Colonialism	183
ii. A New Disciplinary Method	185
Bibliography	195

Acknowledgements

I wish to acknowledge the assistance I have received from the following people in preparing this dissertation, and for their tremendous help throughout the completion of my doctorate: First, I thank Chris Prom and the staff at the University of Illinois Archives for their assistance with the Steward Papers; and the library staff at the University of Victoria. I also thank David Williams and Pat Swift, Tony Fisher, Samantha Bailey, Marie Page, and Barbara Dolding. I am indebted to my parents, Raymond and Lenora Pinkoski, and to Michael and Margaret Asch, Peter Stephenson, Hugh McKenzie, Shauna McGarvey, Rob Wishart, Seth Asch and Robert Hancock. Finally, I would like to thank my committee, and particularly my external examiner, Regna Darnell, for their careful and engaged commentary on my work and for their keen insights and suggestions for improving this dissertation. Thank you all for your love and support.

For Cohen

Introduction

This dissertation is an analysis of the counter-pose to Franz Boas in American anthropology, an analysis of the “scientific” method of Julian Steward. My analysis of Steward theorises him within several contexts that have been omitted or avoided in the historiography of the discipline. The first concerns the disconnect between the discipline’s explicit recognition that Steward’s fieldwork practices are known to be methodologically unsound (Kerns 2003; Thomas 1983; Rusco 1999) and have led to disproved conclusions (Bettinger 1999; Thomas 1983; Clemmer 1969, 1999), and the fact that his work is consistently represented as containing the most objective, “scientific” method within the canon (Kerns 2003; Carneiro 2003; Sidky 2005).

The second context concerns the connection between Herbert Spencer’s evolutionary model and Steward’s cultural theory; there is an existing fidelity between the two that is explicitly misrepresented within the discipline (Harris 1968; Fried 1968; Ortner 1984; Bohannon and Glazer 1988; Trigger 1998; Fagen 1998; McGee and Warms 1996, Sahlins and Service 1960). The third omission is the lacuna regarding Steward’s work for the US Department of Justice, employment, where, for seven years, he generated testimony and strategy for the US government to deny American Indian land rights (Ronaasen et al 1999, Clemmer 1999, Pinkoski and Asch 2004).¹ Finally, the fourth is an omission of practice, and it is the disregard of Asad’s (1973) plea for anthropologists to examine the “structure of power represented by the colonial system,” and the failure of the discipline to examine its science of the as one such “structure of power.” Taken together these

¹ He started work for the Department of Justice in late 1949.

omissions misrepresent Steward; but more importantly, they create an elision on the topic of the relationship between American anthropology and American Indian policy (or colonialism) within the historiography of the discipline.

The following analysis is offered as a contribution to the enquiry represented by many recent histories of the discipline of anthropology in North America. Strong biographical analyses (Darnell 1990, 1998, Peace 2004, Cole, Kerns 2003, Handler 2000) have been met with detailed institutional histories (Stocking 1968, 1995; Darnell 2001, Price 2004, Trencher 2002; Harrison and Darnell 2006) that highlight and analyse the roles of anthropology and anthropologists in North America over the last 100 and some years. Building upon these attempts to uncover disciplinary genealogies and to historicise the various traditions of the discipline, I offer the following analysis of Steward's theory: I claim that Steward's approach can be best understood as akin to the evolutionary pronouncements of W G McGee, his science mimicking a Spencerian biological analogy, and contextualising his method as pre- or anti-Boasian. Further, Steward's defrocked method was designed and deployed in the service of US colonialism. Finally, the discipline has not adequately addressed these facts within its historiography and curriculum, and as a discipline we have yet to acknowledge or adequately engage the implications of the relationship between anthropology and colonialism in North America.

i. Introduction to the Problem

The American anthropologist, Julian Steward (1902-72) is most often remembered as a “scientific” anthropologist. Steward promoted this image himself, premising his distinction from his contemporaries on what he called his scientific method for the discipline (Steward 1950, 1955). Seemingly accepting of this self-identification, the discipline overwhelmingly characterises Steward as a scientific, and objective anthropologist (Turney-High 1940, Harris 1968, Trigger 1988, Ortnor 1984, Wolf 1998, Carneiro 2003, Kerns 2003, Sponsell 2006, Haenn and Wilk 2006).

Claims of Steward’s “objectivity” are based on an interpretation of his ethnographic and archaeological work in the American Great Basin, work that resulted in the seminal ethnography, *Basin-Plateau Aboriginal Sociopolitical Groups* (1938, hereafter *Basin-Plateau*). They also stem from his monumental six-volume edited collection, *The Handbook of South American Indians* (1946a, 1946b, 1948a, 1948b, 1949, 1950), *The People of Puerto Rico* (1956), and his edited three volume-collection on modernisation and development (Steward 1967a, b, c). And, from the many of his foundational essays written during his 44-year academic career, exemplified by his dozens of contributions to *American Anthropologist* and his collection of his essays, *Theory of Culture Change: The Methodology of Multilinear Evolution* (1955).

From early in his career Steward’s work was heralded. This is illustrated in the *American Anthropologist’s* review of *Basin-Plateau*, where it is claimed that the monograph is

“monumental,” and the reviewer eagerly recommends “it without hesitation as a model for similar works which must and will follow” (Turney-High 1940: 136). Explaining Steward’s project and its relative worth, the reviewer states,

... Steward’s acumen is sharper than that of the average field ethnographer...His sociologic data and their analysis are set down with insight based on intimate knowledge. For this region at least he has shown where and why band organization exists and where and why it does not exist, where population has agglomerated and why, what forces geographic and culture make for the cohesive social groups and which are centrifugal. He has shown in a way which might astound some determinists how and why polygyny, monogamy, and polyandry exist within the same group among these matrimonially realistic people. Indeed, there is a ring of realism to all his people. His Shoshoneans, driven by victual desperation, whose economy was “gastric,” could afford to be nothing else.... This is a work which can honestly be called magnificent. I intend to refer to it again and again. The anthropologist should not only read it with profit but should refer his social science friends to it. It is a genuinely scholarly job, free from several faults which would have been excusable under the circumstances, a mine of factual information, and a sound analytic effort (*ibid.*: 138).

Demitri Shimkin (1964), author of the introductory and contextual essay in Steward’s *festschrift*, also reports that Steward’s method was strong because it was created through

“[a]lmost two years of arduous ethnography [sic] ... which later culminated in a fundamental monograph on the social structure of a sparse population dependent on seedgathering” (3); further edifying the ethnography within the canons of the discipline is Manners’ commentary in Steward’s extensive obituary in *American Anthropologist*, identifying it as a “classic” that “demonstrated the analytic value of a cultural ecological orientation” (Manners 1973: 890).

Steward’s later work, exemplified most fully by his text *Theory of Culture Change*, has created a foundation for the discipline by purporting to offer a scientific method for the study of society in relation to environment (Haenn and Wilk 2006).² This perception is represented uniformly across the discipline, and is demonstrated by accounts such as Ortner’s, who recorded Steward’s influence in her period-piece, “Theory in Anthropology Since the Sixties,” stating that he

emphasized that specific cultures evolve their specific forms in the process of adapting to specific environmental conditions, and that the apparent uniformity of evolutionary stages is actually a matter of similar adaptations to similar natural conditions in different parts of the world (1984: 132).

Or Eriksen and Nielsen’s record, in their *A History of Anthropology*, where they state

² The implications of these findings on the importance of Steward’s continued prominent role in the discipline is articulated exactly in the recent text *The Environment in Anthropology* (Haenn and Wilk 2006), when the authors specify the “Theoretical Foundations” by extending centrality to Steward. They explain “we begin here with Julian Steward’s work dating from the 1950s, because his ideas have had such an enduring effect on anthropological approaches to the environment. This selection provides the outline of Steward’s idea of a ‘culture core,’ those cultural features which articulate most closely with a specific environment” (3). To reinforce Steward’s place within their text, chapter one is a reproduction of Steward’s “The Concept and Method of Cultural Ecology.”

Steward developed a theory of *multilinear evolution*, based on archaeological, historical and ethnographic evidence... By limiting his generalisations to a few important aspects of the cultures he studied and restricting the scope of his theory to societies with comparable natural preconditions, he succeeded in building up an evolutionism that did not lead to speculative generalisations that could be easily falsified (2001: 81, their emphasis).

Typically, Steward's method is understood to offer an "objective" ethnographic portrayal of the Indigenous Peoples of the American Great Basin. It is believed that his fieldwork led to strong, objective descriptions, and that extensive analysis, through the rigorous method of cultural ecology, generated "nomothetic" rules of culture change – a method that prompted Harris (1968), for example, to observe that

... Steward's 'The Economic and Social Basis of Primitive Bands' must be reckoned among the important achievements of modern anthropology. It constitutes the first coherent statement of how the interaction between culture and environment could be studied in causal terms without reverting to a simple geographic determinism or without lapsing into historical particularism.... Despite subsequent critical evaluations of certain aspects of Steward's data, the strategy of Steward's explanation continues to warrant approval (Harris 1968: 666-7).

The cultural laws or rules of evolution that Steward devised are well recorded as foundational in the history of American anthropology because of their presumed objective, scientific method for understanding social organisation. For example, though critical of his work, Thomas states that “Steward’s views have assumed almost monolithic proportions, particularly to general anthropologists working outside the Great Basin. This is understandable and at least in part due to Steward’s overall reputation as a cultural ecologist” (1983: 60). Notably, Trigger identifies Steward’s work as a “more empirical approach to the study of cultural evolution” (1989: 291); and Kerns, the recent award-winning biographer of Steward, describes his work as having “...a propensity for the concrete,” noting that “[h]e used an impressive array of ethnographic and archaeological evidence to support a range of creative, generalizing conclusions about how, in his own words, ‘similar subsistence activities had produced similar social structures’” (2003: 3).

In reproducing Steward’s seminal essay, “The Concept and Method of Cultural Ecology” in *High Points in Anthropology*, Bohannan and Glazer (1988) offer a similar sentiment regarding Steward’s contributions, introducing it with the assertion that his “is a methodology concerned with regularity in social change, the goal of which is to develop cultural laws empirically” (321); and they continue, heralding that “Steward’s concepts of cultural adaptation are theoretically important in that they break the circular argument that only culture can explain culture, which in a sense remains true” (*ibid.* 322). Not to be outdone, Moore effectively sums up Steward’s influence on the discipline when he

states that “[t]oday Steward’s ideas are accepted as basic anthropological insight” (1997: 183), and he concludes that some of his concepts “are the anthropological equivalent of gospel” (*ibid.* 188).

Of all foci of anthropology, Steward’s theory has remained most influential for studies of hunting-gathering societies (Lee and Devore 1968, Ingold 2000; Barnard 2000, Feit 1986, Myers 2004). Here, his work is known to define the social units of “band” societies, and led June Helm, for one, to reminisce that,

The work of the American anthropologist Julian Steward in the 1930s provided the foundation for modern approaches to socioterritorial units and their composition in societies of foragers. Steward sought to determine "cross-cultural regularities which arise from similar adaptive processes and similar environments." From this perspective of "cultural ecology," as he termed it, he analyzed the available literature on hunting-gathering peoples around the world (Helm 1986).

At a minimum, one of the foundational claims that has become canonised within the discipline is that Steward’s theoretical paradigm and the representations of Indigenous societies that flow from it are the result of empirical, scientific analysis and, represent a value-free foundation for the study of society in general and of Indigenous societies in particular. The magnitude of this imaginary regarding Steward’s effect on the discipline has led Myers (2004) to conclude that the Shoshone have become a sort of cultural

“barometer” used to reference social evolution within the discipline; and Ingold (2000) to identify Steward’s work as the “*locus classicus*” within the discipline of anthropology for the comprehension of the social organisation of Indigenous peoples.

In counter-distinction to these dominant representations this dissertation demonstrates that Steward’s paradigm, as it evolved, was politically motivated and intended to present an image of Indigenous peoples that undermined their legal claims to territory and rights. I show that Steward’s work was not objective or neutral, but instead promoted his own aesthetic social preferences, preferences that were couched in assimilation policies, evolutionary pronouncements, and manifest destiny to counter what was the then-dominant anti-evolutionary and cultural relativist anthropology of Boas (Stocking 1968, 1989; Darnell 2000; Sidky 2004; MacDonald 1998).³

ii. Julian Steward and the Discipline

To-date there has been much reported on Steward within the discipline. Because he published widely, generating essays and books that were well read, even foundational to

³ For the purposes of this discussion I am relying on The *Oxford English Dictionary* definition for both “science” and “method,” two themes that are central to Steward’s work, and to how the discipline has reported his work.

Science 4. a. In a more restricted sense: A branch of study which is concerned either with a connected body of demonstrated truths or with observed facts systematically classified and more or less colligated by being brought under general laws, and which includes trustworthy methods for the discovery of new truth within its own domain.

Method 3. a. A special form of procedure or characteristic set of procedures employed (more or less systematically) in an intellectual discipline or field of study as a mode of investigation and inquiry, or of teaching and exposition. **b.** In the titles of treatises or manuals of instruction in an art or science.

the discipline, there is a wealth of commentary and review of his work and of him as a person. He is well understood to have exacted an value-free account of the Indigenous Peoples of the American Great Basin – the Shoshoneans as he called them – distilling an evolutionary theoretical frame that was “scientific” (Kerns 2003). Many report his novelty in the discipline to be his strong method that generated evolutionary pronouncements that had overcome the implicit racism of 19th century unilinear evolutionism (Trigger 1998). Thus branding his work as “neo-evolutionary,” present-day scholars have theorised his work as a significant break from the pronouncements of earlier social theorists such as Tylor, Spencer and Morgan, and claim that Steward’s work is the beginning of modern, scientific anthropology and have reified this notion in the discipline’s pedagogy (Eriksen and Nielsen 2001, Sidky 2004, Sponsell 2006, Bohannon and Glazer 1988, Johnson and Earl 1988, McGee and Warms 1996, Erickson 1998, Barnard 2000, Carneiro 2003, Silverman 2005, Layton 1997, Shimkin 1964, Harris 1968).

Steward has been historicised biographically, with examinations of his personal life and institutional heritage (Shimkin 1964; Manners 1973; Murphy 1977, 1981; Kerns 2003), and theoretically, with a greater focus on his written texts (Harris 1968, Carneiro 2003, Sidky 2004, Clemmer et al 1999, Clemmer 1968, Ronaasen 1992, Feit 1984). These two approaches to theorising him overlap, and a coherent and consistent image of him and his theory has emerged. For example, on the occasion of his 60th birthday, his students and several colleagues presented him a *festschrift*, *Process and Pattern in Culture: Essays in Honor of Julian H. Steward* (Manners ed. 1964). For the collection, Demitri Shimkin,

Steward's University of Illinois colleague, wrote the biographical introduction heralding Steward's fieldwork methods and documenting his incredible contributions to anthropological science (1964: 3-10).

Likewise, in Steward's extensive obituary in *American Anthropologist*, Robert Manners (1973), also details aspects of Steward's life, stressing his contributions to the discipline and celebrating his inconsistencies as a sign of a true scientific method (Manners 1973). Manners' treatment codifies several important notions of Steward within the historiography of American anthropology. He situates Steward within his institutional and genealogical traditions, tracing Steward's connections to UC-Berkeley, Cornell University, the University of Michigan, the University of Utah, the Smithsonian Institute, Columbia University, and finally the University of Illinois; and, he locates Steward within his relationships to his instructors, Alfred Kroeber and Robert Lowie and his many accomplished students, including Eric Wolf, Elman Service, Morton Fried, Robert Murphy, Robert Manners, Stanley Diamond, Louis Faron, David Aberle, Sidney Mintz, Raymond Scheele, Elliot Skinner, Clifford Evans, Frederick Lehman, Pedro Carasco (Manners 1973; Kerns 2003: 242).

Manners also highlights Steward's theoretical contributions, noting his ethnography *Basin-Plateau* (1938), his essay "Cultural Causality and Law: A Trial Formulation" (1949) the text, *Theory of Culture Change* (1955) and the Puerto Rico Project as significant contributions to anthropological theory and method. He claims that it was through writings and projects such as these that Steward was recognised for his

accomplishments and *feted* with the Viking Medal in 1952, elected into the National Academy of Sciences in 1954, presented a *festschrift* entitled, *Process and Pattern in Culture: Essays in Honor of Julian H. Steward* in 1964, and honoured with a journal in his name in 1969 (Manners 894).⁴ It is because of these contributions, but not limited to them, that Manners concludes that

Steward is generally credited with introducing two conceptual terms, *de novo*, into the anthropological lexicon: multilineal evolution and levels of sociocultural integration. His name is also closely associated with the popularization of a number of other terms now widely used in anthropology and related disciplines, such as cultural ecology, the search for regularities, culture type, cultural causality, and the larger context. Whether he invented the terms or not, Steward must certainly share credit for giving to the ideas expressed by them a vitality that they might not have achieved without painstaking explorations and often brilliant analyses. Thus, for example, in his efforts to replace the stultifying culture area concept with the concept of culture type; and his revolt against the restrictions of historical particularism and the perversion of cultural relativism from methodological tool to an immutable principle of identification, Steward helped to place or to keep anthropology within the “sciences” (Manners 1973: 896).

⁴ Named the *Journal of the Steward Anthropological Society*.

In two other personal recollections, Robert Murphy, another of Steward's eminent students, remembers that his professor was a "kindly man" but had "enormous inner tension and conflict" (1977: 15). He, too, recognises *Basin-Plateau* and its "meticulously detailed description of local groups" (Murphy 1981: 184) and "Cultural Causality and Law," what he calls "*perhaps* [Steward's] single most influential article" (*ibid.*: 195), as exemplars of his teacher's prowess, but takes the unusual (but emblematic) approach of describing Steward's life-work by stating:

Steward's study is too well known to warrant detailed recapitulation, but its main argument is that the very structure of Shoshoni society was a reflex of its habitat and exploitative patterns. Possessing none of the technology needed to realize the full potential of the environment, the Shoshoni scavenged from it, subsisting on its sparse game population and its thinly distributed wild vegetation. Steward painted a picture of people reduced to the bare essentials of life, living in a society that was all infrastructure. The Basin Shoshoni had no stable political organization beyond parental authority and the prestige given by age. There were no chiefs, and leadership was a temporary and ad hoc matter, a situation that was consistent with the fact that there were no stable or formally defined socio-political units beyond the conjugal family. The Shoshoni had no tribes, no bands, no villages, no clans, no lineages. They were found scattered in small clusters of a few families each across an enormous terrain extending from southern California to Idaho and Utah;

egalitarianism, individuation, and amorphousness characterized the social system (Murphy 1981: 191).

In the most recent and certainly most complete biography to-date, Kerns (2003) centres on one of Steward's key concepts, the "patrilineal band," and prepares a descriptive analysis of his life. Beginning with his formative years, Kerns argues that Steward was heavily influenced by problems inherent in the daily life of arid environments and the labour that it takes to organise irrigation work to solve them. She demonstrates that his focus on the organisation of subsistence labour and its relationship to the physical environment remained a central component in Steward's *oeuvre* on development and change throughout his career.

According to Kerns, Steward's work with many of the GI's returning to university after the war and the burgeoning field of academic anthropology, spawned numerous influential studies about the nature of Indigenous societies, and materialist analyses about the development and change these societies experienced. His cross-cultural analysis, generating nomothetic explanations of cultural development, differentiated his approach so thoroughly from the dominant Boasian tradition as to develop an entire new method of study. "Cultural ecology," as he called it, became the method to distil levels of multilineal evolution and effectively re-codified evolutionary theory within a scientific rhetoric. Kern's text provides this context; and she systematically undermines Steward's claim to an objective, scientific method for his conceptual basis for the root of society, contending that the "patrilineal band" that he assumed was merely a reflection of his own

social habits and only inferred into his theoretical paradigm. The effect of this assumed model ensconced a male-centred approach to anthropological method, and was replicated and promoted in his theory and practice (Kerns 2003).

There are serious historical and contextual errors in each of these biographical accounts. For example, Manners reports that Steward “was loaned to the Bureau of Indian Affairs [BIA] at the request of John Collier who was then actively involved in the creations of his programs for reform of the Bureau, usually referred to as a New Deal for the Indians” (1973: 892). The “New Deal” or Indian Reorganization Act (IRA) was Commissioner of Indian Affairs John Collier’s attempt to stem federal assimilation policies by reversing the decades old policy of the Dawes Allotment Act (1887). However, Kerns demonstrates that Steward was at odds with Collier’s policies, noting that “[d]espite the 1934 [New Deal] legislation, Steward regarded the assimilation of American Indians as inevitable and as the path to economic improvement” (Kerns 2003: 199). And Steward, himself, recounted that Collier’s

New Deal for the Indians was remarkable in that its policies were even more radical than those of the Russian Revolution. It undertook to redirect culture change toward communal, utopian societies that were presumed to have existed earlier and to establish them within the larger framework of free enterprise... The New Indian policy was messianic, compassionate, intolerant and unrealistic (Steward 1969: 14).

In fact, Steward wrote one report for Collier and the BIA. For that report, Steward was to undertake fieldwork to gather data on contemporary Shoshone political organisation for the BIA, and to offer recommendations for implementing the policies of the Indian Reorganization Act (New Deal), and Collier rejected it saying,

I am tempted to excerpt other and lengthier dicta from Dr. Steward's report, but the one which I have quoted indicates most of the reason why the report does not prepossess me as social philosophy or as factual reporting.

In determining Indian Service policies, and in attempting to evaluate human beings and to chart the future of human spirits, there are needed some endowments of enthusiasm, confidence in the human nature one is dealing with, and social philosophy.

... the shedding of light upon our complicated Indian problem needs something more [than what Steward's report offers]. This is another case showing that achievements in a special science, anthropology or any other, provides no assurance of competency to deal with social problems (Collier (1936), cited in Rusco 1999: 106).

Thus, though recognising Steward's expertise on American Indians, Manners misunderstands or misrepresents Steward's work in the context of the US Indian policy.

Secondly, Shimkin (1964) claims that Steward's expertise as a scientist is based upon an empirical reality. He cites Steward's "[a]lmost two years of arduous ethnography [sic]" that enabled his ground-breaking theoretical pronouncements (1964: 4). However, Steward's fieldwork in the Great Basin was significantly less than Shimkin alleged. Shimkin's error can easily be forgiven, because Steward often overstated his own field experiences; for example, in his one report to Collier he claims that his expertise on social issues in the Great Basin was due to his "ten years' work in the area" (Steward 1936c: 1). For that report to Collier, Steward was to undertake fieldwork in the Great Basin. This was Steward's longest and his self-identified most important experience for garnering data for his ethnography, *Basin-Plateau* (1938). During that trip, Steward and his wife spent from mid-May to September of 1935 in the Great Basin; but, their time was marked by long drives and quick stops in communities hundreds of miles apart from one another. Compounding problems, Kerns notes that Steward had his appendix removed in Salt Lake City during this trip, an illness that cut into his research time and its quality. In fact, according to Jane Steward's journal entries, her husband did almost no fieldwork by September because of his constant movement, hasty visits, and general ill-health (Kerns 2003: 201-3).⁵

⁵ In a detailed history of this report, Rusco (1999) demonstrates that Steward did no specific research for it; and rather, when he did get out of his car and was forced to deal with people face-to-face his time was spent investigating culture trait lists to honour a long-overdue debt to Kroeber. Notwithstanding the very minimal amount of specific work that Steward did for the report, it is important for several reasons. The substance of it contains his analysis of the suitability of the Shoshone to adapt to the new policies of the IRA, showing his antipathy to the Indians of the area and his opposition to Collier's project. Motivated by his firm belief that aboriginal Shoshone culture was "low," with no or a depleted socio-political organisation, he asserted that their then-present cultural form had changed so drastically from aboriginal times that nothing in it was worthy of recognition or preservation against what he considered to be the natural processes of acculturation towards "modern" American culture.

Kerns (2003) demonstrates that Steward's fieldwork used a method that has been seriously discredited. That is, not only did he do no systematic research for this report and others, he explicitly denied the contribution of the women to the society, refused to talk to community leaders, and also lied about who he was working for when he was in the area (Steward 1936: 10, Rusco 92-3). Moreover, as gleaned from journal entries, his fieldwork in the Great Basin amounts to nothing more than very minimal interactions with American Indians over a few months, preferring instead to speak with "white" people to glean his information (Kerns 2003).

Thomas (1983) supports this critique of Steward's methods and of the conclusions drawn in *Basin-Plateau*. He demonstrates that Steward worked with a very limited number of Shoshone informants when doing fieldwork;⁶ and says that

[i]t is particularly critical to note that Steward never worked with the lakeshore adapted Northern Paiute groups, such as the Pyramid Lake or Walker River Paiute. In fact Steward's classic *Basin-Plateau Aboriginal Sociopolitical Groups* (1938) does not even include a sketch of these societies, an extremely important omission. It seems entirely likely that the microenvironments of these large inland lakes fostered a significantly different settlement pattern and probably more complex social organization than Steward's "typical" family band (Thomas 1983: 61).

⁶ Thomas reports that "Steward worked with 40 Western Shoshone, 14 Owens Valley Paiute, 4 Mono Lake Paiute, 3 Southern Paiute, and a single Northern Paiute" (1983: 61).

And, in accordance with Thomas' critique, Steward's student, Robert Murphy notes:

I retraced some of Steward's footsteps when doing research among the Shoshoni in 1954, and found myself unable to collect the same kind of material.... The Shoshoni had not all been settled on reservations, and their small Indian settlements were scattered throughout the towns and ranches of Nevada. This not only continued the dispersed settlement pattern of native times, but kept the people in contact with the land. Not all of the old subsistence activities had disappeared. People still went out each fall to gather pine nuts, some of which were sold on the market as "Indian nuts." The native wildlife had been reduced by the whites, but deer, rabbit and antelope were still taken and constituted an important source of meat during the Depression '30s. People still knew where to find roots, they still used the old springs for water, and even their work for the whites took them into the land. They had not been completely uprooted as most American Indians (1981: 184-5).⁷

Finally, although Kerns' biography addresses many of the failures in Steward's work, like many in his time, to appreciate the role of women in his analysis, and the contribution of women surrounding him, it does nothing to soundly question the basic assumptions that he makes about aboriginal societies as a whole. To this point, a

⁷ The ultimate absurdity of Steward's conclusions about the acephalous nature of the Shoshone are highlighted by Crum (1999) when he notes that "in 1932, some three years before Steward's ethnographic visit to the Great Basin, the Shoshone in northeastern Nevada formed a treaty council to press claims against the for unkept treaty promises" (121), treaties that they had signed 100 years earlier.

discussion of Steward's role in the Indian Claims Commission proceedings and the relationship of his theory to colonial legal ideology are avoided.

iii. The Disciplinary Lacuna

Thus, biographical and theoretical accounts of Steward serve to reinforce a general disciplinary lacuna regarding colonialism and North America. This misrecognition of Steward's work for the US Department of Justice is codified, for example, in Kerns' 400 page biography of him, where only four scattered pages in the entire book address his work for the US Department of Justice in the ICC cases, though he performed this work for at least seven years (Kerns 2003: 247, 259, 282-83). Moreover, when Kerns mentions Steward's ICC work, she contends that his work for the Department of Justice was not to be considered political, but rather understood as "scientific," explaining that it was Steward's "commitment [to science], without regard to politics, [that] helps explain his decision ... to testify for the federal government in the Indian Claims Commission cases" (ibid: 247).

Kerns' biography reinforces the generally accepted gap that has been created through authoritative sources on Steward's life. These include the introductory essay in Steward's *festschrift*, a biographical account that neglects to mention, or fails to understand, the importance of his association with the government (Shimkin 1964); and the recent entry on "Julian Steward" in the *Encyclopedia of Anthropology* (Sponsell 2006), where space is allotted to absolve Steward's relationship to 19th century evolutionary theory, but none is

devoted to an examination of his work before the ICC. In fact, and tellingly, Sponsell alleges that “Steward focussed on traditional culture and *ignored the colonial situation that oppressed indigenous societies*, assuming the inevitability of their sociocultural assimilation or even extinction” (2006: 2129, emphasis mine).

The specific topic of Steward’s work for the government before the ICC is also excluded in the biographies of Steward by Robert Murphy (1977, 1981), neither of which mention the ICC. So too is the topic overlooked in Steward’s obituary written by another of his students, Robert Manners (1973). All of these omissions are significant: First, because of the significant amount of time Steward spent in his relationship with the Department of Justice, employment that reasonably should be included in any biography. Secondly, because of the volumes of academic material that Steward published while he was working for the Department of Justice that had a direct and sometimes *verbatim* relationship to his testimony in claims cases. Thirdly, and incredibly, because both Robert Murphy and Robert Manners, the authors of three of the biographies, worked for and appeared on behalf of the US government in ICC proceedings at Steward’s behest and under his direction.

In contrast to these accounts, I provide an analysis of Steward’s theory as it pertains to Indigenous Peoples and his testimony before the ICC as an expert witness and strategist for the US Department of Justice. I expose his involvement in the Ute (Nos. 44 & 45) and Paiute (Nos. 87, 88, 17, and 100) cases before the Indian Claims Commission (ICC) and emphasise the relationship of his alleged “objective science” to this government

work.⁸ I base this analysis on my archival research of his papers and reports written during this time, an analysis that has been facilitated immeasurably by Kerns' recent biography (2003). As well, I have performed a close-reading of his published *oeuvre*, with particular focus on the representation of Indigenous peoples in his theory, and the connection of these representations to the law. I have also attempted to consult all relevant contemporary and historical disciplinary commentary on Steward published in English.

My approach utilises two methods. The first is "close reading," whereby texts are scrutinised to search for patterns, repetitions, contradictions, ambiguities and similarities (Abrams 1993: 247; Cuddon 1998: 142). The second is a genealogical approach, fashioned by Nietzsche and Foucault and applied most prominently in the historiography of anthropology by Darnell (1999: 3-6); specifically following Darnell's instruction that "[w]e must think historically while we are thinking theoretically" so as

to reclaim the history of anthropology so that it can serve anthropologists as a means of constructing contemporary professional identities upon continuity with the past. Presentism in this reflexive sense, choosing issues for historical attention because they still matter today, is fully commensurate with historicism. It is only when we fail to distinguish the contexts of our own theoretical positions from those of the past that presentism becomes a methodological millstone (Darnell 1999: 1).

⁸ Steward recognised that he worked on "a half dozen cases;" I can also add Docketts 31 & 37, the combined case *The Indians of California vs. The US* to this list making at least 7 cases where he acted. The area of land in question is in the area of many hundreds of millions, land today which now comprises much or all of Utah, Nevada, Colorado, California, and Idaho.

iv. Historic Context of Steward's Work & Outline of Dissertation

When Steward finished his graduate training at Berkeley in the late 1920s, the dominant paradigm in American anthropology was Boasian, which has been explained to have formed “against” evolutionary anthropology by placing emphases on “patterns” and the internal “coherence” of cultures (Darnell 2000: 274). Steward, though trained by one of the most prominent Boasians, Alfred Kroeber, broke from the mould of his contemporaries in the pursuit of what he called the “generation of laws” and taxonomies of social hierarchies (Manners 1973) – an approach that was influenced by his early preparatory schooling in California that promoted ideals of civic duty and manifest destiny (Kerns 1999, 2003).

Spurred from this earlier training, Steward pursued his anthropological research project in an effort to generate regularities of culture change in several significant capacities across the US. After completing his graduate training at Berkeley, he assumed a faculty position at the University of Michigan in 1928, which he left after two years to take an associate professorship in anthropology at the University of Utah in 1931. Again, after two years, he left that position and toiled at odd teaching jobs in and around the San Francisco area during the Great Depression (Kerns 2003).

It was at this time that John Collier became Commissioner of Indian Affairs in 1933 (Philp 1977), and proposed reforms that were an affront to the overt policies of assimilation dominant in American politics at that time (Dailey 2004). Through his 12-year mandate of Commissioner of Indian Affairs, Collier attempted to secure a set of

policies that would ensure Indian self-government, the continuation of traditional societies, and the preservation of cultural and socio-political structures that differed from those of the wider American society (Rusco 1999; Daily 2004; Wilkins and Lomawaima 2001; Sutton 1985). Collier's attempts to reform American Indian policy was based on a questioning of evolutionary pronouncements and policies of assimilation; and by promoting what Collier, himself, called a "confidence in [the] human nature" of all peoples (Collier 1936 quoted in Rusco 1999: 105-6) and a "reverence and passion for human personality, joined with the ancient, lost reverence for the earth and its web of life" (Collier 1947: 7).

In 1935, Steward began the second longest appointment of his career, joining the Bureau of American Ethnology (BAE) at the Smithsonian Institution. He stayed there, as an associate anthropologist, for 11 years; including working for less than one year as a liaison between it and the Bureau of Indian Affairs (BIA) during the Collier administration. It was during this time that his ethnography, *Basin-Plateau* was published by the Smithsonian, and that many of his other articles were written and his edited six-volume *Handbook of South American Indians* was compiled. It was at this time that he joined the exclusive Anthropology Society of Washington (ASW) and founded the Institute for Social Anthropology (ISA) funded through the US Department of State (Kerns 2003: 225) and the Society for Applied Anthropology (Trencher 2002: 451).

In 1946, Steward retreated from applied work and joined the faculty of anthropology department at Columbia University. It was during his six-year stay that he began to

articulate his theoretical position in explicit terms, and when he published some of his most complete theoretical works – including the essays “Cultural Causality and Law” (1949) and “The Levels of Sociocultural Integration” (1950, 1951), and the monograph, *Area Research* (1950), all of which have been identified as some his most important essays and works (Manners 1973; Murphy 1977, 1981; Clemmer et al 1999, Kerns 2003).

It was also at Columbia that Steward was approached by the Department of Justice to act as a strategist and expert witness in ICC cases against Indian interests in their lands in 1949. Steward’s work for the Department of Justice prompted him to leave Columbia and take a “research professorship” at the University of Illinois (UIUC), a position that greatly reduced his administrative duties, offered less teaching, more money, and enable him to bring several of his most accomplished students from New York to work in Urbana-Champaign as his assistants on the ICC cases.

It was there, at UIUC, that Steward reformulated his published work to date, and re-issued it in his monumental theory text, *Theory of Culture Change* (1955) couched in his new method of “cultural ecology,” set-up explicitly in opposition to Boasian anthropology, and masquerading as “objective science.” It is also in *Theory of Culture Change* that at least two chapters, chapter 6 “The Great Basin Shoshean Indians” and chapter 2 “Concept and Method of Cultural Ecology” were included that had previously appeared as US government briefs, reports, or “statement of defense,” at times *in verbatim*, in ICC proceedings to deny Indian interests in their lands without this admonishment in the text (Ronaasen et al 1999; Clemmer and Myers 1999).

The outline of the dissertation follows these changes in Steward's career, marking his switch from theory to application and following the *post facto* development of his academic work in the service of the government – following through to the culmination of his applied and theory work in his major anthropology text, *Theory of Culture Change* (Steward 1955).

Chapter Two traces Steward's training, detailing his representations of Indigenous Peoples in his early work and establishing an historical foundation with which to discuss his major theoretical contributions to the discipline; it positions him in relation to Alfred Kroeber and Boasian anthropology, and introduces his post-fieldwork axiomatic claim that all humans live in a base group organised with territory ownership and jurisdiction. His early life and training are important for establishing an accurate chronology and history of his fieldwork experience and for contextualising his relationship with the rest of the field.

Chapter Three discusses Steward's extremely productive 11 year stay (1935-46) at the Bureau of American Ethnology (BAE) in Washington, DC. I examine his work as a liaison to the Bureau of Indian Affairs (BIA) during the implementation of Collier's Indian Reorganization Act, and introduce his theories about Indigenous Peoples within the context of American Indian policy. Finally, I trace through the structure that he established in the discipline during this time, where he was able to amass such support

that he attempted to take over the American Anthropological Association (AAA) without an academic post in 1945 (Trencher 2000).

Chapter Four focuses the discussion on Steward's tenure at Columbia University (1946-52), situating him institutionally within the storied department of anthropology, and further exploring his relationship to Boasian anthropology. For my illustrations, I offer an analysis of four of Steward's foundational works published during this time: "Cultural Causality and Law" (1949), published in *American Anthropologist*; "Evolution and Process" (1953), published in Kroeber's 1000-page tome, *Anthropology Today*; also, the monograph *Area Research: Theory and Practice* (1950), published by the Social Sciences Research Council (SSRC); and finally, "The Levels of Sociocultural Integration" (1951), first published in *Southwest Journal of Anthropology*. To end the chapter, I theorise Steward's work in relation to the social theory of Herbert Spencer, ultimately questioning the accuracy within which Steward's "neo"-evolutionary theory is reported and understood in the discipline (E.g. Sponsell 2006; Trigger 1996; Bohannon and Glazer 1988; Erickson and Murphy 2001).

Chapters Five and Six are primary-source document analyses of Steward's work for the Department of Justice, 1949-56. These chapters are based on research in Steward's archives, and present an analysis of his work for the US Department of Justice in assisting with the development of legal and ethnographic arguments before the Indian Claims Commission (ICC). Chapter Five is an original analysis of his first cases before

the ICC regarding the Ute, ICC Dockets 44 & 45, and also provides background to the ICC in relation to American Indian policy.

Chapter Six is an analysis of Steward's work for the US Department of Justice before the ICC for the much larger and better organised Paiute cases, Docket Nos. 87, 88, 100, and 17 before the ICC.⁹ This analysis shows that Steward's relationship with the government was intimate; moreover, it shows that Steward's role was much beyond that of an expert, and he quickly attained a position as adviser to the government and overlord to many in the anthropological community. The chapter also introduces some of the disciplinary responses to the involvement of anthropologists in the ICC proceedings.

Finally, Chapter Seven is a discussion of the culmination of Steward's theory and method through an exploration of his major text, *Theory of Culture Change*. The chapter offers an analysis of the monograph, focussing on the theory, method and application as articulated and advanced in the book. It explains his method as a specific counter to that of Franz Boas, and demonstrates that there is no separation between his alleged "objective" science and his applied work for the US government to deny American Indians rights to their traditional lands. The ramifications of these findings are significant. This is because Steward's academic work, that which claims the acephalous nature of the Shoshone and that which is heralded as objective and foundational science within the

⁹ Steward also worked on the combined *Indians of California* case. This case is remarkable for several reasons. First, one of the experts for the "Indians" was Alfred Kroeber and this pitted Steward against Kroeber in court. Secondly, this case involved several other notable anthropologists, such as Omer Stewart and Ralph Beals. This case raised the profile of the anthropologists' involvement in the ICC proceedings and spurred Sol Tax, among others, to raise the issue to the general membership of the American Anthropological Association. I have not dealt with archival material of this case, but rather have focused on the disciplinary response to the involvement of anthropologists working for the government.

discipline, was also, precisely, the US government's defence, at times *in verbatim*, before the ICC to deny Indian rights to land.

Chapter Two. **Becoming an Anthropologist: Steward's Work pre-1935: Anti-Diffusionism, Misogyny, and the Representation of Indigenous Peoples**

Steward's early training in the biological sciences, coupled with the social evolutionism taught in his prep school armed him with a somewhat unique approach to anthropology at the time that he became a professional anthropologist. At Berkeley, he would come to counter his professors' claims, demanding that they must "explore causes" or "make laws" and "generalizations," immediately setting himself up in opposition to orthodox Boasian anthropology (Darnell 2000). In this chapter, after introducing Steward's early biographical history, I discuss his anthropological training at Berkeley and his relationship with Alfred Kroeber. Secondly, I discuss Steward's first published works, identifying them as written against Boasian diffusionism and being heavily misogynistic, and racist. Finally, I introduce Steward's axiomatic statements regarding human social organisation which were written directly after he completed his fieldwork.

i. Steward's Early Training

Julian Steward was born in Washington DC on January 31, 1902. His parents both worked for the Department of the Interior of the US federal government. His mother, Grace Garroitt, was a clerk in the Pensions Office; and his father, Thomas Steward, was Chief of the Board of Examiners of the US Patent Office. Steward's parents were well educated – his father particularly, as he attained a degree in civil engineering, and a bachelor's and a master's of law (Kerns 2003: 19-26). However, by the time that Julian was 9 years old his mother had converted to Christian Science, a change which precipitated his father's separation from the family. Kerns (2003) reports that despite

Grace Steward's commitment to Christian Science spiritual beliefs, neither Julian nor his father followed the practices of the church (27).¹⁰ In fact, shortly after Grace Steward converted, marital problems caused a lengthy separation of Steward's parents. The schism between his father's "scientific" interests and his mother's "religious" beliefs represented a deep division in Steward's own approach, prompting Kerns to note that

He would always equate religion with dogma and irrationality. When his skepticism, his habit of questioning, collided again and again with his mother's certainty and habit of quoting Mary Baker Eddy, he angrily concluded that she accepted dogma at the expense of reason (Kerns 2003: 29).

Shortly after his parents divorce in 1917, Steward applied for admittance to a prep school near Death Valley, California, and the following May, he enrolled at Deep Springs Preparatory where he began to form an important association with the school and the imaginary of the Great Basin that he cherished throughout his life (Shimkin 1964; Manners 1973; Murphy 1977, 1981; Kerns 1999, 2003; Clemmer et al 1999). The school instilled in him a belief in liberal humanism and bestowed on him the responsibility of civic duty burdened onto the elite of American society. Steward's stay at Deep Springs is consistently identified as formative for his interest in ecology, hydraulic systems, and the organisation of male labour. This period is also seemingly represented as the happiest time in his life (Shimkin 1964; Manners 1973; Murphy 1977; Kerns 1999, 2003).

¹⁰ I accept Kerns' (2003) detailed historic account of this time in Steward's life. The specifics of this time-period conflict in various accounts; however, Kerns' version is significantly more detailed, better researched, and thoroughly referenced, and as such I adopt it where the narratives of Steward's life differ.

In 1921, after three years at Deep Springs, Steward graduated with a scholarship from the Telluride Institute to attend UC-Berkeley. While there he took his first, and only, undergraduate anthropology course co-taught by Boasian anthropologists Alfred Kroeber, Robert Lowie, and Edward Gifford. Within 18 months, however, Steward left Berkeley to continue his studies at Cornell University; and, once in Ithaca, Steward eagerly moved into Telluride House, and there he was able to rejoin many of his friends from Deep Springs (Kerns 2003: 63). When Steward entered Cornell in 1922, it is reported that he had every intention of pursuing a law degree so as to fulfill his social service obligation instilled in him at Prep School; but, when the opportunity to take the law classes finally availed itself he declined, and instead majored in zoology and geology (Manners 1973: 889). Kerns reports that Steward did not leave much of an imprint on Cornell, nor did he explore much of the ethnographic or ecological setting of up-state New York, instead he devoted himself to his studies (Kerns 2003: 62-70).

ii. Berkeley

Steward returned to Berkeley in 1925, and his second tour there was marked by several important features for his training as a professional anthropologist. First, the anthropology department's focus permitted him to become familiar with the intimate workings of Boasian anthropology through the on-going culture trait studies of California, the Southwest, the Great Basin, and the Great Plains. And, he did partake in numerous short archaeological and ethnographic studies in the department's focal ethnographic areas gaining a wide range of experiences in the area (Kerns 2003).

Secondly, Steward's tenacity combined well with Kroeber's connections, and permitted him to gain a variety of professional experiences, such as editor and organiser of anthropological projects; and because his interpretation and analyses developed in opposition to the Boasian approach taught by his professors, his approach established him as a unique student and scholar at the time. Intellectually, Steward's position relative to Kroeber was often contrary, and the relationship was strained at various points, particularly noted in his attacks on Kroeber's work (1929, 1955, 1955b), and in what he ruefully remembered as "Kroeber's reluctance to explore cause and effect relationships in culture" (Manners 1973). Despite their differences, however, Steward gained from Kroeber's advice and guidance as a mentor, and Steward often sought Kroeber out in this role. Kroeber often accommodated Steward's requests by providing professional and personal advice. Thus, notwithstanding their differences in accepting the efficacy of evolutionary comparisons, Kroeber's support for Steward greatly assisted his student's career development. This is seen, for example, in Steward's first academic position at Michigan, which was a direct result of Kroeber's intervention; so too were his soon-to-come professorship at the University of Utah, and his temporary teaching at Berkeley during the Great Depression. It is fair to say, that despite their differences in theoretical approach, theirs was a professionally supportive relationship, facilitated by what appears to be Kroeber's life-long generosity.

Steward's graduate training at Berkeley combined anthropology and geography almost equally. At this time, both departments were run by men of similar ilk: Kroeber's hold

on the anthropology department was matched by Carl Sauer in the geography department, and

both men shared many interests, particularly in culture history, as well as a strongly Germanic worldview. Both were bilingual, having grown up in families in which German was the language of the home. At one time they discussed the possibility of a joint department... (Kerns 2003: 88-9).

Steward's relationship with Kroeber was both professional and personal. For example, when Steward (1973) recounts his meeting of the Kroebers in his biography of Alfred, he recalls,

It happened that I first knew Theodora Kroeber in 1926 when we both took a seminar under Kroeber, though she had known him slightly a few years earlier. They were married in 1926. After that time my friendship with the Kroebers continued (1973: vii).

Though they remained professionally respectful to one another, when questioned deeper examples from their relationship also demonstrate the strong divide in their theoretical approach, and Steward's disdain for Kroeber's methodology of culture history. This opposition is seen in Steward's (1961) obituary of Kroeber in *American Anthropologist*, where Steward offers a serious critique of Kroeber's theory and ultimately undermines the validity of his supervisor's approach (ibid.: 1049). In fact, Steward champions his

own work to conclude his obituary of Kroeber, listing his reservations with Kroeber's theory, by saying

In histories of social science, appraisals of the great minds tend strongly to show the interests and preoccupations of the historians. The present article is a very humble attempt to suggest some of Kroeber's main achievements, but it cannot claim objectivity. The comments on Kroeber's achievements are made in the light of my own view that causes, explanations, or processes which are not peculiar to each relativistically unique culture can be identified. This is a fairly new approach, and whether the mounting body of causal hypotheses represent a new trend will be determined only by time.

In spite of my views, which differ in some ways from Kroeber's, I am deeply convinced that Kroeber's five hundred odd publications are, and will be for many decades, an almost inexhaustible mine not only of information but of problems, concepts, and hypotheses which have not yet made sufficient impact upon the world of scholarship (Steward 1961: 1058-9).

Demonstrating an even stronger animus is Steward's biography of Kroeber, which was written in response to perceived failings in Theodora Kroeber's (1970) biography of her late husband (Steward 1973: xx). Here, Steward states explicitly that Kroeber's theory had not been sufficiently addressed in the original biography and Kroeber's life needed to be re-theorised, correcting what Steward perceived as its faults.

Finally, another example of their relationship is seen in this letter from Steward to Marvin Harris, written in 1969 to address Harris' recently published *Rise of Anthropological Theory*. In the letter, Steward writes,

As a student of Kroeber and Lowie, my first year, 1925-6, was loaded with area courses which lacked anything nomothetic. Despite the Boasian orientation of Kroeber and Lowie, which I did not know at the time, I entered anthropology hoping to find a means of explaining cultural development. At the end of the first year I asked Kroeber when I would learn about explanations, upon which he said in some horror, "What do you mean? I deal with cultural phenomena, not explanations." Lowie was actually far more sympathetic to my interests, as shown by his support of my still unpublished thesis *The Ceremonial Buffoon of the American Indian*, which ventured reductionism in tracing recurrent themes of humor to inherent human psychological constants or potentials. Kroeber argued vigorously against my endeavor....

Of course I was overwhelmed by Kroeber's erudition and in my OB I tried to treat him kindly. My point about his anticipation of problems and hypotheses is that he did again and again amass data only to stop short of drawing any conclusion. For example, in his *Primary and Secondary Features of Australian Social organization*, the nature of his distinction

implies a causality. His several pages comparing Old and New World achievements in Anthropology lays out rather precise parallels but then stops. I think you would say of these and other cases that the pull of the Boasian relativism was too great.

Your speculation about how I got that was only partially correct. First, I was interested in causes before I really got into anthropology, and was quite disturbed that Kroeber repudiated this interest. Second, the key factor of the national intellectual climate was the depression, which started after I finished my studies at Berkeley in 1928. I had taught at Michigan two years, 1928-30, and Utah 3 years, by the time the depression became so acute that everyone was asking Why?, and thinking generally took a sharp Marxist turn. It was during the thirties that Columbia became a communist cell far more than people knew, and, curiously, many adopted the political and economic orientations yet remained thorough-going relativists in their anthropological work. I too read Marx and others but it was dangerous to proclaim a Marxian position.¹¹

In response, it appears that Kroeber chose not to engage or respond to his student's work (Friedman 1987; Kerns 2003).¹²

¹¹ Steward to Marvin Harris, March 8, 1969. This letter was written shortly after Steward had suffered a stroke and was nearly blind, and there are many spelling mistakes in it. I have chosen to correct the obvious spelling errors.

¹² Kroeber (1955) might be the exception in print.

Steward's personal relationship with Kroeber was generally good and their work rapport professional, his relationship with Carl Sauer, however, was not strong.¹³ Even though Steward shared Sauer's method of mapping territories and analysing environments, their theoretical orientation differed diametrically: Sauer was interested in understanding how human use and occupation altered landscapes and Steward was interested in the causal relationship between environment and behaviour, or what he called culture (Kerns 2003). In fact, in the same detailed letter to Harris quoted above, Steward said, "Carl Sauer contributed nothing to my thinking. He has always been no more than an intellectual iconoclast, bent on baiting anthropologists, whatever their views. In fact, geography has never gotten off the ground intellectually."¹⁴

Presenting the details of these relationships is important because of the specific errors of fact in many of the biographical representations of Steward. Steward did not have a good relationship with Sauer. He did, however, have support from both Kroeber and Lowie, though he seems to have had more of a theoretical connection with the latter. That Steward openly criticised Boasian anthropology but continued to receive the support of Kroeber and Lowie indicates a complicated history in the Boasian genealogy.

A final point worth noting about Steward's time at Berkeley is that this is when he met his first wife, Dorothy Nyswander. Nyswander was a behavioural psychologist, nine years older than Steward. Kerns demonstrates that Steward's scientific approach to understanding human behaviour was heavily influenced by Nyswander, an influence that

¹³ Contra Harris (1968), Manners (1973)

¹⁴ Steward to Marvin Harris, March 8, 1969

had been completely omitted from the historiography of Steward. And it is also important to note that Nyswander's psycho-behavioural approach influences Steward's somewhat theoretically anomalous PhD dissertation, on ceremonial Indian buffoonery (Kerns 2003). Nonetheless, Manner's (1973) obituary, Shimkin's "Introduction" to Steward's *festschrift* (1964), and Murphy's two biographical accounts (1977, 1981), all fail to acknowledge the influence of Nyswander in Steward's life. In fact, none of them mention Steward's first marriage at all. This relationship is the first of several examples that demonstrate the fundamental gap in the disciplinary knowledge of Steward.¹⁵

As Steward approached the completion of his graduate training, he intended to submit his dissertation for defence in the spring of 1928, and hoped soon thereafter to marry Nyswander. However, an illness, seemingly set-off by stress and overwork, forced him to take a break from school. To relieve stress, Kroeber and Lowie recommended Steward undertake a fieldwork hiatus to witness Hopi initiation ceremonies.¹⁶ This trip, like all of his documented ethnographic fieldwork, was not particularly successful (Kerns 2003). That is, after arriving late and further succumbing to his "illness," he ended up resting in Salt Lake City with Nyswander for several weeks before returning to Berkeley. The delay from his undefined illness put him behind his self-imposed schedule to finish his dissertation, and he put off completing it for another year (Kerns 2003).

¹⁵ In 1926, after completing her PhD and teaching for one year at Stanford, Nyswander was offered an assistant professorship at the University of Utah, and she soon relocated to Salt Lake City. During that same summer, Steward undertook his first archaeological fieldwork along the Columbia River between Oregon and Washington. Although Nyswander's move happened shortly after they began dating, the couple managed to maintain their budding relationship (Kerns 82).

¹⁶ The hiatus from Berkeley was prompted by a request from Elsie Clews Parsons; Steward did procure some ethnographic data that would later be published (1931c)

Energized after visiting with his girlfriend Steward returned to California and undertook more archaeological fieldwork near Santa Barbara, and he did some ethnographic fieldwork around Mono Lake with the Paiute. It was during this time that he observed that the Paiute had “no agriculture” but did have an extensive practice of irrigation for wild food plants, an organizational feature previously unknown to anthropology (Steward 1930b; Shimkin 1964: 3; Kerns 2003: 98). Ethnographically, this was an important observation for Steward to make, and it helped to further his interest in the relationship between irrigation projects as technological development and social evolution. This was the first of his many theoretical and ethnographic contributions to general anthropological theory.

iii. Odd Jobs: Universities of Michigan, Utah, and Berkeley

At the end of summer 1928, Steward was faced with an unforeseen and incredible opportunity. His graduate school colleague, Anna Gayton, had received a faculty position at the University of Michigan, but had fallen ill. Fortuitously for Steward, Kroeber and Lowie recommended him to replace Gayton for the approaching fall-term, and Steward accepted the offer and left hastily for Ann Arbor. Upon his arrival he was scheduled to teach an ethnology course and to be part-time curator of the Museum of Anthropology.¹⁷ At the age of 26, Steward had his first academic position, one that was very important due to the limited number of academic posts available, and because the US was heading into the Great Depression. Regardless of the importance of the position, it was one that he would not keep long (Kerns 2003).

¹⁷ Steward did not start or head the department, however.

After teaching the school year at Michigan, the following summer of 1929, Steward completed and defended his dissertation at Berkeley (Manners 1973). In doing so, he became the 6th PhD graduate of Berkeley's department of anthropology, and his position at Michigan was raised from lecturer to instructor (Kerns 2003: 112). The prospect of him leading the department at a major American institution looked good, but Steward missed Nyswander, and began trying to move back west earnestly. And, though he spent a second school year at Michigan, at the end of the term in 1930 he left rather precipitously to join Nyswander at the University of Utah. Nyswander was able to help secure him a job at Utah because of her own position at the university and because of her "very warm relationship" with the president, George Thomas (ibid. 116). When Kroeber was informed about the news, he warned Steward against giving up his job at Michigan; but Steward was convinced that his new-found position at the University of Utah would be a superior one, and Kroeber ultimately supported his move personally and professionally.

In June of 1930, on his trip to Utah from Michigan, Steward and Nyswander married. Steward took up his new life in Salt Lake City with vigour and began collecting archaeological materials; Steward's job at Utah was better than the one he left at Michigan. Not only was he closer to his wife, but he joined the faculty as an associate professor, and he also became Head of the Department of Anthropology, with himself as the only faculty member. By the age of 28, and after completing his dissertation only the summer previously, Steward attained his second position in a department of anthropology, and had very quickly achieved the rank of associate professor. Although his time in Utah would ultimately be brief, it is the time noted for his archaeological work

in the north and west of the state, an area that was virtually unknown to the archaeological community before his foray into the area (ibid. 116-8).¹⁸

While observing a Bear Dance ceremony at the Uinta Ute Reservation in the summer of 1931 Steward met and began dating a young woman, Jane Cannon (ibid.: 130).¹⁹ This new relationship caused Steward's forced-resignation from the University at the end of term in 1933; and once finalising his divorce to Nyswander, Steward and Jane Cannon were married in October of 1933. The couple, both unemployed, then relocated to Berkeley.

After spending the school-year taking odd jobs and facing great financial uncertainty in the San Francisco Bay area,²⁰ the Stewards reportedly traveled in the Great Basin for a little over a month in May and June of 1934. Through journal entries from both their diaries, Kerns records that Steward did no methodologically sound ethnographic fieldwork during this trip, preferring to visit with Parks people and archaeologists, visit with friends, and look for work (ibid: 156-60).

Back at Berkeley in June, Steward wrote and presented a paper at the American Academy for the Advancement of Science (AAAS) entitled, "Ecological Aspects of the Patrilocate." This paper would later be published in Kroeber's *festschrift*, with the new name, "The Economic and Social Basis of Primitive Bands" (Steward 1936). This paper

¹⁸ Archaeologists have now addressed and discredited Steward's conclusions of the area (Bettinger 1998; Thomas 1987).

¹⁹ The bear dance observations published as (1932) in *American Anthropologist*.

²⁰ These jobs included one temporary position at the National Park Service with fellow anthropologist, Paul Radin (Kerns 2003: 154).

is the framework that Steward developed for his theorisation of the concept of “band,” the framework from which he modified his ideas in numerous subsequent publications that he would link together from this article.²¹

In the fall of 1934 Steward replaced Kroeber at Berkeley for the term. This opportunity, coupled with other teaching jobs in the area gave Steward the security to plan out an extensive research project. It was then that he committed to a career in professional anthropology, developing what he acknowledged was a 20 year research plan (Kerns 2003). Manners, too, identifies that this was when Steward devoted his research and writing to “dominant processual regularities,” and where he confronted the Boasian idea that “no two cultures are alike” head-on, by creating scientifically discerned cross-cultural “typologies” of social evolution (Manners 1973: 889). His comparative method and theory is what he would come to call “cultural ecology” and “multilineal evolution” respectively.

During this term Steward also wrote his landmark theoretical piece in archaeology on settlements patterns (Kerns 2003: 169; Bettinger 1998) based on his “1934 field observations and conversations with archaeologists” called “Ecological Aspects of Southwestern Society” (Kerns 2003: 155). In the paper, Steward argues that social organisation of a level “higher” than bands, for instance a “clan,” must have developed from innate male dominance forming the “patrilineal band” through a form of economic-

²¹ Steward identified that this paper was the framework for this major ethnography, Basin-Plateau (Steward 1938) and then the reformulation of the concept of bands in *Theory of Culture Change*, specifically noted as condensed into chapter 6, “The Great Basin Shoshonean Indians” (Steward 1955; fn 1, see also chapters 7, 8, 9, 10)

techno-material determinism. This paper was first rejected for publication by the *American Anthropologist*, but was eventually published in the Austrian journal, *Anthropos* (Steward 1936b; Kerns 2003: 168-9).²²

In early 1935, not being able to secure permanent employment, Steward took the US civil services examinations for at least the second time; this time looking for employment as an anthropologist for the Bureau of Indian Affairs (BIA). While waiting for the results from his exams, Steward received a small Social Sciences Research Council (SSRC) grant to do fieldwork in the Great Basin, an amount that Kroeber matched so that Steward could do culture element surveys (ibid: 167). Steward would later and often lament over this work for Kroeber, work that he described in a letter to Marvin Harris late in his life as tedious and beneath him.²³

Possibilities for permanent employment improved in the spring of 1935, as Steward received an offer for a position as an archaeologist for the National Park Service. He declined this offer, confident to hold out until another opportunity availed itself that would permit him to undertake his major research project of taxonomies. He continued to write busily at this time, and prepared for a period of fieldwork in the Great Basin utilizing the SSRC grant and the money from Kroeber (Kerns 2003). Julian and Jane Steward embarked on this trip together, and their time in the Great Basin lasted from mid-April to August of 1935. According to Kerns' documentation, this fieldwork trip would provide the data for several important papers, and though by present standards it

²² It also appears in re-print as chapter 9 in *Theory of Culture Change* (Steward 1955).

²³ Steward to Marvin Harris, March 8, 1969.

was not methodologically sound or inter-culturally successful – often finding his informants “unwilling” to participate in his exercises or believing that they were “lacking knowledge” of their culture – it was Steward’s most successful fieldwork experience (ibid: 175-88).

As the Stewards’ trip through the Great Basin came to an end, Julian was unexpectedly offered a position as an assistant professor at the University of New Mexico. Once again, Steward declined the job offer, and rather, two days later, accepted a position from the Bureau of American Ethnology (BAE), as an associate anthropologist.²⁴ Professionally, Steward was happy to return to Washington and to be working with William Duncan Strong and Matthew Stirling, men whom he considered his colleagues because of their “scientific” approach. He was also happy to have a position with no teaching responsibilities, and enough freedom to be able to devote his time to his research project. Steward was in the official employ of the US Federal Government as of September 1935 – he was 33 years old (ibid: 196).

iv. Steward’s Theory 1929-35

Steward’s approach in his early published work is typified by three general characteristics. As stated above, in his first of 47 published contributions to *American Anthropologist* Steward began his life-long critique of Boasian anthropology by critiquing diffusionism as an explanation for the existence of similar culture elements in different locales. In the essay, “Diffusion and Independent Invention: A Critique of

²⁴ The BAE was funded by appropriations from Congress through the Department of the Interior.

Logic,” Steward (1929) offers a philosophical argument to promote his *prima facie* assumption that cultural forms exist in discreet lines of evolving and developing matter through processes of independent invention. He asserts that his approach offers a superior method for analysing the existence of identical cultural traits across great distances than does the method offered by the Boasian diffusionists (Steward 1929).

In the paper, originally given at the AAA meetings in 1928, Steward asserts that diffusionists are guilty of logic errors and are prone to be “confused.” Contrastingly, he contends that evolutionists have a defined method and are unbiased investigators who have overcome the failings inherent in the dominant Boasian approach. He argues for a conceptual necessity that understands cultural lines as bounded and distinct, because only this understanding could permit logical comparative analyses. To remedy the “illogical” programme of the diffusionists, he offers three “principles” for determining a method that would permit scientists to conclude that “when a culture element is found in two or more localities, the probability that independent invention has occurred is:” 1) directly proportionate to the difficulty of communicating between localities; 2) directly proportionate to the uniqueness of the element, what he calls the “quantitative criterion;” and 3) inversely proportionate to the probability of the trait being derived from a common ancestral culture (Steward 1929: 493).

This paper contains a statement of Steward’s early methodology, as he explains that “we are concerned here...with...defining the methodology used by unbiased investigators – if there be such – and stating its logical justification” (1929: 492). This essay demonstrates

an important characteristic of Steward's approach, because he acknowledges that he privileges the subjective judgment of the ethnographer to discern the distinctiveness of cultural traits – if they follow his method – and he acknowledges that he is creating these distinctions so as to rank and type them. In counter-distinction to the diffusionists, the method that he proposes for analysing cultural elements and culture change, he claims, is “objective” because it is based on “quantitative” components and is a function of time (Steward 1929: 493-4).

A second characteristic of his early published work is his unabashed androcentrism, typified by his complete misrecognition of the contribution of women and women's roles in the societies he was studying. What little fieldwork Steward did do focused exclusively on the role of men, and his utter devaluation of the role of women in his early work prejudiced his results. For example, in “Shoshoni Polyandry” (1936) he states that “social, economic, and legal institutions ordinarily give the male a monopoly of matrimonial, if not strictly sex, privileges in each family” (Steward 1936: 561); and in “Ecological Aspect of Southwestern Society,” he alleges that the simplest of social aggregates must be “patrilineal” because of ecological conditions and “innate male dominance” (1936: 333). This bias in his work has been very effectively demonstrated in Kerns (2003), and she offers an analysis of his male-centered approach as merely a reflection of his own social habits and only inferred inductively into his theoretical paradigm and never supported empirically by his own work. She notes:

For Steward, the sweeping generalizations about religion and about women conformed to and confirmed some of his own views; and although those views derived from his own social and emotional experiences, they took on the authority of science (Kerns 2003: 87).

Clearly, Steward's model ensconced a male-centred approach to the anthropological method, and was replicated and promoted in his theory and practice regardless of its empirical validity (*ibid.* 2003).

The final characteristic raised is his assumptions and representations of Indigenous peoples, including his statements about the social organisation and land-holding capabilities of Native Americans. Overwhelmingly, his representations are characterised by his belief that the cultures of the Indigenous Peoples of the Great Basin were failures due to culture contact and processes of assimilation which occurred easily and naturally, with the "low" Indigenous culture being affected by the "high" American culture.

A representative account of his description of Indigenous peoples is offered in his paper "A Uintah Ute Bear Dance" (Steward 1932). In this paper, Steward argues that the Bear Dance no longer retains aboriginal elements because its purpose and nature has been changed in the face of contact with Euro-Americans through the constant "deculturation" of the aboriginal Ute cultural form. He motivates this process of culture change through the Ute's desire to acquire western goods and the processes of diffusion. That is, the cultural change that he describes is as much about the Ute's desires for Western goods

and structures, and the “superior” material form of the Euro-Americans fills the gap and desire of the culturally impoverished Ute. This prompts him to conclude that the Bear Dance is no longer ceremonial but rather is “social” and his that it retains no functional importance for the people. He says,

The changes in the Indian’s attitudes toward the Bear Dance involve emphasis rather than innovations, for the ceremonial aspect is almost gone, whereas the social appeal is increasing. The most feared species, the grizzly has disappeared and the brown or black bear is virtually extinct, tribal religion is on the wane, and the medicine-man is losing prestige. Since the religious motivation is lacking and even the climax of the whole affair, the appearance or “coming out” of the bear on the last day of the dance is considered unessential, the dance is now considered an occasion for “good times,” relations between the sexes being of paramount importance (1932: 265).

Indicative of his fieldwork experiences, Steward was surprised by the *social* aspects of a community dance and stated that he did not need to witness the entire event to conclude that none of its aboriginal form remained. In fact, in his published account, he acknowledged that he left the dance “a little after 5 o’clock [because] I was forced to return to Salt Lake City, but things were then at their height” (*ibid*: 272). Socializing is hardly an unknown characteristic of dances, and his observations are certainly a demonstration of the continuation of social networks and cultural practices. In fact, he notes

Although the Indian now regards the Bear Dance as an opportunity for pleasurable social behavior rather than religious devotion, it nevertheless represents a set of behavior habits foreign to those of the white man's culture. For the relations between the sexes are precisely the reverse of those usually regarded by the Caucasian as proper, normal, and frequently even as inevitable.

These points well represent these three significant characteristics of Steward's work to this point in his career: Namely, that all cultures and societies are isolates, and exist on individual developmental trajectories that can be discerned through a scientific method; one that assumes that human social organisation is dictated by the norm of "inherent male dominance;" and finally, North American Indigenous Peoples are motivated by desire and greed to acquire Euro-American material culture to overcome their own culture of poverty.

The final point that I want to raise about Steward's published work up to this point in his career is his consideration of the land-holding capabilities of Native Americans. His statements during this time are atypical for him, in any sense; and, in fact, his comments in this regard are specific points that he retracts in his later work. Importantly, though he steadfastly concluded that Indians lived in communities that were contaminated and broken down by culture contact, he contends that the "Indian bands" of the Great Basin were "politically autonomous," "communally landowning," and all have rules for "land

inheritance,” and he concluded that all people live in at least this state of social organisation. Axiomatically he states,

All peoples in an area of low population density have some form of politically autonomous, landowning band, which is greater than the bilateral family. The size of the band and the extent of the territory it utilizes are determined by the number of persons who, due largely to ecological factors, habitually cooperate at least during part of the annual round of economic and social activity. Band unity is expressed in a consciousness of common interest and submission to some degree of central control during community enterprises, although such control may be lacking during parts of the year. The authority of the leader is consequently small and temporary and his position is seldom a fixed institution (1936: 343).

In this early paper, written directly after his fieldwork but before joining the federal government, Steward specifies that the Owens Valley Paiute, the Southern California Shoshone, and “other Paiute” are either composite or patrilineal bands and are therefore, *de facto*, politically autonomous, land owning, and are a recognisable group with a degree of central control and common interests within his own schema (*ibid.*: 338). Therein, not only did he say that all peoples live in an organised, rule based society, he notes that,

although the family is often the seasonal independent subsistence unit, additional social and economic factors require the unity and territorial

autonomy of an aggregate of several such families, that is, the band. The most important factors which produce the band are: (1) Among the apes and most other mammals, the "social" aggregate is usually greater than the biological family. Therefore, primates provide no reason to suppose that human beings ever were divided into family groups... (1936: 332).

And, in another article of the same year, "Ecological Aspects of Southwestern Society," Steward compares his conclusions of "primitive" social organisation to Radcliffe-Brown's, noting

Band or Horde, as used by RADCLIFFE-BROWN for comparable groups among the Australians has the merit of being noncommittal on these points, but because small, *territory-owning bands* may be of quite different composition, it is advantageous to use distinguishing terms for the sub-varieties (1936: 89, my emphasis).

He then goes on to list the "clan," "patrilineal band," "bilateral band," and matrilineal band as these sub-varieties (*ibid.*). That is, immediately after his fieldwork Steward concluded that the Indigenous peoples of the Great Basin, like all other peoples in the world, were socially organised, land-owning groups. How he changed his position and specifically countered these statements will be discussed in the following chapters.

The next chapter follows his move from theory to practice, to his first long-term career position at the Smithsonian Institution. It was in Washington that Steward began his 11-year stay at the BAE and embarked on several applied projects, including a short stint associated with the BIA.

Chapter Three. **Steward and the Bureau of American Ethnology: Representations of Indigenous peoples (1935-46)**

Steward's 11 year stay (1935-46) at the Bureau of American Ethnology (BAE) in Washington, DC was remarkably productive. This chapter explores Steward's work at the BAE starting with his first "applied" work, as a liaison to the Bureau of Indian Affairs (BIA), contextualising this work with respect to American Indian policy. Secondly, I discuss his representations of and theories about Indigenous peoples, including analyses of his fieldwork experiences during this time. Finally, I trace through the institutional structure that he established in the discipline – gaining such potency that he attempted to take over control of the national organisation without an academic post (Manners 1973; Trencher 2002; Kerns 2003).

i. The Bureau of Indian Affairs (BIA) and American Indian Policy

When Steward completed his graduate training in anthropology the most important federal policy regarding Indigenous peoples in the US, the Dawes General Allotment Act was coming to an end. The Dawes (Severalty) Act of 1887 had been designed to undermine community ownership of "tribal" property (Kelly 1983). It called for the active assimilation of Indigenous peoples into American society by fostering integration programmes and promoting citizenship rights. It also helped to facilitate Christian missionary activities to promote "health," "education," "religion," and "commerce" within Indian communities (Kelly 1983; Taylor 1984). For decades, the Dawes Act undermined Indigenous traditions, expropriated property and lands, and promoted goals of ahistorical liberal equality based on notions of social evolution (Rosenthal 1990).

Daily (2004) contextualises John Collier's appointment and work at the BIA in contradistinction to the goals of Allotment and the policies espoused by missionaries in the US. He documents that missionaries worked unabated under the protection of the Dawes Act until Collier's appointment in 1933. Until then, American Indians were forcibly converted to Christianity and assimilation into American society encouraged. Toward this goal, the Christian missions and the BIA worked together to "manage" Indians. Describing the relationship as a partnership, Daily explains that "the BIA helped missionaries gain easy access to the Indians by allowing them to operate religious education programs in federal boarding schools and by granting them tribal lands on which to build churches." And, in return for this assistance, "missionaries supported the BIA's expanding programs by promoting the doctrine of Indian wardship. According to this doctrine, Indians were wards of the state who were not yet capable of supporting themselves in a cutthroat capitalist economy" (Daily 2004: 4).

It was in opposition to this politic that Collier directed his energies in the years leading up to his surprising appointment as Commissioner of the BIA (Philp 1977; Dailey 2004); and the reforms that he proposed were an affront to the overt policies of assimilation dominant in American politics at that time (Dailey 2004).²⁵ In fact, Collier's reforms

²⁵ I am accepting the US Government's "Historical Chronology" of US Federal Indian Policy Periods. They list: 1) *The Early Period*, "When Europeans first sailed to America, the tribes were sovereign by nature. They conducted their own affairs and depended upon no other source of power to uphold their acts of government." 2) *Indian Removal (1816-46)*, "As the United States grew in size and power, additional land was required for settlement and development. The U.S. government began a policy of Indian removal, which was in effect from 1816 to 1846. Through treaties and coercion the government actively, and sometimes forcibly, removed Indigenous peoples to areas west of the Mississippi River." 3) *Reservation Period (1865-1890)*, "As the growing population demanded still more land, and since the available land base was shrinking, removal was no longer an option. As a result, Indigenous Americans were moved onto

were such a challenge to the *status quo* that, in one of his first policy decisions implemented after taking over the BIA, Daily describes his effect as “[r]eversing the pattern of the past several decades” (Daily 2003: 62).

The most important piece of legislation enacted while Collier was Commissioner was the Wheeler-Howard (Indian Reorganization) Act in 1934 (Philp 1977; Dailey 2004). The Act, best known as the “IRA” or the “New Deal,” was signed by Roosevelt on June 18, 1934, and

[a]lthough it bore little resemblance to Collier’s original proposal, the IRA established a turning point in Indian history by abandoning future land allotment. It extended the trust period in restricted land, allowed for the voluntary exchange of allotments to consolidate checkerboard reservations, continued existing practices of inheritance, and restored to tribal ownership remaining surplus lands created by the Dawes General Allotment Act (Philp 1977: 159).

Initially, Collier proposed four Titles for the Act. They were:

reservations.” 4) *Assimilation Period*, “At the end of the treaty-making period in 1871, the United States Congress began a policy aimed at narrowing tribal and individual Indigenous rights and encouraging Indigenous Americans to move from reservations. Assimilation, allotment, and U.S. citizenship for Indigenous Americans became official policy goals and continued until the late 1920’s.” 5) *Policy of Toleration*, “For a short period from about 1930 to 1943, the U.S. government adopted a more tolerant attitude towards Indigenous Nations. The Indian Reorganization Act encouraged economic development and a revival of Indigenous community life and culture. This benevolence was short lived, however, and was replaced with a policy of termination. 6) *Termination (1944-1958)*, “Termination was designed to produce rapid, forced assimilation. Under termination, the trust relationship between Indigenous Americans and the U.S. government would gradually decrease and eventually dissolve.”
<http://web.em.doe.gov/public/tribal/histchro.html>, visited, September 19, 2006.

- I) Indian home rule;
- II) expenditures for education required for self-government and the preservation of cultural traditions;
- III) restructured federal policy on Indian land tenure which “authorized appropriations for enlarging reservations and consolidating those that had been checkerboarded by sales to non-Indians;” and
- IV) a forum to address legal jurisdiction by proposing the creation of a Court of Indian Affairs with Indian judges to hear criminal, civil, and probate cases (Daily 84-6; Rosenthal 1990).

Through his proposal, Collier attempted to secure a policy that would ensure Indian self-government, the continuation of traditional societies, and the preservation of cultural and socio-political structures that differed from those of wider American society (Rusco 1999; Daily 2004; Wilkins and Lomawaima 2001; Sutton 1985).

In the end, the proposed Act was modified by Congress to such an extent that the opponents who railed against it considered the Act that passed a victory for them and a great loss for Collier (Dailey 2004: 88). The rewritten Act eliminated the Court for Indian Affairs and the goal of the preservation of cultural traditions (Title IV and II

respectively), and greatly reduced Title I, which removed the granting of municipal powers to nations. It did end allotment, however, and a sum of \$10 million was appropriated from the federal budget to stimulate tribal economies. Despite this setback to his social agenda, Collier's mark on American Indian policy is recognisable due to his long-term appointment as Commissioner which lasted another decade after the IRA passed (Dailey 2004; Philp 1977; Kelly 1983).

ii. Steward and the BIA

After being hired by the BAE in September, the Stewards took up permanent residence in Washington, D.C. in December of 1935. Steward's first job was to replace William Duncan Strong at the BAE, working temporarily as a liaison between it and the BIA (Kerns 2003). The dynamics of the BIA at this time led to one of the most volatile periods in the history of American Indian policy. Collier's programme fit well with the tenets of Boasian anthropology, and Steward openly opposed Collier's approach, believing that the initiative of home rule, for example, was a form of forced segregation that ran counter to the inevitable processes of assimilation (Steward 1969). Steward's approach differed so greatly from Collier's, Kerns notes that "[d]espite the 1934 [IRA] legislation, Steward regarded the assimilation of American Indians as inevitable and as the path to economic improvement" (Kerns 2003: 199). Thus, it was in the context of tremendous political competition to determine federal Indian policy that Steward joined the BAE and became formally associated with the BIA.²⁶

²⁶ Contra Manners (1973), Steward did not join the BIA at the request of Collier.

In the summer of 1936 Steward was charged with the task of preparing a report on the social organisation of the "Shoshone Tribes" for the BIA. This report was to assist in the implementation of Collier's plans "to provide reservation lands and federal recognition for the 'landless' Western Shoshone Indians of Nevada" (Blackhawk 199: 215), and to implement Collier's wider goals of the IRA. Philp's reports that

In order to implement his goal of restoring tribal life, Collier turned to anthropologists who replaced missionaries as the dominant influence on the Indian Bureau. At a meeting in Pittsburgh during December in 1934, the commissioner and members of his staff asked anthropologists how to utilize their discipline in organizing Indian self-government under the IRA, especially the tasks of establishing tribal constitutions. They suggested that the government take a consensus of their profession in order to find people acquainted with Indian needs (Philp 1977: 161).

One of the most important features of Steward's report for the BIA is that it contains his base description of the Indigenous people of the American Great Basin and his candid position for policy. Although the report does not contain detailed ethnographic descriptions, with the bulk of the discussion focussed on his recommendations, he does describe the "Shoshoni tribes," as "impoverished 'foot Indians'" and concludes "[that they] had no political organization beyond the union of families in small villages" (1936:

2).²⁷ This base description provides the rationale for his opposition to Collier's plans. In the report, he observes:

Unfortunately, it cannot be assumed that these Indians will be sufficiently solicitous of their constituents to act for the general welfare. As a matter of fact, they are usually so far removed from the native point of view and so bitterly opposed to the old generation that father is often set against son, and they are so strongly motivated by desire for personal gain that only the safeguards in the constitutions and strings held by the Federal government will protect their constituents. This may seem a strong statement. But it is important to insist that sincere devotion to the common good was completely foreign to Indian thinking, in which respect they do not differ greatly from white men. There are two reasons for this attitude in the Shoshoneans. First, their native culture made them even more individualistic than other Indians. As they possessed practically nothing which required cooperation in communal enterprises, going through the annual round of food seeking largely in family groups, their philosophy was every man for himself and family. Second, their association with white persons unfortunately has often made selfishness, even dishonesty a virtue (Steward 1936: 3-4).

²⁷ Defined by Steward, the Shoshoni Tribes include "Ute, Shoshoni, Northern Paiute, and Southern Paiute, in Western Colorado and Wyoming, Utah, southern Idaho, Nevada and eastern California and Oregon." (1936: 2)

In another example that illustrates his opinion of the Shoshone contained within the report, Steward equates the allocation of traditional lands for Shoshone people with segregation, and unnatural evolutionary forces. Stating his belief of the Shoshone's cultural poverty and culture loss, he says,

It is, therefore, futile to take measures to preserve anything native, for logic indicates that it cannot be done and observation shows that most traits have lost all functional importance. White patterns of thought and behavior are growing relentlessly and, though their progress is faltering and their acceptance fraught with grief, it is impossible to stem them. To attempt to do so with respect to social structure and basic values would be to attempt to crystalize [sic] what was natively vague and to bring into harmony with present conditions things that were lost precisely because they failed to harmonize. The non-reservation Shoshoni of Nevada seem to have made a much happier adjustment than most of their reservation kin. And I am prepared to state that the effect of segregation on a reservation would be to prolong individual maladjustment and augment the clash between conflicting types of persons.

Even if some native traits could be preserved, it is difficult to see what purpose they would serve. These people possessed virtually nothing which win them the admiration, sympathy or support of Whites. Anything distinctively Indian about them brands them as queer and "inferior" (rarely

as picturesque) and enhances difficulties flowing from race prejudice. I have known the White people of this region too long and heard too much of the contempt they hold for "The squalid Indian" to have any doubt of this. The common desire of the younger generation is, moreover, to become White as soon as possible. I do not, of course, advocate enforced assimilation; nature will take its course. I do insist that segregation on reservations, where it is not necessary, would be a great mistake (1936: 7).

In his account, he represents contact as occurring from high culture to low culture. He says the Shoshone adopt, through no agency or process, "culture" from Euro-Americans and become assimilated through what he calls the course of nature. He concludes that the Shoshone had been so tarnished from contact with Euro-Americans that there was little or nothing Aboriginal "left to protect" or recognise through federal policy. Given these assumptions of minimal sociocultural institutions in the first instance, processes of diffusion, and culture loss, he offers his opinion of the Shoshone and their ability to incorporate any of the aims of Reorganization. In his specific recommendation for the landless Shoshone receiving reservation lands via the IRA, Steward says that this policy would hamper the natural processes of acculturation and cause social unrest. He says:

Assimilation to white culture is proceeding rapidly, hastened by their contact with whites on ranches where they usually live and in schools. They are confronted with race prejudice, but it is tempered by a certain kindness

which follows from the fact that the Shoshoni have attached themselves to various ranches where they are not economic competitors with the Whites.

Now, to the extent that the Shoshoni are assembled in large, land-owning groups, they will become competitors with Whites. This does not argue that it would destroy them, but there is no question that it will entail some change in the attitude of the White people towards them, sharpening race conflicts as in the south where the negroes did not become the object of attack and suppression until after the Civil War. (One also thinks of the effect of race prejudice added to economic competition in certain modern fascist nations.) (Steward 1936: 11).

Throughout the report, Steward demonstrates his opposition to Collier's program, and urges for a continuation of policies similar to that of the Dawes Allotment Act. He recommends that the Shoshone have no special rights because they have no recognisable aboriginal political form or legal structure. Moreover, he believes, assimilation should be encouraged so as to assist in the natural development of the Indigenous Peoples into dominant American society. He bases his conclusion on the assumption that because the Indians were minimally evolved in the first instance of culture contact, aboriginal cultures were severely broken down or "extinct" by the 1930s (Murphy 1981: 183).²⁸

²⁸ In the "condensation" of his 1938 monograph on the Shoshone, he says "[a]fter the Indians were defeated, the division between peaceful and warring factions soon faded and the functions of war leaders were eliminated. Thenceforth, the principle need for leaders was to deal with the white men, especially with the officials of the United States government" (1955: 113).

Secondly, he argues that processes naturally destroyed native cultural forms and supplanted them with more advanced and legitimate ones. In this manner, he resorts to an explanation that he, himself, called “illogical” as an explanatory model in his first theory paper (Steward 1929).²⁹ In the report he concludes that Indigenous Peoples’ political rights are special, different, and unique and that supporting them would be akin to racism. He contends that acknowledging any Indian land rights in the Great Basin would exacerbate race tensions, establish a policy of segregation, and impede natural processes of evolution.³⁰

Finally, in a statement that foreshadows Canadian residential school policy, Steward advises that the education system must be addressed so as to counteract the traditions of the home as an impediment for assimilation. For, he says, the then-present system for the “education of the children, even by the most enlightened persons... cannot wholly serve to counteract the effects of the home” (1936c: 14). The inconsistency in this case, as in the Uintah Ute Bear Dance discussed above, is that if Shoshone culture was gone, why was there a continuation of traditions? If Shoshone or Aboriginal culture is “extinct” or even “severely broken down,” as he alleges, why do the traditions of the family home need to be counteracted specifically at school? Similar to the continuation of the Ute Bear Dance, in the face of empirical reality, Steward disregarded his own observational data to make his claims for policies.

²⁹ This would not be the last time that Steward appealed to the processes of diffusion to explain cultural forms that he could not account for in his evolutionary scheme. Soon, he would come apply this process to an explanation of the culture-history of the Carrier in explaining their relationship with the Tsimshian (E.g. 1960: 733).

³⁰ This argument has contemporary relevance (E.g. Kuper 2004; Silverstadj 2005; Asch and Kenrick 2005)

As mentioned in Chapter Two, Kerns (2003) and Rusco (1999) show that Steward did no or virtually no methodologically sound fieldwork for his report to Collier; and given his approach, it is not surprising that Collier rejected it, saying

I am tempted to excerpt other and lengthier dicta from Dr. Steward's report, but the one which I have quoted indicates most of the reason why the report does not prepossess me as social philosophy or as factual reporting.

In determining Indian Service policies, and in attempting to evaluate human beings and to chart the future of human spirits, there are needed some endowments of enthusiasm, confidence in the human nature one is dealing with, and social philosophy.

... the shedding of light upon our complicated Indian problem needs something more [than what Steward's report offers]. This is another case showing that achievements in a special science, anthropology or any other, provides no assurance of competency to deal with social problems (Collier (1936), cited in Rusco 1999: 106).

Notwithstanding these obvious shortcomings of the report, Steward's 1936 BIA-Shoshone report is important because it contains Steward's specific statements and recommendations for American Indian policy.

iii. Other BAE Accomplishments: *Basin-Plateau*, Fieldwork, and the *Handbook of South American Indians*

In addition to his report to the BIA, Steward's 11 years with the BAE is noted for three major accomplishments. First, this is when he published his ethnography on the Great Basin, *Basin-Plateau Aboriginal Sociopolitical Groups* (1938), based on what he says was six months in 1935 and four months in 1936 of fieldwork in the Great Basin (ibid. ix). He acknowledges that the monograph is an expansion of his 1936 essay, "The Economic and Social Basis of Primitive Bands." Notwithstanding that Steward's fieldwork experience for the ethnography was considerably less than he reported, *Basin-Plateau* is represented within the discipline with tremendous accolades. Harry Holbert Turney-High raves,

In this volume Dr Steward continues his study of the peoples of Uto-Aztecan speech in the Basin Plateau area, that vast and neglected portion of the North American continent. This is the most timely work, praiseworthy not only for the meticulous care which has obviously gone into it, significant not only for the informative character of its detail, but of great usefulness in its actual and potential value to social theory. Whatever one might think of topical societal treatises in anthropology, the great need of economics and sociology for *ethnologic fundamenta* in these necessitous times is such as to transcend the preferences of intellectual parochialism. I do not know whether or not this is the light in which the author views his work. It is in its value not only to anthropology but to other social sciences that I find myself save in calling this

book well-nigh monumental and recommending it without hesitation as a model for similar works which must and will follow.

He renounces the Marxian dialectic of economic determinism by name. In spite of this, one has the impression that at last the coldly realistic Economic Man of the Eighteenth Century has finally been found. And far be it from me to deny that the terms Smithian and Shoshonean are synonyms. Steward is best equipped to interpret his field data and while the way he has done it might not please everyone, his sincerity, sanity, and objectivity cannot be suspected (1940: 136-7).

Although not defined as such until later, it was in *Basin-Plateau* that Steward introduced the methodological framework that he would become best known for, cultural ecology. He defined the method by claiming that the most relevant characteristics of all cultures are those that are most closely linked with the physical environment, and the subsistence technologies used to exploit it. The relationship between environment and subsistence technology, Steward asserted, was to become the focus of cultural ecology, and the method used to rank societies on a social evolutionary scale.

In defining this method, Steward constructed an enduring image of the peoples of the Great Basin, describing the Shoshone as "gastric." That is, he claimed that they were driven entirely by the quest for food to stave off imminent starvation. Specifically, he said that "[s]tarvation was so common that all activities had to be organized toward the

food quest, which was carried on mostly by independent families" (1938: 46). Flowing from his description of the harsh environment, Steward offered statements on the cultural adaptations for exploiting it, determining that the Shoshone had no communal cooperation or interests, and no mechanisms to organise the individual members. Inconsistent, however, with his earlier work about the nature of their political organisation, Steward contends that because of the limited technological resources utilised by the indigenous peoples of the Great Basin they could not have any formal organisation beyond the nuclear family (ibid.: 247).

He explains that stemming from their low population density, their perceived hostile environment, and the perception that they possessed minimal technology; and then asserted that the Shoshoneans could not have any formal social organisation beyond the family because they were in constant pursuit of food. Summing up his analysis, and demonstrating the turn he makes in his theoretical project to this point in his career, he says,

The type of sociopolitical groups in the Basin-Plateau area was conditioned to a definable extent by human ecology. Rainfall, soils, topography, and climate determined the nature, quantity, and distribution of plant and animal species which were required for food. The hunting and gathering devices and transportational facilities known in the area allowed only a certain quantity of these to be procured and consequently limited the general population density. The subsistence habits required in each region largely determined the size,

nature, and permanency of population aggregates. These, in turn, predetermined many, though not all, features of social structures and political controls... Habitual cooperation of the same people and therefore the development of fixed if limited controls was impossible. Likewise, habitual and exclusive utilization of particular territories by certain groups could not develop (Steward 1938: 256-7).

After claiming that “all peoples” exist in social aggregates greater than the family in the “Economic and Social Basis of Primitive Bands” (1936a) he specifically addresses his previous statement, contending for the first time that then-contemporary Shoshone social organisation is non-Aboriginal (1938: 259), claiming that they existed aboriginally at what he would come to call the proto-social level of the “family” in the “levels of sociocultural integration” (1950, 1951, 1955). He made this claim despite his axiomatic statement in his formative papers on bands, his explicit recognition in his report to Collier, where he identified that the Shoshone “had no political organization beyond the *union of families in small villages*” (1936c: 2, emphasis mine), and the implicit recognition identified in the “Ecological Aspects of Southwestern Society” that the groups of the area are organised in patrilineal bands (1936b).

Steward specifically addresses this contradiction in his conclusions at the end of *Basin Plateau*, explaining that there are two types of bands. First, he lists groups that follow “unilineal” (matrilineal or patrilineal) descent and look a lot like “clans;” and secondly, groups with “composite” descent. He states unilineal bands are exogamous groups, and

occur “where ecology prevented group size from exceeding fifty to one hundred members and where emphasis upon hunting or other factors leading to male dominance made it patrilocal... [and the counterpart] matrilineal band... which probably occurred among some primitive horticulturists” (*ibid.* 259). On the other hand, composite bands are “not unilineal but consist of unrelated families and need not observe exogamy.” He goes on to say, that “this type occurs where the group is somewhat larger” and representative of the “Southern Bushmen, Andamanese, many Algonkians and Athabascans of Canada;” he then lists the Owens Valley Paiute, the Northern Shoshoni, and probably the Ute as composite bands too (*ibid.* 259-60). He distinguishes these groups from the “Western Shoshoni, probably the Southern Shoshoni, and perhaps some of the Northern Paiute,” because these latter groups were organised only at the family level and had no land ownership. He argues that this is because, in those areas, it was impossible to have aboriginal social organisation beyond the family because of technological limitations, and slots these groups into a “level” that is inconsistent with his earlier axiomatic statement about human social organisation, placing them below apes in his new taxonomy.

Given the new organisational level of the “family,” he then attempts to define the requirements for the emergence of the band, or the minimal unit of social organisation.

He says,

Other ecological factors permitted the growth of more complex sociopolitical forms in certain parts of the Basin-Plateau area. Villages amalgamated into bands when one of two conditions were present. First, they became bands

when a fertile environment permitted large and closely spaced villages, obviated the necessity of extensive travel, and allowed certain people habitually to exploit a given territory and associate together in communal activities... Second, bands formed when transportation was so improved that large groups could live together and either bring their foods to central point or travel as a body in search of them. Ecology thus permitted, if it did not cause, band development (Steward 1938: 257-8).

In the first instance, he argues that in harsh environments no social organisation could form. In the second instance, he alleges that the process for the emergence of the band, the minimal form of social organisation beyond the nuclear family in the Great Basin, was deemed a response to contact with Europeans. That is, for Steward, in the Great Basin social organisation at the band level is not an aboriginal form; aboriginal political organisation was only rooted in the biological family, particularly brothers, and contained no social organisation. He alleges that when social organisation emerged it was either in the form of the patrilineal or composite band which formed in *response to* the introduction of the horse and because of warfare with Euro-Americans.

Steward repeats this sentiment in another article, "Shoshoni Polyandry," where he writes about the social organisation of the Shoshone (Steward 1937). He alleges that the Shoshone were not organised beyond the individual family. He explains that this was because it was impossible to have socio-political organisation unless there was greater material technology and better transportation, though he offers no proof for this claim.

Here too, he assumes that aboriginally the communities were solitary, nuclear families, with no leaders and no overarching social connections. He alleges this is because organisation was impossible, due to the limited technology to communicate across the distances of the Great Basin. In fact, he concludes that the Indians were so fractured aboriginally that the “white man” brought solidarity in the form of the technological advance of the horse, and it was this technological advance that brought about group cohesion and ultimately changed the aboriginal form (Steward 1937). The process and implications of this claim is developed more fully in Steward’s subsequent work (E.g. Murphy and Steward 1956).³¹

Steward’s enquiry into the social organisation of the Shoshone for his ethnography went far beyond a simple cultural description. At the outset, he states explicitly that his project has a wider explanatory purpose – that being an analysis of the “determinants” that produce society (1938: 1). Of his rationale for studying the Shoshone, he says, “greatest success” towards this goal of discerning the determinants of society “should attend analyses of societies which evince a less complicated history, whose structure is simpler in content and form, and whose institutions were most extensively patterned by subsistence activities” (*ibid.*). He states that his analysis would be an impossible task to undertake on a “complex” society; therefore he assumes his conclusions and says that a

³¹ It is important to note the chronology of his argument about the emergence of bands. His introductory piece on the subject began with his essay “The Economic and Social Basis of Primitive Bands” (1936a) which he states grew into his ethnography, *Basin-Plateau* (1938). Following, the development of the argument, it culminates in *Theory of Culture Change*, which he identifies chapter 6, “The Great Basin Shoshone Indians: An Example of the Family Level of Integration” as the “condensation” of *Basin Plateau* (1955: 101, fn1). It is also developed, expanded and modified as chapters 7, 8 and 9 of the same text.

“simple” society offers a less complicated history and general rules as to the nature of human social organisation could be determined.

iv. Fieldwork

Also important for Steward’s career and development during his time at the BAE was his fieldwork experience. In 1938, he undertook a trip to South America, and, in doing so, joined his colleagues at the BAE in their new wartime focus on Latin and South America (Kerns 2003: 209-10).³² Steward planned an 18 month fieldwork trip, and anticipated visiting Ecuador, Peru, Chile, and Argentina starting in mid-June, in Ecuador. His goal was a planned a trip to Tierra del Fuego, where, in the difficult environmental conditions he hoped to find the existence of a patrilineal band, and he could then study a “functioning” primeval hunting group. Bringing his wife along as an interpreter the Stewards once again embarked on fieldwork together. However, this time they found the conditions much harder than their jaunts around the Great Basin, and Jane did not remain on the journey long, returning to the US within six weeks (*ibid*: 212). Steward remained on, but fearing malaria, he never made it to Tierra del Fuego, and traveled no further south than Peru, rarely leaving the company of American archaeologists. Kern’s effectively sums up Steward’s self-described “fiasco” of a trip, by saying “[t]he series of small catastrophes that had marred the trip, his persisting unease with an alien language and culture, his physical discomfort with the damp cold, and perhaps especially his wife’s departure had left him feeling adrift and alone” (*ibid*.: 215). In the end, Steward’s planned 18 month trip to South America failed, being reduced to “a few months of

³² Strong did work in Honduras and Stirling worked in Ecuador (Kerns 2003).

travel,” with his fieldwork consisting merely of visiting with American archaeologists, Hebert Spinden and Wendell Bennett. Riddled with anxiety and hypochondria, Steward, too, soon thereafter aborted his trip and returned to the US, and according to Kerns there is no evidence that he performed any fieldwork during his stay (*ibid.*: 212-16).

Secondly, in 1940 Steward undertook another fieldwork trip, this time to central British Columbia to study the Carrier. Still in search of an extant patrilineal hunting society, Steward thought the Carrier were a good research subject because they had remained relatively untouched by civilization (*ibid.*: 218-9). In his Carrier work, Steward reports that “[t]he anthropologist studies these modern primitives to gain insight into social change and to test social theory” (1941: 83). He states explicitly that the questions of social theory that motivated his research project were: “What relation had economic pursuits to social organization?” and to distinguish “[i]n the present Carrier culture, what is Indian, what is white?” (*ibid.*: 83). For six weeks Steward stayed in a hotel at the south end of Stuart Lake (1941: 86), and from one informant he claimed that he determined “hunting territories, genealogies, traditional subsistence patterns and major social and economic changes that had taken place” in the area since contact.

v. The Handbook of South American Indians

Partly because of others perceived that he had first-hand knowledge of South America, it was also during his tenure at the BAE that Steward compiled and edited the six volume collection, *The Handbook of South American Indians* (1946a, 1946b, 1948a, 1948b, 1949, 1950). These books greatly extended his ethnographic expertise, with Manner’s

claiming that they are “certainly one of the most important contributions to the ethnographic literature of the past thirty years... which Steward conceived, organized, edited and contributed to before leaving the agency in 1946” (1973: 892). Although Manners was incorrect in stating that Steward “conceived” of the *Handbook* (Notes and News *American Anthropologist* 1934 pp. 317-319), he was correct, however, in recognising the relative importance of the collection to the discipline. The *Handbook* has been criticized for repeated errors of under-reporting population densities, for example (Miller 1978: 940); however it has retained a foundational place in English speaking anthropology as a representative ethnographic and theoretical frame of South America Indigenous cultures. The book contains over 80 entries from scholars around the world, and according to Rouse (1953), a “significant by-product of the publication of the *Handbook of South American Indians* has been the development of a new theory of South American culture history.” Although the collection did not undo culture history studies, it is the beginning of Steward’s sustained cataloguing of cultures in evolutionary typologies, and extended his ethnographic expertise from North to South America

The attempt to Reorganise the AAA and the effect on the discipline

Manners (1973) lists two other important features of Steward’s time at the BAE. First, he notes,

During his last years at the Bureau of American Ethnology Steward served as chairman of a committee to plan the reorganization of the [American Anthropological] Association and to rewrite its constitution so as to make it

more responsive to changes that accompanied the rapid expansion of the profession (893).

Trencher's (2002: 450-2) analysis of the history of the discipline at this time explains that:

In 1940, there had been a move primarily by second-generation anthropologists to the AAA to what they termed a professional as well as a scientific association. Led by Julian Steward, they sought the creation of a section for applied anthropology in the AAA. But the association, primarily run by Boasians working in academia rejected the new section, which led to the creation of a separate Society for Applied Anthropology in 1941, with Steward at the helm (Trencher 2002: 450-1).

Explaining the effect on the discipline even further, she reports

At the Annual meeting in 1945, the AAA membership voted to have the incoming president Ralph Linton appoint a Committee on Reorganization to 'ascertain the views of the professional membership of the AAA, of allied societies, and of local groups, concerning the proposals for the reorganization of the AAA.' The committee sought 'to devise an organization that can act for the entire profession and the same time counteract the separatist trends in anthropology as a science.' The phrase

‘separatist trends’ referred particularly (although not exclusively) to a move by a group of younger anthropologists (referred to here as the ‘second generation’), many of them trained by ‘Boasians,’ who had an ‘outlook... very different from that of their seniors.’ Trained closer to World War II than World War I, these anthropologists (Julian Steward, Ralph Linton, Homer Barnett, George Peter Murdock, and Alexander Spoehr, among others) often had their professional start working for the government in New Deal programs in the 1930s and thus had their early work experience in applied rather than academic settings, where opportunities had grown scarce. They sought, as Boas had nearly half a century earlier, to form a scientific and professional organization. But while the vocabulary was the same, the definitions had changed. Many members of the second-generation cohort had an epistemologically more rigid (positivistic) view of science and experientially different view of what constitutes professional anthropology...

Manners links and follows this movement within the discipline to Steward’s (1947) “Comments on the Statement on Human Rights,” where the struggle to define the purpose of scientific anthropological research first appeared in his writing. Manners says the comments “drew a line between the scientist as scientist and the scientist as citizen” (1973: 891). Acknowledging that “Steward, again representing the argument for a more positivistic scientific practice, claimed that the AAA statement was inappropriate: In the absence of objective scientific evidence that human rights exist, ‘as a scientific

organization, [and claimed that] the Association has no business dealing with the rights of man” (Trencher, quoting Steward, 2002: 453).

Secondly, after joining the powerful Anthropology Society of Washington (ASW), Steward started the Institute for Social Anthropology, a State Department project “designed in part to help develop methods for dealing with problems of culture change and modernization.” (Manners 1973; Kerns 2003) Manners continues explaining “Its primary purpose, however, was to send anthropologists to teach in Colombia, Mexico, Peru, and Brazil and to carry on fieldwork, particularly on subjects of such practical interest to the countries as economics, patterns of land use and land tenure, etc.” (Manners 1973: 893).

Though Steward was still to advance three tremendous contributions to anthropological theory – multilineal evolution, cultural ecology, and the levels of sociocultural integration – his turn had been made and the foundation for his entire theoretical position was well established. His applied work put him at odds with the policies of Collier and American Indian policy, just as he opposed the method of the Boasians in his earlier training. In discussing the next phase of Steward’s life, I focus on his work from 1946 to 1952 – his short-lived professorship at Columbia University. This next phase begins with an analysis of his teaching and training of students; and, secondly, explores the specific articulation of his theoretical position, method and his stated relevance for anthropological research, and social activism.

Chapter Four. **Steward and Columbia University: anti or pre-Boasian, 1946-52.**

Steward's career up to the time that he joined Columbia University was undeniably impressive, and though he was still to make several notable contributions to the discipline, the foundation for his theoretical position had been established by time he left the BAE in 1946. Steward's appointment at Columbia began his short-lived, but extremely influential stint in the classroom training graduate students; as well, this was when he wrote several of his most important theoretical pieces (Murphy 1977; Kerns 2003; Clemmer and Myers 1999).

Steward arrived at Columbia in the midst of the department's ideological "re-focussing" following the death of Boas, and coinciding with the influx of many new students attending university at the end of WW II under the GI Bill (Silverman 2005). These two factors permitted his six year tenure at Columbia to help shape the orientation of the department and ultimately the method of the discipline in North America. This claim is evidenced in his institutional, intellectual-genealogical and theoretical dominance in the discipline, in what is today recognised as his theory and method as *the* science of the discipline (Moore 1997).

Also while at Columbia Steward started working with the US government in 1949, acting as an expert witness and strategist for the Department of Justice in cases before the Indian Claims Commission (ICC). Thus, after compiling ethnographic data on the American Great Basin at the start of his career and then the Indigenous peoples of the entire Western hemisphere while at the BAE, Steward started his teaching career when he

joined the faculty at Columbia. It was there that he began to frame his work in theoretical terms, to train students in the method of cultural ecology, and to measure the efficacy of anthropological research in terms of its utility and application for the US government (Steward 1950: xii, 2, 155).

This chapter theorises Steward with respect to his time at Columbia University and Boasian cultural relativism. It contrasts the debate within the discipline over the United Nations' *Draft Declaration of the Rights of Man*, documenting Steward's criticisms of the AAA's Statement on Human Rights (AAA 1947). It introduces four of his most influential articles that were written during this time, showing their sympathy to Spencerian evolutionism, and undermines the claim that Steward's work is "neo"-evolutionary (Sponsell 2006; Bohannon and Glazer; Trigger 1998).

i. Columbia University

Unmistakably, for the four decades before Steward joined the department of anthropology at Columbia it was personified by one man, Franz Boas. Institutionally speaking, Boas helped professionalize the discipline in the US, through his work at the Museum of Natural History, and then at Columbia University where he trained the first generation of American cultural anthropologists.³³ His first students included Alfred Kroeber, Robert Lowie, Paul Radin, Frank Speck, Alexander Goldenweiser, Edward Sapir, and Clark Wissler. He then trained a second generation of prominent students after

³³ At this time Harvard granted more PhDs in anthropology than Columbia, but the majority of these were in archaeology (Silverman 2005).

WW I, including Margaret Mead, Ruth Benedict, Alexander Lesser, Ruth Bunzel and Gene Weltfish that ran parallel to the other cohorts trained in the American PhD granting universities, Harvard and Berkeley. Thus, Boas' prodigious training of students helped to secure what some would call a "Boasian-approach" in American anthropology, even if many of the so-labelled claimed there was no Boasian-school *per se*, only that they shared an insistence on scientific empirical research (Mead 1959b; Harris 1968: 252; C.f. White 1963b, Peace 2004: 100-2).

As Stocking (1960) lays out in detail, Boas led the department at Columbia while working to change the practice of anthropology in the US. He sought this change by attempting to form a national organisation of professionally trained anthropologists. Stocking shows that Boas' attempt to establish this organisation, whose members were professional anthropologists as opposed to hobbyists, was met with resistance by the Acting Chief of the Bureau of American Ethnology, W J McGee. McGee was a self-trained surveyor from Iowa who had strong connections to the Anthropology Society of Washington (ASW). He believed that membership to the national organisation, just formed as the American Anthropological Association (AAA), should be open to any member of the public, and that no professional requirements should be needed for membership (Stocking 1960: 8).

Following Stocking's history, Harris (1968) demonstrates that McGee's theoretical orientation made logical errors such as conflating language, culture and race in his social evolutionary pronouncements. In fact, Harris says

McGee was an inexhaustible mine of every error of substance and theory that it was possible to commit on the basis of the most vulgar prejudices masquerading as scientific expertise. In seeking to understand Boas' renaissance-like involvement with all four fields of anthropology, we must bear in mind that people of McGee's ilk had opinions about everything, most of them so blatantly wrong as to create a virtual vacuum once they were challenged and assigned to their proper oblivion... McGee's confusion of race, language, and culture was representative of learned opinion both within anthropology and in Western society in general. The debasement of contemporary primitive peoples to the level of Anthropeidea was, as we have seen, an important expression of Euro-American imperialism. McGee's version is especially obnoxious: 'The savage stands strikingly close to the sub-human species in every aspect of mentality as well as in bodily habits and bodily structure' (Harris 1968: 255, quoting McGee 1901: 13).

Recognising that McGee's approach to anthropology was empirically inaccurate and theoretically dubious, Boas sought to counter his amateur approach with professional standards of method and practice. Under the aegis of the then-mostly defunct American Ethnological Society (AES), Boas opposed McGee's haphazard methods as baseless and unscientific, and pushed for a restricted membership for the national organisation

(Stocking 1960). Darnell (2000) identifies the dynamics surrounding the debates to organise the discipline, noting that

Boas's reaction against [McGee's] paradigm took the historical development assumed by evolutionary anthropology as problematic, an empirical question in particular cases. He insisted that race, language and culture were analytically independent, structured his ethnographic descriptions around the history of limited geographical areas and the context of elements within particular cultures (Darnell 2000: 274).

Harris locates the non-professionals' opposition to Boas and in particular the specific opposition advanced by McGee, as influenced by the social philosophy of Herbert Spencer (*ibid*: 254). Harris makes this connection because of McGee's adherence to a biological-evolutionary model and his promotion of a self-interested liberal individual. To highlight McGee's connection to Spencerian sociology, Harris quotes his 1894 *American Anthropologist* article, "The Citizen," where McGee states

Just as patriarchy gives way to hierarchy, and hierarchy to absolute monarchy, so limited monarchy is giving way to democracy or republicanism; already the foremost nation of the earth is a republic, and all other civilized nations are either republican or undergoing changes to in the direction of republicanism. So according to the experience of the ages, the

best nation is a republic one, and the best citizen is the individual adapted to life under republican conditions (McGee 1894, quoted in Harris 1968: 254).

At the time of the founding of the AAA, the opposition to Boas was framed in terms of Spencerian evolution. This approach conflated all cultural aspects into an evolutionary teleology that equated cultural forms with biological ones, and promoted self-interested liberalism as the natural progressive path for human and social development.

Although Boas lost his initial battle to control the make-up of the AAA to McGee, he was able to leave an indelible mark on the discipline through his ethnological contributions, academic genealogical tradition, political engagement, and contributions to anthropological theory (Stocking 1968a, 1968b; Silverman 1981; Darnell 2000). There is little doubt that the first four decades of anthropology at Columbia, and resultantly American anthropology in general, Boas was the leading intellectual and political force. However, notwithstanding his obvious contributions to American anthropology, the belief that Boas contributed *anything* to anthropological theory is contested in some accounts of him (White 1963, 1968; McGee and Warms 1996; Carneiro 2003). In contrast to these assertions, Lesser suggests that Boas contributed “two great theories” to anthropology, stressing that Boas’ work created conceptual

distinctions among race, language, and culture; his transformation of the culture concept from its prior use as a synonym for ‘civilization’; and his insistence upon understanding cultures in their historical contexts, apart

from the observer's ethnocentric standards of evaluation, became premises of modern anthropology (Lesser 1977: 3).

Similarly, Stocking concludes his historical account of Boas' reaction to 19th century evolutionism, saying that

Boas did not... offer a definition of anthropological 'culture.' But what he did do was to create an important portion of the context in which the word acquired its characteristic anthropological meaning. He was a leader of a cultural revolution that, by changing the relation of 'culture' to man's evolutionary development, to the burden of tradition, and to the processes of human reason, transformed the notion into a tool quite different from what it had been before. In the process he helped to transform both anthropology and the anthropologist's world (Stocking 1968: 233).

And, Harris (1968) echoes Stocking's sentiment, recognising Boas as the founder of the scientific method in American anthropology. Rather backhandedly but nonetheless, Harris notes "[g]iven the condition of anthropology at the end of the nineteenth century, the one great reformatory movement that was necessary (but not sufficient) to the further progress of the sciences of culture was precisely that which Boas initiated" (1968: 261). Thus, Boas positioned himself against the evolutionism of McGee, and offered a method based on what has been called "a radically different understanding of the epistemology of

fieldwork” (Bunzl 2004: 437) that adhered to a “[r]elativism, in the sense of the withholding of judgment by any external or a priori standard” (Stocking 1968: 230).

Upon his retirement from Columbia, Boas was replaced by Ralph Linton, a Harvard trained PhD and member of the Anthropology Society of Washington; and shortly after his death in 1942, the Boasian relativism followed by his students at Columbia came quickly under attack (Kerns 2003: 235-6). Reports are unanimous that Linton and Benedict were at odds with one another, and that the Boasian hold on the department quickly began to wane with Boas’ death and the dominant focus of the department under attack (Sidky 2004). Combined with Benedict’s untimely death soon thereafter in 1948, the additions of both Strong and Steward from the BAE, the change in faculty marked the absolute shift from the Boasian tradition at Columbia to a more materialist/evolutionary approach (Silverman 2005). Moreover, the removal of women faculty members Gene Weltfish and Marian Smith, and the influx of many male graduate students due to the GI Bill, Boasian relativism quickly dissipated to the periphery of New York colleges and evolutionary anthropology reared its head at Columbia in the middle of the 20th century (Kerns 2003).

ii. STEWARD AND COLUMBIA

Steward was hired at Columbia University by William Duncan Strong, his fellow Berkeley chum and former BAE colleague. Strong, as chair of the department of anthropology at Columbia, was charged with hiring a replacement for Ralph Linton, who was leaving to head the department of anthropology at Yale (Silverman 2005). Privately

Strong and Linton recruited Steward to help confront the psychological focus of the department, primarily promoted by Ruth Benedict. Strong and Steward shared a materialist approach to understanding culture change with focus on ecology, and they both worked to submerge the Boasian relativism continued by Benedict, Margaret Mead, Gene Weltfish, and Ruth Bunzel (Kerns 2003: 239-41; Murphy 1977: 10).

Thus, when Steward joined the department of anthropology at Columbia, he once again joined an institution that was undergoing tremendous reorganisation due to political and ideological conflicts. However, this time, Murphy recounts, Steward's materialist focus jived very well with the men returning home from war-time duty and who had "learned justice at the barrel of a gun" (Murphy 1981: 177). With his new method, which was soon-to-be called "cultural ecology," Steward supervised numerous soon-to-be prominent scholars' PhD dissertations, sat on their graduate committees, and greatly influenced their careers. Murphy (1977) reports that Steward supervised the completion of 35 doctoral dissertations in his six years at Columbia, and that he sat on many dozen more dissertation committees. As a whole, these students were very politically active, and some of the men formed an exclusive anthropology reading group called the "Mundial Upheaval Group" (Wolf, quoted in Friedman 1987: 109).³⁴

In counter-distinction to his students' political activism, Steward championed his own work as objective and apolitical. Kerns quotes him, saying explicitly, that "[m]y teaching

³⁴ It is primarily because of these students that Steward also carries with him the mythologised image of bringing Marxist analyses to American anthropology amidst the era of McCarthyism. It is more accurate, however, to align Steward's work with the social Darwinism of Herbert Spencer, and rather debate the relevance of the Marxist moniker for Wolf, Diamond, Harris, and Leacock.

is entirely non-political in every sense” (2003: 247), and thus promoted his own work as neutral and objective. Manners reports that Stanley Diamond, another of Steward’s prominent students, hammered his professor on this point recognising that “although Steward’s very decision for neutrality... is itself a decisive act that could have significant social consequences, our discipline offers neither proof nor assurances that one of these strategies is either more benign, more fecund, or more ‘scientific’ than the other” (Diamond, cited in Manners 1973: 891). This difference with his students’ belief in open political activism was also manifest in his relationship with Eric Wolf, and is evidenced in an exchange in the journal *Science*, where Steward replied to Wolf’s review and critique of his penultimate book, *Contemporary Change in Traditional Societies* (Steward 1967). In the original review Wolf critiqued Steward precisely on this point, and Steward responded to the review saying,

The moral responsibility of scientists for social change and its attendant ills has been increasingly debated in *Science* and elsewhere since the bomb was dropped on Hiroshima. I submit, however, that the issue has been improperly phrased and an unnecessary dilemma thus created... Today, scientific research is a basic culture value, and we obviously cannot hold any particular scientists responsible for its effects. Science must above all remain free (1968: 147).

As evidenced by these debates later in their careers Steward and his students would come to disagree about the method of a politically active anthropology; however, in the late 1940s they all worked together on the Puerto Rico Project (Steward 1956), one of the

most famous research projects ever undertaken in American anthropology (Lauria 1990; Duncan 1978; Silverman 2005). Research for the study was done by Manners, Mintz, Padilla, Scheele and Wolf, and who Manners recalls fondly as “their Puerto Rican assistants” (Manners 1973: 893). Although Steward left his students to fend for themselves in their fieldwork, and many complaints were levied against him for his lack of instruction, he designed the research program of the Puerto Rico Project to fit his larger theoretical project of eliciting evolutionary types (Kerns 2003: 252). The aim was to offer an “historically grounded institutional analysis of the culture and subcultures of Puerto Rican society in their insular setting and in the overall context of the island’s political, economic, and social ties to the United States” (Manners 1973: 893). Steward himself heralded it as an example of anthropological research measured in terms of utility to the government in the exertion of sovereignty (Steward 1950: 155) and scientific research in terms of its utility for the government in times of war (*ibid.*: xii).

Foreshadowing the future work of Wolf, Mintz, Manners and Roseberry, Steward says the Puerto Rico Project was conceived explicitly to analyse “culture change in a complex and contemporary society” (Steward 1956); and it was there “that Steward’s theory of cultural ecology [was] set out and given its most extensive field research application: that technological adaptations to given ecological settings provided an analytical framework to explain human society and culture” (Duncan 1978: 4). For Steward, in evolutionary terms, Puerto Rico was considered to be the next step on the scale of human development, and a far cry from the most basic level of humanity that he and his students imagined in the Great Basin.

iii. THEORY

It was during his time at Columbia that Steward began to publish his strongest theoretical pieces to date. Possibly developed because of chiding from Boasian Clyde Kluckhohn,³⁵ it was at Columbia that Steward began articulating his theoretical position explicitly (Kerns 2003). In their introduction to the recent collection, *Julian Steward and the Great Basin: The Making of an Anthropologist*, Clemmer and Myers (1999: xiii-iv) identify

³⁵ I believe that we can understand the advent of Steward's explicit theoretical position at this time with respect to the AAA Statement on the UN draft Declaration on Human Rights and that Kluckhohn's chiding may have helped to isolate who his opponents were. The Statement was written by Boasian trained anthropologist, Melville Herskovitz, and submitted on behalf of the AAA Executive to the Commission, asking "How can the proposed Declaration be applicable to all human beings, and not be a statement of rights conceived only in terms of the values prevalent in the countries of Western Europe and America?" (AAA 1947: 539).

Claiming authority from recent anthropological conclusions drawn from data compiled throughout the world, Herskovitz asserts that "[a]ll peoples" resolve "the problem of subsistence, of social living, of political regulation of group life, of reaching accord with the Universe and satisfying his aesthetic drives..." (ibid: 540). From this axiom, he then condemns the draft Declaration's focus on Western values of privileging the individual and dismissing culture in its formulation, stating that the "individual can develop only in terms of the culture of his society" (540).

Clearly advancing a cultural relativist position, Herskovitz offers three propositions to the Commission, noting that they must square the Universal Declaration with existing academic knowledge; he writes: 1) The individual realizes his personality through his culture, hence respect for individual differences entails a respect for cultural differences; 2) Respect for differences between cultures is validated by the scientific fact that no technique of qualitatively evaluating cultures has been discovered; 3) Standards and values are relative to the culture from which they derive so that any attempt to formulate postulates that grow out of the beliefs or moral codes of one culture must to that extent detract from the applicability of any Declaration of Human Rights to mankind as a whole (ibid.: 541-2).

Steward reacted to the AAA's Statement to the UN Commission on Human Rights, countering it on two fronts. First, he asserts that "[t]o be universally valid ... the Statement must apply equally to the cultural values which underlie the internal policies and motivate the foreign affairs of the civilized nations" (1947: 351). Citing the case of the "brutal treatment of Jews in Hitler Germany," he notes that "the Statement is a value judgement any way it is taken. [And] if it does not advocate tolerance for *all* cultural values, no matter how repugnant some of them may be to us as individuals, then it must imply disapproval of some cultural values, though it also says we have no scientific basis for making any value judgements" (351-2).

Secondly, he questions the role of anthropologists in this matter, stating that "we have gotten out of our scientific role" and thus have entered into the dangerous realm of taking a "political stand" or making "value judgements." He concludes that as "a scientific organization, the Association has no business dealing with the rights of man;" and that "we shall serve science better, and I daresay we shall eventually serve humanity better, if we stick to our purpose" (352).

four published works from this time-period that demonstrate Steward's burgeoning contribution to anthropological theory and form the basis for his theoretical work. They list: "Cultural Causality and Law," published in *American Anthropologist* (1949); "Evolution and Process," published in Kroeber's edited tome, *Anthropology Today* (1953);³⁶ "The Levels of Sociocultural Integration," published in the *Southwest Journal of Anthropology* (1951); and finally the monograph, *Area Research: Theory and Practice* (1950). I will discuss these in turn.

A. Steward's "Cultural Causality and Law" has somewhat cautiously been called "perhaps his single most influential article" (Murphy 1981: 195), and "possibly his most theoretically complete piece" (Kerns 2003: 253). In it, he traces what he calls the causes of the development of the state in several civilizations in human history (Murphy 1977: 29). Murphy acknowledges that Steward's argument in "Cultural Causality and Law" is the same technical argument as his theorisation of the Shoshone, and identifies that in both accounts that it is the technological factors that dictate the level of human social evolution (1973: 195); and hypothesises that it is the control of irrigation and the subsequent social stratification that gives rise to the power structure of the state in each case in the history of human social evolution.

Likewise, Kerns identifies that Steward's

³⁶ In the revamped edition of Kroeber's *Anthropology Today*, reconfigured by Sol Tax (1962) Steward's article has been excised entirely.

central argument was that agriculture based on irrigation had developed in the six areas, which were arid or semiarid; and that efforts to control labor for the purpose of building and maintaining irrigation systems and other public works, and efforts to control the distribution of water, had led to increasing stratification, political centralization, and the formation of the state. Steward implicitly drew on what came to known as Wittfogel's hydraulic theory, but he cited Wittfogel... only as a source of empirical data... (254).

In the paper, Steward takes a unique approach for him by referring to and citing other scholars' work. He begins the article with a retrospective statement about the discipline, contending that evolutionary pronouncements had given way to historically particularist ones in the beginning of the century. He concludes, though, that "[i]n spite of a half century of scepticism concerning the possibility of forming cultural regularities, the conviction is widely held that the discovery of cultural laws is [now] an ultimate goal of anthropology" (*ibid.* 2).³⁷ He offers, here and elsewhere (e.g. 1955), that his theory is *sui generis*, and that his is truly a unique conceptualisation of human social change (evolution). First distinguishing himself from 19th century evolutionists by simple assertion, he labels his contemporaries as following out-dated traditions: He states that Leslie White and V. Gordon Childe are holdovers from Lewis Henry Morgan; A.R.

³⁷ For this assertion, he finds support, he says, from the work of White, Lesser, and Lowie, and notes that his own personal attempt to formulate regularities is with respect to the patrilineal band. It is important to remember that though he had "no doubt that many such laws can even now be postulated, [and the present need is] to establish a genuine interest in the scientific objective and a clear conceptualization of what is meant by regularities" (Steward 1949: 2), in his fieldwork, much to his consternation, he was never able to locate the patrilineal band (Kerns 2003).

Radcliffe-Brown and Robert Redfield follow the functionalism of Emile Durkheim; and Malinowski follows an unnamed “scientific theory of culture”(1949: 1).

Articulating what he understands to be the goals of then-current anthropological theory should be he lists three criteria for formulating cultural regularities, or laws. These criteria outline much of his methodological approach for his subsequent work. First, he states, “*There must be a typology of cultures, patterns, and institutions*” (*ibid.*: 3, his emphasis). Explaining this first criterion with nothing more than a simple tautology, he posits that because anthropological terminology demonstrates hundreds of types of culture elements the elements must exist with regularity. He offers no support for this axiom other than his own belief in its necessity.

Secondly, he states, “*Causal interrelationship of types must be established in sequential or synchronic terms, or both.*” Advocating for a “functional” interpretation for constructing historical phenomena, he says, “[i]nsights into causes are deeper when the interrelationships of historical phenomena are analyzed functionally” (*ibid.* 3, his emphasis). Citing Childe and Wittfogel for exemplary work on the socio-economic structure of early civilizations, he compares their approach with Redfield, Radcliffe-Brown, and Malinowski saying that the synchronic functional approaches of the latter should only be utilised when historical data is unavailable.

Third, and certainly most importantly, he states: “*The formulation of the independent recurrence of synchronic and/or sequential interrelationships of cultural phenomena is a*

scientific statement of cause and effect, regularities, or laws.” Taking aim at the Boasians, Steward says that the particularist approach cannot identify independent cause and effect relationships and must fall back on the supposition of single origin hypothesis, and therefore processes of diffusion (*ibid.*: 3), a point that he believed he had already refuted (Steward 1929). Once again, he claims that the use of diffusion to explain cultural phenomena is illogical, and lacks a rigorous explanation of culture change.

As an explicit alternative to the Boasian approach, he suggests that “regularities can be found only by looking for them, and they will be valid only if a rigorous methodology underlies the framing of hypotheses” (*ibid.* 5). Explaining, he says,

The present statement of scientific purpose and methodology rests on a conception of culture that needs clarification. *If the more important institutions of culture can be isolated from their unique setting so as to be typed, classified, and related to recurring antecedents or functional correlates, it follows from this that it is possible to consider the institutions in question as the basic or constant ones, whereas the features that lend uniqueness are the secondary or variable ones* (1949: 6, emphasis his).

His method for interpreting or distinguishing core features from peripheral ones is an extremely powerful tool in anthropology and archaeology, and has come to be known as

the concept of culture core. This notion is reified in Canadian law as the test to discern Aboriginal rights through the integral to a distinctive culture test.³⁸

B. Clemmer and Myers also identify the article, "Evolution and Process," as one of Steward's most significant theoretical contributions written while he was at Columbia. The argument is crucial for Steward's emerging theoretical project, as it is in this essay that he defines one of the most important concepts in anthropological theory, "multilinear evolution," helping to define the discipline's answer to 19th century unilinear evolutionism.

In this general piece, Steward wades into the contemporary anthropological debates by presenting a polemic, reacting to his contemporaries in the Boasian tradition of American anthropology, and challenging the dominant trend for its lack of grand, comparative theory. Specifically, he attacks Kroeber, as well as the evolutionary approach of White, setting his project up, once again, as unique.³⁹ This essay reintroduces social evolutionism to mainstream anthropological theory, and promotes a schematic representation of the levels of human groups. It re-establishes the 19th century evolutionary paradigm, but this time distinguishes the levels through a claimed "objective" methodology for evaluating differences between cultural forms. He also

³⁸ As discussed below, this is the rationale of the distinctive culture test in *R v. van der Peet* [1996].

³⁹ At the outset of "Evolution and Process," Steward attacks Leslie White's evolutionism. In contemporary readings of the history of anthropology, often White and Steward are lumped together as "neo-evolutionists." Where these two scholars differ in their conceptualisation is crucial to Steward's argument. White self-identifies his approach as "unilinear," while Steward's is "multilinear." The fact that Steward accounts for multiple lines of development is a necessary component for his comparative stance, and his belief that weaker lines die out naturally.

argues that not all forms progress and survive, and thus claims that his approach is multilinear because not all can and will progress and survive.

Steward says that “multilinear evolution” is “essentially a methodology based on the assumption that significant regularities in cultural change occur, and it is concerned with the determination of cultural laws” (1952: 318). He argues that its method is about gaining “concreteness and specificity” for comparison and understanding of culture change. Its concern is the generation of taxonomic features, conceptions of historic change, and cultural causality (*ibid.*: 313). He says his method distinguishes social levels and exacts cultural laws through objective scientific means, ultimately demonstrating, he believes, that societies exist on a true evolutionary continuum. Explaining the “meaning of evolution,” Steward says,

Cultural evolution, although long an unfashionable concept, has commanded renewed interest in the last two decades. This interest does not indicate any serious reconsideration of the particular historical reconstructions of the nineteenth-century evolutionists, for these were quite discredited on empirical grounds. It arises from the potential methodological importance of cultural evolution for contemporary research, from the implications of its scientific objectives, its taxonomic procedures, and its conceptualization of historical change and cultural causality... (*ibid.*: 313).

The publication of this paper poses a challenge to the simple understanding of the relationship between Kroeber and Steward (Kerns 2003). Written at Kroeber's request to be included in his tremendous edited anthology, *Anthropology Today* (Kroeber 1953), a 1000-page collection that combines the breadth of the discipline from the top contemporary scholars, this essay, at least implicitly, bears the Dean's approval. Moreover, publishing a paper that called for a return to evolutionary theory and explanations of cultural causality, laws, and evolutionary comparisons specifically as a counter to Boasian anthropology, coupled with Kroeber's unwavering lifetime of support his student – professionally, financially, and personally – demonstrates a complicated relationship between them, and a curious insight into the genealogy of Boasian anthropology in the Americanist tradition (Darnell 2001). This essay provides his self-identified general introduction and framework for his entire theoretical and methodological project. It is of note that this paper is republished as the first chapter of *Theory of Culture Change*, with the new title, "Multilinear Evolution: Evolution and Process" (1955).⁴⁰

C. Steward's third important piece written during this time, as noted by Clemmer and Myers (1999), is the Social Sciences Research Council's (SSRC) Bulletin, *Area Research: Theory and Practice* (Steward 1950). Though relatively unknown, this small book is one of the most curiously demonstrative pieces of literature in Steward's *oeuvre*, demonstrating his project with specifics and clarity. Written while he was on leave for a

⁴⁰ It is also very curious that when Tax (1962) republished *Anthropology Today*, re-titled as *Anthropology Today: Selections* Steward's essay has been excised from the collection and no mention of it is made.

term at the SSRC in 1949 shortly after the completion of the Puerto Rico Project, Steward wrote what was intended as a report on methodologies and practices of “area research” from the “perspective of anthropology.”

Defining “areas” as abstractions determined from scientific analysis, he acknowledges that they could be cultural areas, nations, groups, dependencies, tribes; or that they could be abstracted based on other arbitrary phenomena like race, language, and technology (*ibid.*: 7). Recognising that an “area” could be anything left to the fancy of the scientist, he says, however, that the purpose of area research studies is the primary importance. Recalling the then-recent call for area studies, he notes that it is the responsibility of social scientists

[t]o accumulate and make available a body of knowledge of practical utility regarding the principal areas of the world [though it] could require investigations of every conceivable kind (ibid: 2, emphasis his).

Continuing directly, he notes that

During the war there was an enormous demand for hundreds of different kinds of spot information. So far as this demand is concerned, it can undoubtedly be expected that any area specialist will make available whatever miscellaneous knowledge he happens to possess when needed (*ibid.*).

In *Area Research*, Steward is preoccupied with what he perceives as the plight of anthropology due to its traditional subject disappearing (*ibid.*: 151). With respect to the discipline, he offers the Puerto Rico Project as the exemplar of a relevant anthropological research project, on what he calls “complex and changing societies;” and, he heralds the Project as a strong contribution for the growth of the social sciences (*ibid.*: 154), and for showing the relevancy of anthropology to this growth (*ibid.*: 95). Thus, his report focuses on a project that examines the determinants of culture change in what he considers more “complex societies,” such as Puerto Rico, and he offers a role for anthropology in government research initiatives. He acknowledges that the “concepts and methods” employed in the Puerto Rico Project helped “to ascertain how the influences emanating from a highly industrialized society affected the local or regional varieties of culture found in one of its agrarian dependencies” (Steward 1950: 154). He says, that the purpose of the project was to “understand the influences that have been changing these communities [in Puerto Rico], it was necessary to understand the insular-wide economic, political, religious, and other institutions, including changes in the latter under United States sovereignty” (*ibid.* 155).

Explaining this position, he says the “ultimate justification of social science is that it can predict trends in human affairs – that it can state with some precision what will take place under specifiable circumstances” (*ibid.*: 155). He advocates for anthropology to synthesise the analyses of other social science disciplines, creating an interpretive

hierarchy and structure for the social sciences with anthropology at the top liaising with the government.

For undertaking area studies Steward introduced a new concept, “the levels of sociocultural integration,” that would permit anthropology to contribute more fully to a useful social science amidst what the discipline perceived as the disappearance of “primitive peoples.” In explaining this approach, he says,

In science generally, there is a good precedent for dealing with levels of integration. The distinction between the inorganic, organic, and superorganic is a very old concept and it means that the sciences dealing with each level frame their problems in terms of special aspects of phenomena....

If the basic concept of levels is valid – and this would not seem to be very debatable – types of sociocultural organization no less than the phenomena of the inorganic and organic levels must be divided into sublevels....

According to the principle of sociocultural sublevels, each higher sublevel is more complex than the lower ones not only in the qualitative [sic] sense because it has more parts but, as in biological sublevels, that it has qualitatively novel characteristics or unique properties which are not evident in or foreshadowed by the lower ones. That is, the new whole at each higher

sublevel induces changes in the very nature of the parts and creates new relationships between the parts and to the whole.

This point may be illustrated with a simple and basic phenomenon. The human family is found in all societies but, like the cell, its nature and its functions vary accordingly to the whole. In a few sociocultural units, such as the Eskimo or the Great Basin Shoshoneans, the family more or less constitutes the social, economic, educational, and political whole. The family has persisted throughout world history, but its nature and role in larger sociocultural wholes have changed tremendously. The contemporary American family, for example, has lost many of the primitive functions, while others have been so modified as to give it unique meaning and relationships that are specific to the context of modern civilization.

In the historical development of sociocultural systems, the individual family units amalgamated into larger groups whose nature and functions were very different from those of the family (1950: 108-110).

This telling description provides his rationale regarding evolutionary typologies: Because he assumes Indigenous peoples to be small, simple, and homogenous he believed them to be naturally assimilated by more complex forms; and resultantly, the newly emerged form cannot be an aboriginal one.

Perhaps the most telling analysis of the Bulletin can be taken directly from the “Forward” to the book itself. Paul Webbink, urges caution for accepting the validity of the ideas espoused in the book. In fact, Webbink urges

The appraisal [to follow] is frankly that of one man, and has been approached, as the author himself states, principally from the point of view of an anthropologist. It is clear that at many points geographers, historians, political scientists, or persons trained in other disciplines would have proceeded with different assumptions and somewhat different objectives. Some area specialists within and outside the Council’s Committee question whether any generalized theoretical definition of area research can span the range of situations [herein] (Webbink 1950: viii-ix).

D. Finally, Clemmer and Myers identify “The Levels of Sociocultural Integration: An Operational Concept” (Steward 1951) as the fourth important theory-piece written during this time. This article was published as Steward developed his theoretical position and explored his political acumen in the academy. It is important because he further develops the concept of the levels of sociocultural integration that originated in *Area Research*; and he also applied it for the US Department of Justice in the context of the Indian Claims Commission. In this manner, the “levels of sociocultural integration” is the unfettered nub of his praxis, what he later comes to call the “substantive application” of his theory and method (Steward 1955: 5).

In the article, Steward offers the same basic descriptions of the Indigenous peoples in the Great Basin as his report to BIA in 1936. He combines the assumption of “multilinear evolution” with his new (but still unnamed) method to establish a schema for evaluating the level of a society, and, as well, proposes a method to solve problems of acculturation (1951: 383). He contends that there are “levels” of social forms, and he urges for his method of discernment and ranking to be the manner to categorise them into a typology. In distinction, he claims, that the evolutionary trajectory of each society is not unilinear, saying,

Similarly, this concept applied to culture is essentially heuristic and does not purport to explain the developmental sequences of particular cultural types.

The cultural types of Morgan, Tylor, and others is a developmental taxonomy based on concrete characteristics of cultures. The concept of levels of sociocultural integration, on the other hand, is simply a methodological tool for dealing with cultures of different degrees of complexity. It is not a conclusion about evolution (Steward 1951: 380).

However, in this essay, Steward states that the “family represents a level that is lower in a structural sense, and in some cases it appears to have been historically antecedent to higher forms” (1951: 381-2). Thus, after contending that the absolute purpose of anthropological theory is to generate laws of cultural regularities and to frame these laws within the assumption of multilinear evolution, it is hard to reconcile his claims that the

theory of the levels of sociocultural integration is not a conclusion about evolution given everything that he establishes in this paper and his greater theoretical project. A further discussion of this concept will exemplify this point.

As noted in the original publication of this essay, Steward developed the concept of the levels of sociocultural integration first in *Area Research: Theory and Practice* (1950: 95, 106-114, 152, 168) identifying it as an abstraction subject to the discretion of the scientist (1950: 8). He confirms this again, noting that “the levels can be used as an analytic tool in the study of changes within any particular sociocultural system” (1951: 383). He introduces the concept saying, “within the cultural tradition of each area, sociocultural systems have developed through a succession of levels, each higher level being not only more complex than the lower but *qualitatively* different in that it has characteristics that were not evident in antecedent patterns” (1950: 152, emphasis mine).

A determination of the sociocultural levels, Steward says, is representative of the “growth continuum” of increasingly complex and newly emergent forms. This schema he patterns specifically after the biological understanding of evolution (Steward 1951: 379; Murphy 1981). The bottom level of the scale of sociocultural integration is known as the “family-level;” and representative of this level, the “family was the reproductive, economic, educational, political, and religious unit” with no higher social organization (1955: 54). For Steward, each higher level on the scale is marked by increasing complexity, and is

evidenced by increasing inventories of cultural traits, increasing heterogeneity and the emergence of formal political structures.⁴¹

The theory that he produced during this time was based consistently on an assumption of evolutionary development, and it was markedly anti-Boasian. More abstract, “Cultural Causality and Law” (1949) and “Evolution and Culture” (1953) are general statements about the process of evolution. “Cultural Causality and Law” (1952) develops the historical sequences leading to the formation of the state, and it also calls for the necessity to distinguish core and peripheral features of cultures; while “Evolution and Culture” argues for a comparative science to rank these features in a “cultural taxonomy” (1952: 320).

Area Research and “Levels of Sociocultural Integration” are less abstract attempts to describe a new method for anthropological research. Steward notes, in the case of the Puerto Rico Project, that he organized it to fit with his larger theoretical plan of examining culture change and uncovering cultural laws. In *Area Research* he points to the Puerto Rico project specifically as an excellent example of relevant anthropological research for the social sciences. He also notes that this research was organised through a utility matrix for the government – to secure sovereignty in other territories or to provide

⁴¹ Developed in consort with the US Department of Justice, the “Levels of Sociocultural Integration” was the explicit argument advanced to deny Paiute Indians land rights before the Indian Claims Commission (Ronaasen et al 1999).⁴¹ The levels of sociocultural integration is Steward’s stated praxis (1955: 5), and, I believe, his most important concept. It is the culmination of his ethnographic description discerned through his method and applied in his fully developed theoretical model. As will be discussed more fully below, this essay is later reproduced as chapter three of *Theory of Culture Change*, showing the application of “Multilinear Evolution” (Chapter 1) and “Concept and Method” (Chapter 2). This directive establishes the science of his project, and it is the substance of the theoretical component of the following chapter.

useful information in times of war. Thus, in the *Bulletin*, he offers a programmatic for creating and practicing a new method of anthropological research within the social sciences measured in its utility for the government.

In the essay, "The Levels of Sociocultural Integration," Steward builds heavily on the title-concept, as it was first developed in *Area Research*. As explained below, it belies a Spencerian teleology of progress, complexity, and social evolution. Like "Evolution and Culture," "The Levels of Sociocultural Integration" is directed pointedly at the discipline of anthropology, with a strong critique of the historical particularists, and an even stronger directive to promote a new method so as to rescue anthropological studies from the relativists by employing a "scientific" approach. In these works, Steward sets himself aside from his intellectual tradition by openly critiquing Kroeber, and simultaneously dismissing his contemporaries White and Childe too; he asserts, with no evidence, that his programme is superior to all others because it is backed by "science" and a "proper" method, and that his work in the discipline is *sui generis*.

iv. Steward and Spencerian Social Theory

In recent decades there have been a number of analyses of Herbert Spencer's social philosophy in anthropology and sociology (Bailey and Gayle 1993; Erickson and Murphy 1998; Trigger 1998; Bohannan and Glazer 1988). The authors agree that Spencer's work has a significant relationship to contemporary political theory, and they urge for a greater understanding of it. Within this literature, Spencer is typically represented as a free enterprise liberal whose politics were not just conservative but ahistorical and racist

(Trigger 1998: 57-8). Turner *et al* (2002) provide a somewhat tongue-in-cheek-statement of how Spencer is now represented in contemporary sociology. They say,

Herbert Spencer, the first self-conscious English sociologist, advocated a perspective that supported the dominant political ideology of free trade and enterprise. He naively assumed that 'society was an organism' and developed a sociology saw each institution as having its 'function' in the 'body social,' thereby propagating a conservative ideology and legitimating the status quo. What is even worse, Spencer coined the phrase *survival of the fittest* to describe the normal state of relations within and between societies, this making it seem right that the elite of a society should possess privilege and that some societies should conquer others (Turner *et al* 2002: 45).

A demonstration of the connection between Steward's theory and the sociology of Herbert Spencer is offered to close this chapter. Although there are differences between the two, I will draw four parallels in their work demonstrating a strong sympathy between them, and challenge the notion that Steward's work is "neo-evolutionary" at all (Fried 1968; Bohannon and Glazer 1988; Trigger 1998; Erickson 1998). At the outset it is important to note where they differ, however. First, Spencer believed that generations learn how to cope with their environment and pass this new learning to subsequent generations as instinct (Bowler 2003: 214, 220). Steward rejected this notion, believing

instead that if changes occurred in one generation the original cultural form was destroyed and new forms emerged (Steward 1932, 1937, 1938, 1940).

Secondly, Spencer advocated that the state should be removed from impinging on individual freedoms (Bowler 221), generating a definition of liberalism that is at odds with Steward's. Spencer said that in "essence Liberalism stands for the freedom of the individual versus the control of the State" (Spencer 143; C.f Trigger 1996: 57). As we saw above, in specific reference to *Area Research*, Steward practised and promoted active government social policies to create order, assimilate differences, and promote middle class ideals. Through his work, Steward articulated a position that privileged the state as the individual that demanded and required freedom. The two agree, overwhelmingly, however, that evolution meant progressive change, and the development of more complex patterns and material is the natural result of combative processes – in essence, what survives is the best. It is with this notion of evolution and progress that I will introduce four important parallels between the work of Steward and that of Spencer.

Spencer adhered to a strong belief that the same laws govern both biological and social evolution (Bowler 222). To illustrate this point, in beginning his discussion of "political integration," in the *Principles of Sociology* (1969 [1876]) he offers that "[t]he analogy between individual organisms and social organisms ... holds in respect to the actions which cause growth. We shall find it instructive to glance at political integration in light of this analogy" (Spencer 1969: 202). He continues, drawing on the same referents as Steward and describing the same processes of assimilation, saying,

Every animal sustains life and grows by incorporating either the materials composing other animals or those composing plants; and from microscopic *protozoa* upwards, it has been through success in the struggle thus to incorporate, that animals of the greatest sizes and highest structures have been evolved.

Remarkably similar to Steward, he explains his analogy between social and biological evolution, continuing:

This process is carried on by creatures of the lowest kinds in a purely physical or insentient way. Without nervous system or fixed distribution of parts, the rhizopod draws in fragments of nutritive matter by actions which we are obliged to regard as unconscious. So is it, too, with the simple aggregates formed by the amassing of such minute creatures... At a higher stage, however, the process of taking in nutritive materials by a composite organism, come to be carried on in a sentient way, and in a way differing from the primitive way in this, that directly furthers the life of the whole, and indirectly furthers the lives of the component units...

Analogous stages may be traced in the growth of social organisms, and in the accompanying forms of action. At first there is no other life in the group than that seen in the lives of its members; and only as organization increases

does the group as a whole come to have that joint life constituted by mutually-dependent actions. The members of a primitive horde, loosely aggregated, and without distinctions of power, cooperate for immediate furtherance of individual sustenance, and in a comparatively small degree for corporate sustenance (Spencer 202-3).

Trigger (1998) also picks up on this point, noting that it is the “pressure of feeding” that leads to economic and social development in Spencer’s work (1996: 57), permitting a strong parallel to Steward’s gastric assumption of the Shoshone, and the base motivator for all human organisation through all time.

Second is the notion of environmental determinism, the idea of which runs rampant in Steward’s description of the Shoshone, particularly in his discussion of the formation of the patrilineal band (1936, 1938, 1955). Like Steward, Spencer, too, points to “the inclemency of the weather” and the “infertility of the soil” in keeping population levels low (1969: 205). He also notes that the “structure of the habitat” facilitated the ability or inability for communication, thus forcing social organization to be dependent on the techno-environment. He says, “the illustrations before given, showing that the mountain-haunting peoples and peoples living in deserts and marshes are difficult to consolidate, while peoples penned in by barriers are consolidated with facility” (ibid.: 206).

Statements such as these have easy parallels to Steward’s description of the Great Basin, and other Indigenous peoples throughout the world. Such as when he states, “[o]wing to the natural environment of the Great Basin area and to the simple hunting and gathering

techniques for exploiting it, it was inevitable that the individual family or at the most two or three related families should live in isolation during most of the year” (Steward 1955a: 102).

A third parallel between them is the use of the metaphor of levels or stages of organisation. As seen in the work of Steward, this concept provided the “operational” mechanism to rank and then afford different political rights to peoples of the world. Explaining that this is a conceptual abstraction of the social scientist, he urges that its importance lies in its utility to the government in times such as war or colonization. Albeit Spencer disagrees, seemingly, with the use-measure, he does offer the descriptive framework that Steward uses. For example, he says,

In primitive headless groups of men, such customs as regulate conduct form but a small aggregate. A few naturally prompted actions on meeting strangers; in certain cases bodily mutations; and some interdicts on foods monopolized by adult men; constitute a brief code. But with consolidation into compound, doubly compound, and trebly compound societies, there arise great accumulations of ceremonial arrangements regulating all the actions of life – there is an increase in the mass of observances.

Originally simple, these observances became progressively complex. From the same root grow up various kinds of obeisances [sic]. Primitive descriptive names develop into numerous graduated titles... And besides the

increasing heterogeneity which in each society arises among products having a common origin, there is the further heterogeneity which arises between this aggregate of products in one society and the allied aggregates in other societies....

The advancement in integration, in heterogeneity, in definiteness, and in coherence, is thus fully exemplified (Spencer 160-61).

Finally is the base representation of Indigenous Peoples in both of their work. For Spencer, Indigenous peoples have rudimentary, primitive, or proto-social organisation. Counter to what he believes to be the natural processes of the emergent heterogeneity of "primitive peoples," he says

Besides fitness of nature in the united individuals, social union requires a considerable *homogeneity* of nature among them. At the outset this needful likeness of kind is insured by greater or less kinship in blood. Evidence meets us everywhere among the uncivilized. Of the Bushmen, Lichtenstein says, 'families alone form associations in single small hordes; -- sexual feelings, in the instinctive love to children, or the customary attachment among relations, and the only ties that keep them in any sort of union' (Spencer 207, emphasis mine).

And, specifically about the Indigenous of peoples of the American Great Basin, he says,

The Digger Indians, 'very few degrees removed from the orang-outang,' who, scattered among the mountains of the Sierra Nevada, sheltering in holes and living on roots and vermin, 'drag out a miserable existence in a state nature. Amidst the most loathsome and disgusting squalor,' differ from the other divisions of the Shoshones by their entire lack of social organization. The river-haunting and the plain-haunting divisions of the race, under some, though slight, governmental control, lead more satisfactory lives (Spencer 1969: 185).

Thus, beyond these simple descriptions of Indigenous peoples, Spencer, too, attributes the emergent or manifest social organisation of "primitive" cultures as a reaction to the expansion of Euro-American structures (Spencer 1969: 210).

Spencer's *Principles of Sociology* and his *First Principles* compare well to Steward's work, such as his report to 1936 Collier and the BIA where he describes the "Shoshoni tribes" as "impoverished "foot Indians ...[that] had no political organization beyond the union of families in small villages," and bear more than a striking similarity. The significance of the relationship between Steward and Spencer is a very important connection, one that has not been made in the discipline. In fact, Steward is often mistakenly distinguished from 19th century evolutionism when there is no evidence to do so, and the evidence indicates that he only asserted a difference and the discipline uncritically promoted this idea based on a belief in the neutrality of his theory, what they

allege is buttressed in an uncritical notion of 'science' (Sponsell 2006; Bohannon and Glazer 1998).

Chapter Five. **Julian Steward and the Indian Claims Commission: 1949-57**

Notwithstanding the persistent claims of Steward's scientific objectivity and the political neutrality of his work, in 1949 Steward began working for the US Department of Justice providing testimony to deny Indian land rights before the ICC.⁴² In this and the next chapter, I provide an analysis of Steward's work as an expert witness and strategist for the US Department of Justice. Below, I discuss Steward's work on the Ute cases (Docket Nos. 44 & 45) and his relationship with other anthropologists during this time. In chapter six, I discuss the Paiute cases (Docket Nos. 87, 88, 17 & 100) before the Indian Claims Commission (ICC). I base my analysis on primary-source archival research of Steward's papers and reports written during this time, an analysis facilitated immeasurably by the recent biography of Steward (Kerns 2003).

i. The ICC and US Federal Indian Policy

The Indian Claims Commission Act was passed by US Congress in 1946, and the commission founded under the act, the ICC, was organised as a "tribunal for the hearing and determination of claims against the United States ... by any Indian tribe, band, or other identifiable group of Indians living in the United States." Historically, the ICC was one of the "lasting achievements of the Collier era" in American Indian policy (Kelly 1983), marking the end of Collier's 12-year reign as Commissioner of Indian Affairs (1933-45) – and of one of the greatest anomalies in US federal Indian policy (Philip 1977).

⁴² JHSP, Box 2, Steward to Vanech, June 1, 1949. He also took leave from Columbia for a term in 1949 to the SSRC (Kerns 2003: 258), publishing *Area Research: Theory and Practice* (Steward 1950).

One of Collier's long-term goals had been to establish an independent legal body that could adjudicate Indian claims against the federal government (Dailey 2004). As Collier had hoped, the creation of the ICC, with its mandate "to handle Indian cases exclusively under a broad new jurisdiction," established such a mechanism to deal with the multitude of legal claims that Indian Nations had with the US government (Rosenthal 1990: 47). However, the Commission's mandate was predicated on the assumption that the so-called "Indian problem" could be addressed through compensation for lands taken rather than by addressing the systemic problems that facilitated the taking of the lands to begin with (Lurie 1970). And although some progress was made in resolving outstanding grievances, this progress was tenuous because opposition to these claims was well supported financially and morally (Dailey 2004), legally (Rosenthal 1990), and, as we will see below, "scientifically" (Beals 1985; Steward 1950, 1955b, Manners 1956, Stewart 1986).

Although probably unforeseen, the ICC's broad mandate to hear any claims against the United States on behalf of "Indian tribe, band, or other identifiable group of Indians," allowed for an established line of argumentation frequent in colonial litigations. In effect, the defined list opened the door for an argument that there *could* be a group of Native Americans that was not an "identifiable" group, as it could be argued that the particular claimant was not a band, tribe or group. Following this argument, those claimants that were found to be not of an "identifiable" group had no legal standing before the Commission because of perceived "ethnological difference," based on

evolutionary conjecture.⁴³ As it happened, in frequent and continual practice, the US Department of Justice questioned the level of social organisation of the Indigenous peoples before the court, following a line of argument in the common law regarding the colonisation of new territories by limiting the aboriginal interest in the land based on social evolutionism (Asch 1992, 2002; Reynolds 1992, 1999; Pinkoski and Asch 2004; e.g. Wallace 2002).⁴⁴

As cases before the ICC took greater shape both Native American claimants and the federal government solicited expert testimony on the contemporary, ethnohistorical, and aboriginal areas in question before the court; and anthropologists quickly came to the forefront of the legal discourse for determining Aboriginal interests to land (e.g., Steward 1955b, 1970; Kroeber 1955; Manners 1955; Lurie 1956, Stewart 1986; Ray 1955; Barney 1955). Both the renewed acceptance of “neo-evolutionary” developmental stage-theories within the discipline and anthropologists’ newly promoted “scientific” contribution to the social sciences led Ronaasen *et al* (1999) quite appropriately to conclude that the “very nature of the ICC itself placed anthropologists in a position to legitimize the denial of indigenous rights to collectively held land and to other collective rights guaranteed by treaty with the U.S. government” (171; C.f. Barney 1955).

⁴³ C.f. In *Re: Southern Rhodesia*.

⁴⁴ It should be noted, however, that in establishing its original colonial law, the US courts did not follow a doctrine of *terra nullius*, but instead relied on the doctrine of discovery (Williams and Lomawaima 2001). As Reynolds (1992) clearly states, the doctrine of *terra nullius* has two closely understood meanings that appropriately describe the Department of Justice’s argument before the ICC. The first relates to “a country without a sovereign recognized by European authorities,” and the second relates to a “territory where nobody owns any land at all, where no tenure of any sort existed” (Reynolds 1992: 14). For the ICC cases, the Department of Justice pursued the latter meaning of *terra nullius* and argued that “Indian” Peoples appealing to the Commission had no standing before it based on evolutionary criteria. The magnitude of this legal theory is explained by Slatery (1979) as one of the four means that any state can justify the acquisition of new territories in common law, by what is called the “settlement thesis” or the “acquisition of territory that was previously unoccupied or is not recognised as belonging to another political entity” (cited in Asch 2002).

ii. Julian Steward and the Indian Claims Commission

Reams of documents in Steward's archives demonstrate that Steward had an intimate relationship with the US Government in the creation and presentation of their legal arguments before the ICC. To begin, the Department of Justice contacted Steward in April of 1949 while he was on research leave from Columbia University to the Social Science Research Council SSRC (Kerns 2003: 258). At that time, A. Devitt Vanech, Assistant Attorney General for the U.S. Department of Justice, contacted Steward by letter, stating that "The Uintah Ute Indians of Utah" claim "to have been the exclusive aboriginal occupants of a large area of the land in Utah and Nevada."⁴⁵ In the letter, Vanech asks Steward if he "would be willing to assist the Government in this case with regard to the aboriginal occupancy of the area in question." Vanech explained the government's request to Steward because of his "authorship of Bulletin 120 of the Bureau of American Ethnology, 'Basin-Plateau Aboriginal Social Groups' [sic], and other related studies...".⁴⁶ Within a week, Steward replied succinctly and with positive interest to Vanech, requesting a clarification of duties, expectations, and recompense.⁴⁷

Very quickly a response from the Department of Justice outlining the basic details of the case, as well as articulating their expectations for his contributions, was forwarded to

⁴⁵ University of Illinois Archives, Julian H. Steward Papers (Record Series 15/2/21) [hereafter JHSP], Box 2, Vanech to Steward, April 21, 1949

⁴⁶ Ibid.

⁴⁷ JHSP, Box 2, Steward to Vanech, April 26, 1949

Steward.⁴⁸ In this letter, Acting Assistant Attorney General J. Edward Williams gave details of the case to Steward, noting that it was *The Uintah Ute Indians of Utah v. United States*, Docket no. 44 before the Indian Claims Commission, for which they were interested in retaining his services; Williams explains that the “plaintiffs claim that they are a band of Ute Indians whose members comprise bands known formally as the Utah or Ute or Uintah, Yampah-Ute, Pah-Vant, San-Pete-Ute or San-Pitch-Ute, Tim-Pa-Noy, or Timpanoags, and Cumm-Bah Bands.”⁴⁹ The letter continues, stating that the “[p]laintiffs further claim that prior to 1865, they had exclusively and immemorially used and occupied about 25,000,000 acres of lands in Utah and Nevada.” Outlining the specific area claimed to close the letter, Williams asks

[in] the event that your assistance in the case should be found necessary and could be obtained in mutually satisfactory terms, it would be desired that you prepare and present testimony as an expert relating the boundaries of the particular area, if any, of which the six above-named bands were the exclusive aboriginal occupants. It would be contemplated also that you testify in regard to tribal distributions in all parts of the above-described area aboriginally occupied by tribes, bands, or groups of Indians other than the plaintiffs.

⁴⁸ JHSP, Box 2, Williams to Steward, May 3, 1949

⁴⁹ Ibid.

If you would be interested in assisting along these lines, it is suggested that you outline, for the Department's future reference, the condition of your participation.⁵⁰

Almost one month later Steward answered this offer with a brief response, indicating that he would "be quite willing to furnish testimony [sic] as an expert regarding the lands claimed to have been occupied by the Ute Indians." He offers his involvement on the conditions, though, that he would only do the work if he was available, and if there was adequate remuneration. To this point, to end his letter, Steward says,

I have presumed that the Government has fixed fees for expert testimony, and I should be interested to learn what they might be in this case. May I have some indication of when the testimony might be needed, the amount of time that might be required in Washington, and the customary compensation?⁵¹

Not until early September, or about four months later, does the government respond to Steward's request for information about remuneration, informing him that he would "receive an estimated 15 days at \$40.00 per day to prepare and testify as an expert witness in the *The Uintah Ute Indians of Utah v. United States*."⁵² At this point, upon agreeing to work with the Department of Justice, Steward's correspondence is dominated

⁵⁰ Ibid.

⁵¹ JHSP, Box 2, Steward to Vanech, June 1, 1949

⁵² JHSP, Box 2, Vanech to Steward, September 6, 1949.

by material connected with the Department of Justice and the ICC for the next seven years of his life.

In the letters that follow, Steward is asked to prepare commentary on the Ute's delineated boundaries vis-à-vis the named bands.⁵³ As well, in anticipation of what Vanech believes will be the Ute's legal argument in the case, he summarises three points for Steward. He anticipates that: 1) the territory claimed will be the territory stated in the Spanish Fork Treaty of June 8, 1865; 2) that the land claimed will have been alleged to have been used immemorially by the Uintah-Ute Indians prior to June 8, 1865; and, 3) the claim will regard compensation for the Uintah Valley Reservation, which was set aside by Executive Order in 1861.⁵⁴

In preparation for the trial, Vanech asked for Steward's opinion regarding a Ute amendment to their initial claim. In this letter, Vanech "summarized" the Utes' position for Steward, with his emphasis in the original document:

1) In the original petition in No. 44 (par. 1) it was alleged that the plaintiffs comprised certain bands of Ute Indians in Utah, formerly known as the Utah or Ute or Uintah, Yampah-Ute, Pah-Vant, San Pete or San-Pitch, Tim-Pa-Noys or Timpanoags, Cumm-Bah bands.

⁵³ Vanech to Steward, October 21, 1949.

⁵⁴ The third point corresponds to the claims of Docket No. 45.

2) However, in the amended petition in No. 44 (par. 1) it is alleged that the plaintiffs are a band of Ute Indians, commonly known as Uintah or Utah Indians, and that members of the plaintiff band also belong to certain subordinate groups of said band, sometimes referred to as the Utah or Ute or Uintah, Yampah-Ute, Pah-Vant, San-Pete Ute or San-Pitch-Ute, Tim-Pa-Noys or Timpanoags, Cumum-Bah and Weber-Ute bands.⁵⁵

Vanech closes the letter by asking Steward to advise how accurate these amendments to the claim were, and if the government should respond to it. 10 days later, in what he called a “supplement” to the October 21st letter, Vanech forwards to Steward the government’s “brief filed in the *Pawnee Indian Tribe of Oklahoma v. United States*, No. 10 before the Indian Claims Commission,” asking Steward to pay special heed to the pages that “contain a discussion of the leading decisions with reference to the acquisition of ‘Indian Title’ by exclusive immemorial possession.”⁵⁶ Three weeks after the initial request for an opinion on the amendment, on November 14, 1949, Vanech wrote Steward again, and impatiently urged him to advise the Department of his interpretations of the amendment, because it “is necessary that a determination be made as to whether, and in what manner, the Government will answer the amended petitions in the Uintah Ute cases...”⁵⁷

⁵⁵ Ibid.

⁵⁶ JHSP, Box 2, Vanech to Steward, November 1, 1949.

⁵⁷ JHSP, Box 2, Vanech to Steward, November 14, 1949.

The initial letters between Steward and the Department of Justice demonstrate an important fact. Steward very quickly outgrew his role as a simple expert, as he took on a greater role as advisor to and strategist for the government. As such, Steward, with various representatives from the Department of Justice, began laying out the legal and anthropological framework to identify characteristics of recognisable social organisation in law, and demonstrates that his work is inherently political. The distinctions created by their enquiry led to the legal argument that there were non-identifiable groups of humans in the Great Basin, and thus a jurisdictional vacuum existed.⁵⁸ This claim was made in the face of the historic Treaties in the area.

iii. Steward and Indian Claimants

Clearly unaware that Steward had already been courted by the Department of Justice, in the spring of 1950, Ernest Wilkinson contacted Steward by mail and informed him of the Western Shoshones upcoming claim against the federal government. Identifying himself as representing a group of “claims attorneys for the Western Shoshone” who are “preparing to prosecute a case on behalf of these Indians to recover compensation for the taking of that land by the United States,” he solicited Steward’s aid.⁵⁹ Consistent with the details that Vanech had anticipated for the Ute cases stated earlier, Wilkinson cites the 1863 Governor James Duane Doty Treaties with the Western Shoshone, Goshutes, Northwestern Shoshone, Mixed Bands of Bannocks, and Eastern Shoshones, as reason

⁵⁸ JHSP, Box 2, Steward to Vanech, April 26, 1949; Williams to Steward, May 3, 1949; Steward to Vanech, June 1, 1949, Vanech to Steward, September 6, 1949; Vanech to Steward, October 21, 1949; Vanech to Steward, November 1, 1949; Vanech to Steward, November 14, 1949; Wilkinson to Steward, April 4, 1950; Wm. Amory Underhill to Steward, February 7, 1952; Steward to Yost, February 12, 1952.

⁵⁹ JHSP, Box 2, Wilkinson to Steward, April 4, 1950.

for their claim. In the lengthy letter, Wilkinson writes a detailed explanation of how he views the legal case. I quote this letter at length because of the series of events that transpire directly from it, and because of the specific statements that it makes. Wilkinson writes,

We have for sometime been preparing to prosecute a case on behalf of these Indians to recover compensation for the taking of land occupied and possessed by them prior to the taking of that land by the United States....

A few years ago we prosecuted a suit on behalf of these Indians claiming that the afore-mentioned treaties constituted treaty recognition of the land owned and occupied by the Northwestern Shoshones. That case was decided against the Indians by the Supreme Court of the United States in a 5 to 4 decision which held that though the land was owned by the Indians by aboriginal possession the Indians could not recover in that suit because the treaty did not constitute a recognition by the United States that the Indians did possess the land in question.

Since the decision of the Supreme Court in the Northwestern Shoshone case, the Indian Claims Commission Act of 1946 has authorized suits before the newly created Indian Claims Commission, and has specified, as one of the grounds for compensation to the Indians, recovery by the Indians where there has been a taking without compensation by the United States of land to

which the Indians had aboriginal title by the reason of occupancy to the exclusion of other Indian tribes. We, therefore, propose to bring a suit, or suits, against on behalf of the various bands of Shoshone and Bannock Indians based upon the taking of land aboriginally possessed by those Indians.

In the prosecution of a case based on aboriginal possession the question of whether the Indians owned and occupied the land in the usual Indian manner to the exclusion of all other tribes becomes primarily a question of fact.

This question of fact may be proved by the experts in the field of Ethnology, by historical and scientific writings, and by official documents.

Accordingly, in view of your many writings and extensive study of the Indians in the Great Basin, we are desirous of conferring with you to discuss the basic questions of fact involved in this case.

In a recent Court of Claims case decided in favour of the Indians of grounds of aboriginal possession (Alcea Band of Tillamook v. The United States) Dr. John P. Harrington appeared as an expert witness of behalf of the Alcea Indians and testified as to the extent of the Indian occupancy in that case. Some of his manuscripts on the Alcea which have been written for the Smithsonian Institute, Bureau of American Ethnology, were introduced as evidence. This testimony and evidence, in addition to other evidence, tended to prove aboriginal possession on the part of the Alcea Indians. The

court found in that case that the Alceas had possessed the lands in question, in the usual Indian manner, and that they are entitled to recover for the taking by the United States of the land in 1855.

As previously indicated, we feel that you could be of substantial service to us with certain phases of this case and we, therefore, would like to schedule an early conference with you to discuss the matter. We will be willing, of course, to adequately compensate you for the time and effort you will spend in this regard.

[Ernest L. Wilkinson, Signed]⁶⁰

There is no record of Steward's response to Wilkinson in his archive; however, the next letter in succession on this topic in his files is a received letter from the Department of Justice.⁶¹ Expanding on the information shared earlier, Vanech tells Steward that the government's interest in the case focuses specifically on the claim asserted by the Utes "based upon the alleged taking by the defendant 'of land formerly held and occupied in the accustomed Indian manner by plaintiff Indians as a group or confederation.'"

Specifying that the Department of Justice is countering with a ethnographic description of non-identifiable groups or proto-sociality, Vanech continues, that "[w]e do not believe

⁶⁰ Ibid.

⁶¹ JHSP, Box 2, Vanech to Steward, April 11, 1950; with handwritten annotations, dated April 16, 1950 (most likely in the hand of his secretary).

this [the Ute's claim] to be correct, and construe the discussion under sections 1(a) and 1(b) of your notes to sustain our position in this respect."⁶²

At the bottom of the first page of this letter from Vanech to Steward is a handwritten acknowledgment from Steward, saying that it was:

answered Apr. 16, 1950 stating: band identity & chief's authority a fiction of U.S. treaty-makers; chiefs are war chiefs of shifting authority; Ute are a language group only; territory claims is that of the Ute-speaking in Utah,

Requested compensation for [Jay] Jones on Ute history research.

Mentioned visit of Robert Barker from Wilkinson's office.⁶³

Steward's handwritten response to the Department of Justice indicates, for the first time, what the outline of his report was going to be. It was written shortly after receiving the letter from Wilkinson that framed the plaintiff's legal position, and immediately upon receiving the most detailed instructions from Vanech on what the report needed to state for the government's legal argument. Up to this point in his academic work Steward had never stated that band identity and political authority were a fiction of US treaty-makers. In fact, he had made little mention of treaty-making in any of his reports. It was only after finding its importance to the Ute's claim, combined with the instructions from

⁶² Ibid.

⁶³ JHSP, Steward to Vanech, Box 2, April 16, 1950

Vanech, that he introduced this component to his analysis. It is worth pointing out that in *Basin-Plateau*, he acknowledges that

[t]he treaty of 1863 included all of the Shoshoni of northern Nevada. They were given the Western Shoshone or Duck Valley Reservation in 1877, but by no means all Shoshoni went to it. A few of the more westerly Shoshoni joined Paiute on reservations in western Nevada, but most Shoshoni remained near their native haunts, gradually abandoning their native economy and attaching themselves to ranches or mining towns (Steward 1938: 7).

Overall in his ethnography, Steward is consistent in his description of what he believed to be culture change through contact, but it is important to note that he does not question the legitimacy of the treaty, and rather contextualises it in terms of the history of establishing political relations in the Great Basin (*ibid.*: 6-9). It is evident that his position, once again, changed dramatically.

Steward's correspondence pertaining to the ICC demonstrates several important factors about his work with the Department of Justice, and the relationship of this work to his claimed scientific method. In turn, I will introduce several themes from this relationship drawing heavily from archival letters to and from Steward regarding the ICC. The letters demonstrate, overwhelmingly, that Steward played a leading role in the discipline of anthropology on this matter and acted as a liaison with the government, often vetting,

editing and changing other anthropologists' work to suit the needs of the government's legal argument. In this manner, Steward was a strategist, organiser, and shill for the government developing his theory, "slanting" his position, and misrepresenting data in service of the colonial agenda.

iv. Steward's Supporting Cast

By the summer of 1951 Steward added two new components to his work for the Department of Justice. First, two new Dockets were added to his purview: The Northern and Southern Paiute cases, Nos. 87 and 88 respectively; and secondly, because he required help "to assemble pertinent data" in the case, he requested that "an advanced graduate student" be added as his assistant. For this role, Steward requested Robert Murphy, whom, he said, "has now completed all instruction toward the PhD and needs only to take the final examinations and write his dissertation." Steward also said Murphy "is generally conceded by the faculty in the department to be the most able of the eighty odd graduate students now working with us."⁶⁴ Writing to Lee Yost, from the Lands Division of the Department of Justice, Steward said,

It is my proposal that Mr. Murphy be employed for a period not to exceed two months at a rate of \$1.50 per hour, to which he has agreed. I state two months as the maximum period, and I believe that the necessary research might be completed sooner. I feel, however, that ample research should be done on the problem of native groups, land occupation, and land use of the

⁶⁴ JHSP, Box 2, Steward to Yost May 7, 1951.

Northern and Southern Paiute Indians because the situation among these Indians was so atypical of North American tribes as to confuse most historians, Indian administrators, and even many anthropologists. Present day Indians are generally unable to [present] the essential data concerning their ancestors of two and three generations ago. In the case of the plaintiffs, it is possible to ascertain the facts of aboriginal occupancy only by interpreting the early historical evidence.⁶⁵

Unmistakably defining his role, lastly, Steward adds, "If Mr. Murphy's appointment to this task can be arranged, I can proceed with the preparation of testimony. I would work closely with Mr. Murphy, consulting frequently and directing his work."⁶⁶

Later that summer two letters demonstrate the development of Steward's ICC work. The first, from Williams, asks Steward if he can come to Washington to confer on the Uintah Ute and the Paiute dockets before they are tried by the ICC,⁶⁷ demonstrating that the strategy for at least the Great Basin cases had been focused and collapsed into one. The second letter, handwritten from Bob Murphy, indicates that he had, indeed, been hired as Steward's assistant and preparing a report on the Yahooskins for Docket No. 17.⁶⁸

In December of 1951, in preparation for the fast approaching Ute trials, Steward shared his thoughts in several letters to Lee Yost from the Lands Division of the Department of

⁶⁵ Ibid

⁶⁶ Ibid

⁶⁷ JHSP, Box 2, Williams to Steward, August 10, 1951.

⁶⁸ JHSP, Box 2, Murphy to Steward, August 15, 1951.

Justice. First, Steward informed Yost that another of his Columbia students, Paul Ducey, had submitted his report on the Ute and that he will edit it before sending it to the Department of Justice,⁶⁹ indicating that at least two of Steward's students from Columbia were working for the Department of Justice under his guidance at this time. Secondly, of Ute history, he writes "that the most important point is the history of the horse and the change in culture and therefore in tribal classification in response to the horse."⁷⁰ In effect, echoing Murphy's claim about the Yahooskins a few months earlier, Steward comments that, as his

research has progressed on the Ute I have rapidly lost any original notion that these Indians had something that could be called "bands". Evidence on habitual occupation, aboriginal groupings, and boundaries is largely negative. While the Ute as a whole could be roughly bounded (except perhaps in the south, where the distinction between Ute and Southern Paiute is meaningless), I don't see how any lines can be drawn between Ute groups.⁷¹

Thirdly, after having gone through the Ute's exhibits in preparation for court, Steward states that he tried to focus on the historical material before 1850, with a particular examination of the period of colonial expansion into the area 1820-50. He notes that the "Ute's groupings" underwent profound changes during this time and the cultural forms

⁶⁹ JHSP, Box 2, Steward to Yost, December 26, 1951.

⁷⁰ JHSP, Box 2, Steward to Yost, December 7, 1951.

⁷¹ JHSP, Box 2, Steward to Yost, December 27, 1951.

after contact were no longer aboriginal,⁷² and thus establishes the second prong of his attack on Indians' claims to land: The first, questioning the minimal social organisation of the groups, by claiming material impoverishment; and the second, an attack on the possibility of any aboriginal cultures continuing to exist in the face of contact and the "penetration" of Euro-Americans – thus making the treaties false.

v. Steward and the Uintah Ute Case

In his first case before the ICC, Steward presented a 71 page expert report on behalf of the Department of Justice entitled, "Aboriginal and Historic Groups of the Ute Indians of Utah: An Analysis."⁷³ His report identifies a three part "problem" regarding the "identification, characterization, and localization of aboriginal groups" in the Ute's territory; "facts," which he says are crucial, for establishing Native American interests in and rights to the land (1952: 1). The first problem is that the concept of "tribe" has no relevance to the Ute; asserting that the "customary division into tribes is a 'white man's' classification system."⁷⁴ The second problem, closely related to the first, is that the land use and the socio-political structure of the aboriginal groups are so unique that they cannot be understood in terms of Anglo-American patterns and concepts.⁷⁵ The third problem is that the data to enable any analysis is too scarce to make any reasonable judgment, and thus all analyses are simply an interpretation or "guesswork."⁷⁶

⁷² JHSP, Box 2, Steward to Yost, December 31, 1951.

⁷³ A copy of the report with handwritten annotations appears in Steward's archive (JHSP, Box 2); a published version of the report has been published as Steward (1974).

⁷⁴ This is a topic specifically taken up by Fried (1975).

⁷⁵ A line followed by the interpretivists presently (e.g. Nadasdy 2003).

⁷⁶ JHSP, Box 2, Steward to Williams, July 10, 1953.

Thus, he argues that the so-called Tribes of this area, the Northern and Southern Paiute, Bannock, Shoshoni, and Ute, are “not aboriginal names” and none had “an awareness of common culture let alone political unity” (*ibid*: 1). He says that before the horse made its way to the Great Basin, the Indigenous peoples shared a similar “low” culture and lived in scattered families because of the environment (*ibid*: 5-7), but that after acquiring the horse, a new mode of subsistence and new social organisation emerged (*ibid*: 3).

Explaining, he says that because there was no farming, families depended entirely upon their immediate environment (*ibid*: 7), and like amongst other American Indians, the legal concept of exclusive ownership of soil was very rare (*ibid*: 10-1). He states that by 1776 the Ute of Western Colorado were well mounted (*ibid*: 13), leading to a great disruption to the aboriginal cultural form (*ibid*: 16); and claims that political consolidation of any kind could occur only after the horse was introduced to the area.

In essence, he argues that horses permitted political organisation through mounted groups that hunted the bison to extinction (*ibid*: 9), that predatory raiding bands emerged from this new mode of production that menaced settlers, and a general change in social organisation occurred rendering the Ute “qualitatively” changed. The effect of the horse was so great that the white settlers, when they arrived in the area afterwards, made a significant mistake in their signing of Treaties with the Ute. He says,

The whites did not understand the nature of Ute chieftainship. They evidently assumed that the Ute had fairly powerful political leaders, like those among many of the tribes east of the Rocky Mountains, and, as their

negotiations with the Indians required persons who could speak for the tribe, they tended to ascribe powers to the “chiefs” which these men did not have. They did not recognize that individuals who rose to prominence during the Indian wars had not been tribal chieftains in native times (*ibid*: 6).

For Steward, in the face of the existing Treaties in the area, he invalidates them claiming that the “whites” made an error negotiating with the Ute in the first instance, because they had no chiefs or legitimate signatories to international covenants because aboriginally the Ute had no horse; moreover, when they did acquire horses they were no longer “aboriginal” – undermining their claim in both instances.

He ends his report with a summation of then present-day Ute social organisation. He says that because of the “unrestricted nomadism” there was significant movement and, therefore, confusion about membership aboriginally (*ibid*: 69); building from this confusion, he says,

The Uintah Ute never had the internal social or political organization to constitute a political unit. It was not until the Indian Reorganization Act was passed during the nineteen thirties that the people on the Uintah Reservation acquired the machinery for government. The Uintah Ute never claimed exclusive rights to the areas they habitually exploited. They

defended it neither against one another nor against other tribes... (ibid: 71).⁷⁷

Not only undermining the credibility of the signatories to the historic Treaties, Steward then calls into question the possibility for any present-day Indigenous knowledge of the culture history of the Great Basin. He asserts that science cannot know anything of the culture area before 1850 because there are few trappers and explorers' accounts of the area. He also says that early reports from the Office of Indian Affairs should be ignored because they are mostly "enumeration of 'chiefs' and 'bands' which were prominent in the Indian wars..." (*ibid*: 19). And, the information gleaned after the arrival of white people only shows a degraded culture; and Indians from the Great Basin know nothing of their traditions or history themselves because of culture loss (*ibid*: 19-20).

Interestingly, within a few weeks of his testimony, including this report, Williams, from the Department of Justice, informed Steward that the "plaintiffs" had filed a memorandum to have his "reports and testimony in the Uintah Ute cases" stricken from the record.⁷⁸ Although I am uncertain if this was a unique action or just simple legal tactics, the response from Steward indicates that he took the criticisms of his work extremely personally, as he replied with a heated three page letter to Yost answering the questions of his competence and bias, ultimately asking if he should write more to explain his position. In his defence, he asserts that his opinion is a guess, like anyone's,

⁷⁷ The ICC did not rule on the Ute case until 1957.

⁷⁸ JHSP, Box 2, Underhill to Steward, February 7, 1952.

but that he used the best methods, got along well with Utes, and is the true expert of the area, insisting that he is beyond reproach.⁷⁹

There is no indication that the government made Steward aware of their response to this memo to have his testimony quashed for over one year. In fact, it wasn't until in the context of discussing the strategy of the next set of ICC cases regarding the Paiute, that Steward informed Williams that the Department has done an "excellent job in answering the Plaintiff's charges" against his testimony.⁸⁰ I will return to a discussion of this letter about the Paiute cases in chapter six, but first I will discuss the Ute decision, and Docket No. 44.

vi. THE UTE DECISION

The Uintah Ute cases were decided by the ICC with Opinions and Findings of Facts offered on February 21, 1957 (ICC 1974: 360). In no uncertain terms, the court rejected Steward's testimony, and his interpretation of the culture history of the Great Basin. In stating seven "principal issues" that guided their interest in Docket No. 44, Chief Commissioner Witt asks

- 1) What area or areas, if any, were occupied by ancestors of plaintiffs?
- 2) Were the ancestors of plaintiffs such an aboriginal entity as to enable them to hold Indian title to these lands?

⁷⁹ JHSP, Box 2, Steward to Yost February 12, 1952.

⁸⁰ JHSP, Box 2, Steward to Williams July 10, 1953.

- 3) Are plaintiffs the descendants or successors in interest of the original owners of these lands?
- 4) Did the ancestors of plaintiffs divest themselves of whatever interest they may have had in the claimed area by been party to any treaties or agreements?
- 5) Have plaintiffs been estopped to assert their alleged claims...?
- 6) What are the boundaries of the lands allegedly occupied by the ancestors of plaintiffs?
- 7) What was the date of the taking of the land, if any? (*ibid.*: 361).

Noting that the claimed area in No. 44 was approximately 11,000,000 acres of land (1974: 362),⁸¹ Commissioner Witt ruled that, “as indicated, the occupancy of a major portion of the claimed area is adequately established by the record” (*ibid.*: 363). To demonstrate the reasoning for this ruling, he contrasts the two arguments presented to the Court. First, he understands that the US government, as the

defendant denies the occupancy of the Uintah Valley by the Ute Indians in aboriginal times. This denial seems to be based solely on the information supplied by Escalante. However, it seems that before the Comanche could have pushed the Ute out of the Uintah Valley the Ute would have had been in there... [Their account] does not alter the fact that it was Ute country and the Ute remained there or reentered there and held it. We feel that the

⁸¹ Not including the 2 Reservations in claimed in No. 45, the Uintah and Ouray Reservations (ICC 1974: 362).

evidence supports the claim of occupancy of the Uintah Valley by the Ute Indians who came to known as Uintah (ibid: 366-7).

Continuing the Commission states that, “[a]nother allegation of the defendant goes to the lack of an aboriginal entity which was capable of holding Indian title to the area claimed” (ibid. 367). In saying that the criteria for determining title should be “the situation which exists in the particular area under consideration,” the Commission ruled that “[t]he evidence, as embodied in the findings, indicates that there was a certain amount of cohesion among the five groups which eventually went in whole or in part, on to the Uintah Reservation, and whose descendants are plaintiffs herein” (ibid: 367).

The Commission goes on to say that there was strong evidence to indicate “an overall organization even prior to white settlement” (ibid: 368), and then taking a specific swipe at Steward’s testimony, they say

The evidence with reference to the distinction between the Ute and Southern Paiute is both lengthy and involved. However, that may be, it would appear that the most telling argument against the defendant’s contention that there is no classification of the Ute and the Paiute seems to have been accepted by nearly everybody, including defendant’s expert witness.

... So far as the evidence shows these Indians, whatever they were called, had been in this area since time immemorial. If at some indeterminable

period prior to the coming of the white man, certain of them acquired horses and thereby changed their mode of living so as to make them a distinct group and that group, with whatever component parts it might have, exclusively used and occupied a definable area of land, then they are entitled to pursue their claim before this Commission and to recover thereon if they present the proper proof to sustain their allegations with regard to the aboriginal area claimed. (ibid: 368-9)

To round out the ruling, the Commission also reinforces the legitimacy of the Treaties (ibid.: 376), finds that the Ute have not ceded title (ibid.: 380), and acknowledges that the Utes hired a lawyer in 1894 to press Congress for their rights (ibid.: 381). These factors caused the Commission to rule that the “defendant is liable to plaintiffs for the value of the land stated in Finding 3” (ibid.: 386).

There are many other important details about the Ute cases, such as the determination of the actual territory recognised and its worth, but in reference to Steward three points have been demonstrated. First, Steward’s work at Columbia, including the Puerto Rico Project, continued to be applied in consort with the exertion of US sovereignty over new lands and peoples; and this relationship unequivocally involved his students in his projects. Secondly, he was inconsistent in his opinion of the social organisation of the Indigenous peoples of the Great Basin, changing his opinion in the context of undermining Indigenous Peoples’ claims to their lands in court. Thirdly, the ICC rejected

his account, outright, with the Commission ruling very close to what had been initially outlined as their claim.⁸²

Given the Commissions' Findings and their rejection of Steward's testimony for the Ute cases, it is somewhat of a wonder as to why the government continued to use him as an expert. It is possible that because it took 5 years for the ICC to rule on the cases that the Department of Justice continued to retain Steward, not knowing the efficacy of his testimony. It is also possible that the legal objective of the Department of Justice was not focussed on the Commission's Findings, but rather on the next level of court proceedings.

⁸² JHSP, Box 2, Wilkinson to Steward, April 4, 1950.

Chapter Six. The Paiute Cases and the University of Illinois

In August of 1952, after completing his hastily organised testimony for the Ute cases, Steward left Columbia to take a Research Professorship at the University of Illinois (UIUC) in Urbana-Champaign. His new position offered fewer teaching and administrative duties; and his reduced workload at the university coincided with his increased role for the Department of Justice in preparation for the much larger, and certainly much better organised, cases against the Paiute (Nos. 87, 88, 17, 100) and the combined case, *The Indians of California v. the United States* (Nos. 31 and 37).⁸³ In addition to acting as an expert witness and strategist for the Paiute cases, as he did for the Ute cases earlier, with this move Steward undertook more responsibility by supervising and heavily editing the work of Erminie Wheeler-Voegelin and his several high-profile research assistants who were also working on the cases.

i. UIUC and Steward's Research Crew

In 1952, after training many soon-to-be prominent American materialist anthropologists at Columbia, Steward and his family moved to Urbana, Illinois where he became the third anthropologist in the joint anthropology and sociology department at the University of Illinois.⁸⁴ Fresh upon his arrival in Urbana, Steward's stature within the discipline heightened: He was quickly awarded the Viking Medal from the Wenner-Gren

⁸³ Together, the dockets comprise much of the western United States, including what is now: Utah, Nevada, Arizona, California, Idaho, and Colorado. The *Indians of California* case pitted Steward against Kroeber.

⁸⁴ Steward and sociologist Kingsley Davis had been recruited from Columbia by John W. Albig from the University of Illinois; but, only Steward accepted the new post (Kerns 260, 373 ff92). Steward's last year at Columbia saw several of his graduate students complete their degrees, with Diamond, Fried, Mintz, Padilla, and Wolf all defending their dissertations in 1951. Eleanor Leacock defended the following year; Marvin Harris in 1953; and Robert Murphy, Louis Faron, and Marshall Sahlins in 1954.
<http://www.columbia.edu/cu/anthropology/alumni/main/directory/>, last visited June 26, 2006.

Foundation in honour of his editorship of the *Handbook of South American Indians* and for his work on social organisation and cultural evolution; shortly thereafter, he was elected to the National Academy of Sciences (Kerns 2003: 269-71; Manners 1973: 894); and in 1955, the University of Illinois published his theory opus, *Theory of Culture Change: The Methodology of Multilinear Evolution*. If Steward's stature rose at Columbia it certainly crested while he was at Illinois.

Kerns suggests that though Steward's new position freed up his time, provided several research assistants, was generously funded, and offered greater pay, the reason for his move was because he was happy to leave the cut-throat environment of Columbia academics, the confrontational departmental politics, and the responsibility of training so many raucous students (2003: 260). She also reports that Steward was put-off by the outright challenges he received from students, and found their lack of middle class desires unsettling (*ibid.* 237). Ironically, however, if Steward was trying to avoid training graduate students or the demands of having students around him, according to Kerns herself, immediately upon his arrival in Illinois he arranged teaching and research positions for several of his Columbia students. These students include Robert Murphy and Paul Ducey, Columbia PhDs from 1954 and 1956 respectively, who worked for Steward beginning in the early course of the ICC research, assisting while Steward was still at Columbia. Murphy's wife, Yolanda, joined her husband and the Department of Justice research project when it was at UIUC, as did Robert Manners and Elman Service, Frederick Lehman, Ben Zimmerman, and Eric Wolf (Kerns 264-5).⁸⁵

⁸⁵ Though Wolf worked as Steward's research associate during this time there is no indication that he worked on any of the ICC material.

Most importantly, the growth of the team was seen in its connection to the founding of the Ohio Valley Historic Indian Conference (OVHIC), the predecessor to the American Society for Ethnohistory. After first recognising the need to address the issues of the history of “Indian tribes” and for facilitating the exchange of ethnohistorical data with an infusion of undisclosed funds

The second step toward [forming the] OVHIC came in early 1953 when the Illinois State Museum called a meeting to be held at Urbana, Illinois on May 7, 1953, for the purpose of considering the feasibility of organizing a conference and drawing up plans for conducting a program of identification of historic Indian tribes and artefacts.⁸⁶

In the inaugural issue of *Ethnohistory*, “Chairman” Wheeler-Voegelin identified that her organisation was an off-shoot of the OVHIC by saying,

we hope that OVHIC will establish itself firmly as a specialized organization, and that ETHNOHISTORY will become firmly established as the organ of a new field of academic interest. Individual scholars have been contributing to this field for many decades, but up to now ethnohistory has not been represented by any particular group. With the present group

⁸⁶ “The History of OVHIC,” *Ethnohistory*, Vol. 1(1), April 1954, pp. 4-6.

recognition comes much that is heartening and that augurs well for the future of the field as a whole.⁸⁷

And further establishing the connection between the two, in the "Front Matter" to Volume 2(1) it states that

ETHNOHISTORY is published quarterly by Indiana University, is sponsored by the Ohio Valley Historic Indian Conference, and is represented on the Editorial Council of the American Anthropological Association.⁸⁸

ii. The Paiute Cases

As the OVHIC grew into the American Society for Ethnohistory, the Department of Justice's involvement in this process became more evident. Steward's correspondence with his colleagues illustrates that the connection between the people and the institutions became more intimate as well, and that he was the conduit for this research between them and the Department of Justice. For example, in a letter to Steward, Department of Justice anthropologist Jay Jones indicates that he is covering Lee Yost's files while Yost is away.⁸⁹ Jones informs Steward that he will be working on the Paiute cases closely with Erminie Wheeler-Voegelin, whom he says has "an interesting reconstruction on their identity, etc." He also tells Steward that Wheeler-Voegelin's report will be made

⁸⁷ Erminie W. Voegelin, "A Note from the Chairman," *Ethnohistory*, Vol. 1(1), April 1954, pp. 1-3.

⁸⁸ From the Front Matter, *Ethnohistory*, Vol. 2, No. 1 (Winter 1955).

⁸⁹ JHSP, Box 2, Jones to Steward, August 21, 1953.

available to him promptly because Steward must look at her report before writing his, because “we have to have our experts walking in the same direction, at least.”⁹⁰ Similar to the preparation for the Uintah Ute cases, Jones anticipates the Paiute’s argument for their case and informs Steward of the Department of Justice’s thoughts on it. Jones assumes, correctly, that another of Kroeber’s students, Omer C. Stewart,⁹¹ will be the opposing expert, and that Stewart will try to establish Ralph Linton’s definition of a band, basically arguing that the Paiute spoke the same language, had a common culture, and occupied contiguous territories that were used exclusively.⁹²

Jones also informs Steward that he “made up a blank bid for [him] to fill out, if [he was] interested to research the Shoshoni for us.” Explaining that “since there is 100 million acres involved, as well as four overlapping groups; it would be well to make adequate provisions for the time and the expenses that may be involved.”⁹³ He then offers Steward an un-tendered contract, advising him to make a liberal proposal for research monies on the claim, noting that there is a “provision for fieldwork, and another for using graduate students.” Jones says that he “listed all kinds of expenses” he could think of for Steward’s contract, but encourages him, if he thinks of more to “go ahead and list them.”⁹⁴

⁹⁰ Ibid.

⁹¹ Omer C. Stewart was also a student of Steward’s at Utah before continuing on to do graduate work at Berkeley with Kroeber.

⁹² Ibid.

⁹³ Ibid.

⁹⁴ Ibid.

In the letters between Jones and Steward personal and professional comments are intertwined. In one, Jones writes of a trip to Cornell with "Lily," of preparations for the upcoming testimony of Kroeber in the Indians of California ICC case, and of his own personal pursuit of an academic position.⁹⁵ Later, he writes to Steward thanking him for his support "in regard to the jobs at Colgate and Indiana," and informs Steward that he has accepted the position at Indiana, where he will continue to work with Wheeler-Voegelin. Jones also tries to convince Steward to help recruit Robert Murphy as his replacement at the Department of Justice; and the Department of Justice's attempt to enlist Murphy does not resolve itself until a few months later, when Steward reports to Ralph Barney, with finality, that Murphy will not accept the position, and instead "prefers to remain here at Illinois."⁹⁶

In preparation for the Paiute cases, a conference was set up for November of 1953 at Indiana University. A couple of weeks before the meeting Steward was informed that he was "authorized to proceed to Bloomington, Indiana, for a conference with Dr. Erminie Wheeler Voegelin, Mr. Ralph Barney and Mr. Leland Yost in regard to the above cases [Nos. 87, 100, 17]."⁹⁷ In advance of the conference, Steward sent Yost and Wheeler-Voegelin his draft report on the Northern Paiute hoping to discuss it at the meetings. He

⁹⁵ JHSP, Box 2, Jones to Steward, March 1, 1953.

⁹⁶ JHSP, Box 2, Steward to Barney, November 30, 1953. After the Bloomington meetings Steward wrote to Barney recommending a couple of "men" who might be suitable to replace Jones at the Department of Justice. Noting that a person "who might have the qualifications which you definitely need for this job... is a little hard to think of..." but he does recommend Robert Armstrong from the Puerto Rico Project and Alex Kreigger from the University of Texas to possibly fit the bill.

⁹⁷ JHSP, Box 2, Perry Morton to Steward, October 23, 1953. Curiously, of the people mentioned for Steward to confer with, Jay Jones, who had just joined Indiana from the Department of Justice, and had been working on these cases earlier, was not listed.

asked Wheeler-Voegelin to provide any extra citations that he may have neglected, and then he asks about the meetings,

I was wondering what we had to meet about all those days until someone showed me a clipping from a month ago telling about the \$33, 000 Indiana has received for investigations. Or has this anything to do with it?⁹⁸

Still preparing their reports for the Paiute cases, in another important letter Yost informs Steward that money for two more assistants has been allocated to help with the maps of Docket Nos. 17 and 87.⁹⁹ More importantly, however, are the instructions that Yost gives Steward. Yost says,

To be completely prepared in No. 17, you should really read all the exhibits. The petitioners and defendant's exhibits total only about 75. However, I do not have these in a form so I can conveniently send them to you. Therefore I think for the present it would be sufficient for you to study the portions of the briefs pertaining to aboriginal occupancy since the most important exhibits are rather thoroughly discussed, and in addition the appendix to the Government's brief on appeal sets forth many of them verbatim. So I am sending you, under separate cover, complete set of both the trial and appellate briefs for both parties.

⁹⁸ JHSP, Box 2, Steward to Wheeler-Voegelin, November 1, 1953.

⁹⁹ JHSP, Box 2, Yost to Steward, December 10, 1953. Yolanda Murphy and Nancy Eagle were hired to do this work.

You ought also to reread the opinion of the Court of Claims, a copy of which was sent you before the Bloomington conference, since it is extremely important that, on the next appeal, that court have before it expert testimony which will correct its present misconceptions.¹⁰⁰

Through telephone calls and letters Steward and Yost planned the division of labour for the reports for each of the cases, and the substance that was to be included in them. Steward was then left with the job of instructing the team of anthropologists working on the case. In terms of his students, this relationship was of instruction and supervision;¹⁰¹ with respect to Wheeler-Vogelin, it was authoritative and controlling. For example, after one of his telephone calls with Yost he wrote a detailed letter to Wheeler-Vogelin instructing her how to proceed with her report. Explaining the changes required, he says,

To state this differently, since you are far better prepared than I to testify regarding the Yahooskins, I must omit them and leave them to you. This

¹⁰⁰ Ibid.

¹⁰¹ For example, Steward writes,

Other costs besides Service's fee will depend somewhat upon what kind of job you want done...

I suggest you write to Service here at his home, [in] Ann Arbor., giving him the answers to my questions and telling him how to write up his proposal. There are no doubt some points I have overlooked that you will need to explain.

Let me again assure you that Service will make an ideal witness. He comes ready-made with the point of view you want, and he will make an excellent impression on the Commissioners. I think you will find many of the qualities you like in Manners, but a somewhat calmer approach to things.

Best Regards, Julian H, Steward
(December 10, 1955. Steward to Barney, JHSP, Box 1, my deletion of the address of Elman Service).

would mean that you prepare a statement about them, and that the statement should be slanted with reference to the Northern Paiute question rather than the Lamath-Modoc. Since your present ms., as I recall, is concerned with the latter affiliation of the Yahooskin, a new statement would be better. Consequently, Lee and I thought that maybe you could doctor up what I have already written more readily than revise your own statement... I am sorry this seems complicated, but the case seems to have become so and maybe you can help simplify our procedure.¹⁰²

After receiving a copy of Steward's instructions for writing the reports, Wheeler-Voegelin informs both Steward and Yost that the most crucial time period for the Great Basin area was 1855-1870, because the "early white period" altered aboriginal society profoundly. Consistent with Murphy and Steward, she says that there was no aboriginal organisation amongst the groups until predatory bands emerged that "plagued immigrants, miners, and wagon trains, etc."¹⁰³

Triangulating the conversation, Steward responds to this letter and informs Yost that he and Wheeler-Vogelin have devised a plan, and a general outline to present the ethnographic material for the cases. Indicating both power and authority in stating his plans, he advises Yost of a specific organisational scheme for the case. He indicates that he and Wheeler-Vogelin had had a long discussion on how to organise and present the material, and notes that he is sending her his "general introduction and conclusions,

¹⁰² JHSP, Box 2, Steward to Yost, cc'd to Wheeler-Voegelin, February 12, 1954.

¹⁰³ JHSP, Box 2, Wheeler-Voegelin to Steward, February 15, 1953.

together with a copy of that chapter for my book which might make a better Introduction than the long-winded material I have written for the case.”¹⁰⁴ He continues, stating that the “general outline as I see it now would be as follows:

Introductory material, No. 17

- A. Overall interpretation of the problem.
- B. Theoretical discussion of the concepts of tribe, band, and nation
- C. The nature of the Northern Paiute groups as determined by the environment, subsistence, social units in relation to subsistence patterns, food named groups, and general features.

Detailed analysis of Malheur Region, No. 17

Other Northern Paiute, no. 87

- A. West central Oregon: Walpapi, Yahooskin, Surprise Valley
- B. Nevada-Eastern California

General Conclusions¹⁰⁵

Steward closes the letter by discussing the difficulty of preparing exhibits, because of citation problems. In some instances, he says, the reference is a whole book that is out of print, like his “Sociopolitical Groups,” or in others, he, as the anthropologist, is the

¹⁰⁴ JHSP, Box 2, Steward to Yost, March 11, 1953.

¹⁰⁵ Ibid.

evidence, which is beyond any mere exhibit. To illustrate his explanation of his own role and expertise, he says,

In addition, anything I have to say about the Indians cannot be a conclusion based solely upon the information contained in a series of exhibits. It is a total point of view, based on innumerable unpublished experiences and conversations and long forgotten information contained in published material about these and other primitive people.

It would seem to me, therefore, that the general, interpretation sections – the Introduction and Conclusions – represent the anthropologist as a source of information and not only should not require detailed documentation but cannot ever be fully supported by evidence that can be produced for others to examine.

Actually, of course, we have virtually no primary evidence as in the case of biological or physical problems, wherein objects can be produced. Even information from travelers and Indians themselves is second-hand, colored by the individual's outlook. It is like the testimony of persons who witnessed an accident. I should think, therefore, that the competence of the expert witness is a good deal more important than the alleged facts.¹⁰⁶

¹⁰⁶ Ibid.

The last letter in this set of correspondence between the three strategists before the trial, Wheeler-Vogelin responds to Steward and Yost together. She recommends against dismantling the original report as Steward suggested, and keeping the conclusions as they were, maintaining that they “seemed” to be “very real conclusions.” Fundamentally, she agrees with Steward’s three time periods of Paiute history, and that the processes of acculturation of the second phase destroyed the aboriginal form.¹⁰⁷ In fact, she writes to Steward, that

Your statement toward the end that the reason the N. Paiute adapted to white culture so easily was because they had such an amorphous, impoverished, structureless one of their own, was a conclusion I had reached some time ago, based in good part upon the historical reading, and to some extent, also, on my experience with the Tubatulabal.

iii. The Levels of Sociocultural Integration

In a detailed letter to Williams, dated July 10, 1953, Steward describes the specifics of his testimony, outlining his interpretation of the aboriginal social organisation of the Paiute and his strategy for the cases. Consistent with the Ute cases, he says that the aboriginal cultural “forms” of the Paiute have broken down through contact, and that what then-presently existed was a result of the influence of “Whites.”¹⁰⁸ Though he admits that some areas of the Northern Paiute’s territory had an abundance of food and a “band”

¹⁰⁷ March 17, 1954.

¹⁰⁸ JHSP, Box 2, Steward to Williams, July 10, 1953.

could have emerged there, he contends, aboriginally, scarce resources determined that no chiefs, authority, or social organisation beyond the biological family could develop in the area. In fact, reproducing a line from Spencer's understanding of authority in Indigenous communities (Spencer 1969 [1876]: 159, 185), Steward reports that the Paiute had a changing leadership of shamans, but no consistent form of authority or office of leadership. He asserts that the families are "free," having no residence patterns beyond those dictated by the location of foodstuffs and water. He asserts that socially unencumbered family movement indicates that there was no chief, and as a result no bands. He distances himself from the report of the Paiute's expert, Omer Stewart, commenting that Stewart's method to establish territories through the use of place names is shoddy because he did not assume that scarcity drove social organisation.¹⁰⁹ He asserts, as in the Ute case, that there is nothing to distinguish the Paiute from the Shoshone, and that just because they refer to themselves as Numa (the People), it does not mean that the Paiute are a collective.

¹⁰⁹ As this set of correspondence indicates the level of familiarity those working for the Department of Justice shared, the breadth of their contact, and their motivation; for example, a personal exchange between Steward and Ralph Beals is shown here:

Julian,
 we think Omer [Stewart] is going to be the opposing witness. If you could sit in as you did in the Paiute cases it might show him up. Let me know.
 [Signed,] Ralph (Handwritten note, May 16, 1957, JHS Box 1).

Dear Ralph,
 Your scheduled Shoshone hearings for August 26th come just when I shall be leaving the country in connection with my Ford project. While it might give me a certain pleasure to try to embarrass Omer I doubt whether I could bear to sit through another of his performances. I am sure that Bob Murphy can do a good job for you.
 Sincerely, Julian H. Steward.

Following this base description, the Department of Justice framed their legal defence to deny Indian Title in the Paiute cases on Steward's ethnographic account and his theory of the "levels of sociocultural integration" as a way to argue that the Paiute had no cohesion, leadership, or common identity based on "science" (1954, 1974, and reproduced as 1955: 101-21). The US Department of Justice relied entirely on reports from Steward and Wheeler-Voegelin on the Shoshone to assert that "the government was not liable for any claims because the petitioners did not hold original Indian title" (Stewart 1959: 51, Ronaasen et al 1999). In fact, based on Steward's reports, the Department of Justice argued that the traditional lands of the Paiute were in a jurisdictional vacuum, alleging that the Paiute were "inherently incapable of acquiring and/or holding 'original Indian Title'" because they were not a recognised group based on the neo-evolutionary theory of the levels of sociocultural integration" (Defendant's Requested Findings of Fact, Northern Paiute Nation, quoted in Ronaasen 1993: 52; C.f. Ronaasen et al 1999; e.g. Steward 1955: 102-3).

In his testimony, Steward states his opinion that the Shoshone were living in relative isolation and explains how this description fits with his more developed notion of the levels of sociocultural integration. To this point, he says,

Owing to the natural environment of the Great Basin area and to the simple hunting and gathering techniques for exploiting it, it was inevitable that the individual family or at the most two or three related families should live in isolation during most of the year. "Family" in this case

signifies the nuclear, biological, or bilateral family, consisting of mother, father, and children. Unlike many primitive peoples, the Shoshoneans were not organized in extended family or lineage groups ... (1955: 102).

Steward claimed that the Paiute, a "Shoshonean" People, were at the family level of sociocultural integration, alleging that pre-contact Shoshone families lived in isolation with no formal ties between groups of families, and few informal ones (1955: 111-6). He likened the family to a net without any social knots that could establish connections between groups; a model that, he says, ultimately permitted "liberty" for each individual biological unit (*ibid*: 117). Explicitly, he describes "[t]he typical Shoshonean family" as "independent and self-sufficient during the greater part of the year, perhaps during 80 or 90 percent of the time," and alleges that "the family subsisted and fulfilled most cultural functions with little assistance from other families, and that it probably could have survived in complete isolation" (*Ibid*: 108). Moreover, he claims that family-level groups were rather rare in the pre-European contact period of the western hemisphere, and he suggests that "this level" is represented "in South America by the Nambicuara, Guató, Mura and perhaps other groups," and in North America by only two: "the Eskimo" and the "Shoshonean peoples" (*Ibid*: 119). In fact, he says, "[p]erhaps there have been people similar to the Shoshoneans in other parts of the world; for the present, however, the Shoshoneans must be regarded as typologically unique" (*Ibid*: 120).

Thus, in his testimony before the ICC, Steward claimed that the Shoshone represented the lowest level of his evolutionary taxonomy, and that they were "typologically unique."

He described them as “gastric,” motivated solely by their want of food, as atomistic biological groups, and finally used them to create a baseline reference point of social aggregates for his evolutionary taxonomy. Occupying the lowest level of human social evolution for all people, for all time, through this description the Shoshone became a metaphor for the bottom of the evolutionary typology, and necessary for the pronouncements of his entire ecological-evolutionary project. The magnitude of this imagery led Myers (2004) to conclude that the Shoshone have become a sort of cultural “barometer” used to reference social evolution within the discipline, and Ingold (2000) to identify Steward’s work as the “*locus classicus*” within the discipline of anthropology for the comprehension of the social organisation of Indigenous peoples.

Steward’s political location is exposed when his ICC testimony is contrasted with his original statements about Shoshone political organisation. In “The Economic and Social Basis of Primitive Bands” (1936), Steward states that all bands are “politically autonomous,” “communally landowning,” and have rules for “land inheritance,” and concludes that all people live in this state of social organisation, at a minimum.

Axiomatically he states,

All peoples in an area of low population density have some form of politically autonomous, landowning band, which is greater than the bilateral family. The size of the band and the extent of the territory it utilizes are determined by the number of persons who, due largely to ecological factors, habitually cooperate at least during part of the annual round of economic

and social activity. Band unity is expressed in a consciousness of common interest and submission to some degree of central control during community enterprises, although such control may be lacking during parts of the year.

(1936: 343)

In this early paper, written directly after his fieldwork but before joining the federal government, Steward specifies that the Owens Valley Paiute, the Southern California Shoshone, and "other Paiute" are either composite or patrilineal bands and are therefore, *de facto*, politically autonomous, land owning, and are a recognisable group with a degree of central control and common interests (*Ibid.*: 338). Therein, not only did he say that all peoples live in an organized, rule-based society, he notes that,

although the family is often the seasonal independent subsistence unit, additional social and economic factors require the unity and territorial autonomy of an aggregate of several such families, that is, the band. The most important factors which produce the band are: (1) Among the apes and most other mammals, the "social" aggregate is usually greater than the biological family. Therefore, primates provide no reason to suppose that human beings ever were divided into family groups. (2) In practically all human groups several families cooperate in some economic activity and frequently share game and even vegetables foods communally. This provides a kind of subsistence insurance or greater security than individual families could achieve... (1936: 332).

From 1949 until at least mid-1955 Steward testified, wrote reports, advised the government and recruited others with regard to several ICC cases.¹¹⁰ According to his archives, he worked on at least Docket Nos. 44, 45, 87, 88, 100, 17. And, according to the ICC Index of Cases, Steward is listed as having prepared reports and testimony for the Uintah Ute cases, the Northern Paiute cases, and the combined case, The Indians of California (Decisions of the Indian Claims Commission 1973).

It is important to note that though the ICC rejected Steward's testimony and that he "lost" every case where he was an expert witness (Stewart 1985; Lewis 2002) that, notwithstanding his futility, Steward's description of the Shoshone remains steadfast in anthropology (C.f. Johnson and Earl 1988, Haenn and Wilk 2006, Farb 1968, Myers 2004, etc.), and his approach is represented in the discipline as a scientific and objective

¹¹⁰ I do not have confirmation of the termination of Steward's relationship with the Department of Justice. The last documented letter in the archives stating his employment with them is dated August 26, 1955. It is an extremely cordial and frivolous letter from Ralph Barney to Steward. There is correspondence to indicate that Steward was kept apprised of the dealings of the Department of Justice through to 1957, when the rulings were handed down in the Ute cases. And, although not working on the case due to his time in Japan with his Ford Project, the Department of Justice continued to keep him apprised of the workings of the case, as their response indicates:

Dear Dr. Steward,
 There is attached a copy of the petitioners' opening brief in The Northern Paiute Nation v. United States, case No. 87 before the Indian Claims Commission. This brief will undoubtedly be of interest to you, and such comments as you may care to make will be appreciated.
 Sincerely Perry W. Morton – Assistant Attorney General Lands Division to Ralph A. Barney (June 24, 1957, JHSP, Box 2).

method for the comprehension of “band-level” societies, as described by numerous representative authors above.¹¹¹

iv. DISCIPLINARY RESPONSE

After the Paiute trial, in May of 1954, the discipline’s awareness of the role of anthropologists before the ICC became more acute. This heightened awareness can be understood, in part, as a result of Sol Tax organising a symposium at the 1954 AAA meetings to flesh out many of the issues surrounding the use of anthropology at the ICC. Secondly, preparation for *The Indians of California vs. the United States* trial was increasing, a case that drew Alfred Kroeber into the role as expert witness against Steward, and raised the profile of the role of the expert and the place of anthropology in its proceedings to the rest of the discipline.

Tax’s interest in the roles of anthropologists at the ICC was considered as an intrusion by those anthropologists working for the government. Letters of concern circulated between Erminie Wheeler Voegelin, Mildred Mott Wedel, Waldo Wedel, and Steward in the spring of 1955. By June, Steward had concluded that “there is evidently developing a rather dangerous group including Nancy Lurie, Verne Ray, and others which is doing considerable violence to facts and interpretations which is putting anthropology in a very

¹¹¹ Moreover, it is a perception that continues to have implications in Canada as it helps to structure the test for the determination of Aboriginal rights in Canadian law (Asch 1984, 1992, 1998, 2000, 2002; Culhane 1992, 1997; Cruickshank 1992; Dailey 2005; Pinkoski and Asch 2004).

dangerous spot.”¹¹² Mott Wedel and Wheeler-Voegelin reinforce Steward’s claim in their own accounts of testifying, and also add Omer C. Stewart to their list of adversaries.¹¹³

Much of the debate surrounding anthropological expert testimony during the ICC trials has been keenly presented in the initial issues of the journal, *Ethnohistory*. The journal, started by Wheeler-Voegelin and Jay Jones while they were at Indiana late in 1954,¹¹⁴ had much of the early focus on the role of anthropologists at the ICC as can be elicited from the published correspondence in the first few issues as well as letters in Steward’s archives.¹¹⁵ The initial issues of *Ethnohistory* are dominated by Wheeler-Voegelin’s papers, prompting her to beg others not to judge the journal based solely on her work, as she did not have enough copy material to fill the issues.¹¹⁶ Quickly though, much of the focus of the first issues surrounded anthropological testimony and involvement at the ICC. For instance, prompted from Tax’s organisation of the Detroit Symposium on the ICC in December of 1954, the journal published a balanced account of the papers presented at the conference (Ray 1955, Vol. 2, No. 4). Containing six essays and an introduction, the special issue was a planned polemic with Steward, Ralph Barney, and Jay Jones representing the position of the government, and Alfred Kroeber, Nancy Lurie, and Donald Gormley.

¹¹² JHSP, Box 2, Steward to Wheeler-Voegelin, June 2, 1955.

¹¹³ JHSP, Box 2, Wheeler-Voegelin to Mildred Mott-Wedell, May 3, 1955.

¹¹⁴ Ibid.

¹¹⁵ Cf. Manners 1955; Lurie 1956, 1984, Kroeber 1955; Wheeler-Voegelin ; Steward 1955, 1968.

¹¹⁶ JHSP, Box 2, Wheeler-Voegelin to Mildred Mott-Wedell, May 3, 1955

In his foundational article on anthropology and expert witness testimony, Rosen (1977) acknowledges that

the bulk of anthropological testimony has come in the proceedings before the Indian Claims Commission... [which] was authorized to hear Indian claims against the governments, including those based on treaties or on the harm done by less than 'fair and honourable dealings' with the Indians. Under the statute, only a 'tribe, band, or identifiable group' was permitted to file suit. Moreover, in most cases the plaintiffs have been unable to show that a group, as opposed to a collection of individuals, is involved, without demonstrating that the group exclusively occupied a definite territory for a long period of time. From the inception of the Commission, the testimony of anthropologists has been central to these determinations (1977: 566).

Rosen argues that the anthropologists testifying before the ICC "marked the real beginning of anthropological expert testimony in this country" (567) as numerous anthropologists testified to the nature of aboriginal land title, social groupings, and the comprehension of treaties (556). In his account, he focuses on Steward's contributions to the debate that occurred within the AAA and documents the schism that exists between government anthropologists and those testifying on behalf of the Indians – a division most acutely understood as between applied and action anthropologists (Trencher 2002).

Subsequent analyses on the roles of anthropologists have steadfastly bifurcated along these divisions identified by Rosen (i.e. Wishart 1985; Stewart 1985; and Beals 1985). Those anthropologists who worked for the government have argued necessity, utility and liberal accommodation/assimilation as motivators for their involvement (Beals 1985; Stewart 1969); and those anthropologists who Stewart alleged comprised a "dangerous" cabal, typically have attempted to frame their work in terms of Indigenous-US government relations (Stewart 1985) and an empirical reality, based on the inherent humanity of the Indigenous peoples in question (Lurie 1970, 1985).

Chapter Seven. Steward's Theory of Culture Change

In this chapter, I discuss the culmination of Steward's theory and method, and introduce the application of his science as defined in his theory opus, *Theory of Culture Change: The Methodology of Multilinear Evolution* (1955). Steward's theoretical project is presented most fully in this book, a text that continues to be among the most influential in the discipline today. Ethnographically, it represents the American Great Basin, Puerto Rico, and Carrier Country in central British Columbia, Canada. Analytically, it presents the "substantive application" of his "concept and method," generating what he alleges is a non-theoretical hierarchical taxonomy of all societies through all time. Combined, the essays in *Theory of Culture Change* form Steward's explicit statement of his theory, method, and the practice of his anthropology; it is his science writ large. Moreover, three chapters were used by the US government's Department of Justice in ICC proceedings without this contextual information acknowledged in it (Clemmer 1999; Ronaasen et al 1999; Pinkoski and Asch 2004). Thus, *Theory of Culture Change* is Steward's praxis and it is set up in opposition to Boas and articulates what I am asserting is a science of colonialism.¹¹⁷

i. *Theory of Culture Change*

Theory of Culture Change is the culmination of Steward's preceding articles, with nine of its 12 chapters having been previously published, either explicitly or substantively. In fact, the text is the re-statement of his published work in reverse chronological order,

¹¹⁷ This is true of Steward's Puerto Rico work too (e.g. Steward 1950, 1951, 1955; C.f. Lauria 1990; Duncan 1980). Silverman (2005), for example, records this history, as the golden age of anthropological research.

with the addition of his most oft-reproduced essay, “The Concept and Method of Culture Ecology,” as Chapter 2. Republishing his essays in this format gave Steward the chance to reformulate his ideas and re-cast them within his newly stated method, “multilinear evolution;”¹¹⁸ a concept that he states “constitutes the methodological position of the present collection of essays...” (*ibid*: 4). This format also allowed Steward the opportunity to demonstrate how his theory and method were “elaborated” and “substantively applied” in his new operational concept, “the levels of sociocultural integration,” as detailed in “The Levels of Sociocultural Integration” and “The Great Basin Shoshean Indians: An Example of a Family Level of Sociocultural Integration,” Chapters 3 and 6 respectively (*ibid*: 5, 43-63, 101-121).

In his “Introduction” to the book, Steward defines the three components of his model that are necessary for his method of anthropological research. First, in establishing the definition of “multilinear evolution,” he attempts to distinguish his theory from the 19th century unilinear evolutionists, specifically, distancing his theory from that of Morgan, Tylor, Childe, and White. He admits to using the term “evolution” reluctantly but admits that he “can find no better term” (1955: 5). He charges that unilinear evolutionists adhere to a belief in “developmental stages” and promote that “contemporary primitive peoples exhibit a kind of arrested development” in their analyses (*ibid*: 4). In contrast to their formulations, he suggests that the cultural relativists see social development as “divergent,” and focus on unique and particular features of cultures (*ibid*: 4). Noting that relativists recognise similarities and relationships due to diffusion, and once again finding

¹¹⁸ Chapter 1, Multilinear Evolution had been previously published as “Evolution and Process” in Kroeber’s edited *Anthropology Today* (1953), as discussed above.

fault with this supposition, he claims that they consider each culture area to be distinct from all others, and this assumption offers no basis for the comparison of cultures (*ibid*: 4). He states that the method brought forward in his text is different, and does not follow the approach of the diffusionists or the unilinear evolutionists. Rather, he claims that multilinear evolution “assumes that certain basic types of culture may develop in similar ways under similar conditions but that few concrete aspects of culture will appear among all groups of mankind in a regular sequence” (*ibid*. 4). Doing nothing more than asserting his own conclusions regarding evolutionism, he has proved nothing.

Secondly, he explains that the schema to discern how multilinear evolution unfolds is developed in the conceptual and methodological framework of “cultural ecology.” Introducing and contextualising his formative concept, he alleges that the subsistence technologies that are adapted to a given environment provide the most important cultural features, what he calls the “culture core,” and social organisation and the superstructure arise as epiphenomena from this core. He states, that given these initial vectors, he can determine the scientifically-identifiable, and ultimately integral characteristics of a given culture, and, as a result of his deduction, the *level* of sociocultural evolution of the given culture.

Thus, thirdly, he integrates the concept of the “levels of sociocultural integration” into his theoretical and methodological frame, explaining it as one of his most important concepts by acknowledging it as the “substantive application” of his entire project (1955: 5). He contextualises the “levels of social cultural integration” as the synthesis of his theory and

method, combined to apply a comparative evolutionary scale of societies through what he calls “a practical, non-theoretical evolutionary taxonomy of existing human societies” (*ibid*: 11). This application, he says, combines “multilinear evolution” and “cultural ecology” in an “operational concept” to demonstrate that the relevant characteristics of all cultures are those most closely linked with the physical environment, and the subsistence technologies used to exploit it. Here, he imagines that subsistence technologies that are adapted to an environment provide the most important cultural features, and the social organisation and superstructure arise as epiphenomena from this core (*ibid*: 37); and the levels of sociocultural integration are the (natural) hierarchical rankings of specific societies in multilinear evolution (*ibid*: 5, 101).¹¹⁹

¹¹⁹ Kern contextualises *Theory of Culture Change* through the concepts of multilinear evolution and cultural ecology (2003: 273-81). She contends that multilinear evolution was a hastily prepared theoretical statement that Steward offered to counter Kroeber. She motivates *Theory of Culture Change* through Steward’s anxiety over rejection, and his simultaneous desire for recognition as an important figure in the discipline, while continually trying to establish himself as unique from his doctoral supervisor. She explains the novelty of *Theory of Culture Change* in institutional and personal terms without analysing its methodological statements, or its place within the history of the discipline. Moreover, she does not explore its “substantive application.”

For instance, Kerns notes that Steward’s approach had been anti-Kroeberian in focus up to this point in his career; but in specific terms it was becoming anti-Boasian. That is, long before *Theory of Culture Change*, while still at the BAE and writing of his recent Carrier field-experience, Steward published an article in *Scientific Monthly*, posing the question: Is there “Determinism in Primitive Society?” (1941). Acknowledging the schism between professional and hobby anthropology that had plagued the discipline in the US, he replicates the Boas and McGee debate that had occurred four decades previously. Wading into the debate about the method and theory of anthropological research, and by implication the organisation of the AAA, he says,

There has recently been renewed interest in the general proposition, stated in many ways with varying degrees of moderation, that technological and economic changes largely predetermine social and political trends. Politicians, businessmen, and laymen argue the power of some form of “economic determinism” as against ideologies. What has anthropology, centering its attention largely on the simpler peoples of the world where it should be easier to isolate the causes of social change, to say of this proposition?

In answering this telling question he continues, explaining that “subsistence patterns/social organization” are “repeated so consistently under identical economic conditions and environmental conditions that a cause and effect relationship between the latter and the former is unmistakable” (1977[1941]: 180-1). Thus, in answering his own question regarding the efficacy of economic determinism as an explanatory model in anthropological theory, he aligns himself with the interests of “politicians, businessmen, and

ii. STEWARD v. BOAS: Method and Theory

To open *Theory of Culture Change*, Steward begins by offering the following statement:

In cultural studies it is important to distinguish a scientific, generalizing approach from a historical, particularizing approach. The former attempts to arrange phenomena in orderly categories, to recognize consistent interrelationships between them, to establish laws of regularities, and to make formulations which have predictive value. The latter is more concerned with the occurrence of phenomena in time and place, the uniqueness of each constellation, and the ethos or value systems which characterize culture areas. The concepts and methods of the former must differ in part from those of the latter. My purpose in this collection of essays is to develop a methodology for determining regularities of form, function, and process which recur cross-culturally among societies found in different culture areas (Steward 1955: 3).

To an anthropologist, Steward's statement clearly refers to "Boasian" anthropology, and immediately establishes the orientation of his text as counter to a Boasian-approach.

However, the precise intention of Steward's statement is made clearer when contrasted with Boas' explicitly stated method. In "Franz Boas and the Humboldtian Tradition," published recently in the *History of Anthropology* series, *Volksgeist as Method and Ethic: Essays on Boasian Ethnography and the German Anthropological Tradition* (Stocking,

laymen;" and the precise position advocated by his fellow ASW member, McGee as proposed against the orthodox, "ideological" Boasian stance of professional or scientific anthropology (Stocking 1968). This point would lead to Steward's attempt to reorganise the AAA in the 1940s (Trencher 2002)

ed. 1996), Bunzl demonstrates that Boas' consistent methodological statement was offered

In "The Study of Geography," [where] Boas contrasted two scientific methodologies: the physical and the historical. For the former "the aim of science [was] to deduce laws from phenomena," and the "single phenomenon itself" was insignificant, but merely served as "an exemplification of a law," as a means "to find new laws or to corroborate old ones." In contrast, the historical method had as its goal "the investigation of phenomena themselves," and was "unwilling to consider them the as a subject to stringent laws." The two methods had their origin in "two different desires of the human mind." Arising from "its aesthetic wants," the physical method sought to arrange the myriad of phenomena of the world "systematically," so as to "put the confused impressions in order." The historical method, in contrast, grew out of an "effective" impulse; "the mere occurrence of an event" triggered the desire to study its "true history." (Bunzl 1996: 17, quoting Boas (1887) throughout).

When Bunzl's account of Boas' method is juxtaposed with Steward's stated project in *Theory of Culture Change* it places the two in direct opposition.

Emphasising this point in later work, Bunzl (2004) further explains Boas' method, saying that:

Most readings of “The Study of Geography” have concentrated on the epistemological contrast between the aesthetic impulse, the desire to deduce laws from phenomena, and the affective impulse to investigate phenomena for their sake. But implicit in this crucial distinction was also an ethnographic research program that derived from such German counter-Enlightenment figures as Johann Gottfried Herder and Wilhelm von Humboldt. In the late 18th and early 19th centuries, they had developed a *Humanitätsideak* (ideal of humanity) in opposition to such French Enlightenment figures as Voltaire. In contrast to the conception of a uniform development of civilization, they had argued for the uniqueness of values transmitted throughout history. The comparison of any given nation or age with the Enlightenment or any other external standard was unacceptable. Each human group could be understood only as a product of its particular history, propelled by a unique *Volksgeist* (genius of a people)... For Boas, the reason to explore cultural phenomena was not that they were “Other” but that they were “there.” This seems like a trivial distinction, but it has enormous epistemological ramifications... rather than focus on their inherent Otherness (in an act of reification), he sought to understand them as the products of particular historical developments. Ultimately, it was not their difference that made them interesting, but the fact that they contributed to the plenitude of humanity (Bunzl 2004: 437-8).

Given the explicit opposition of Steward's theory and method as identified in *Theory of Culture Change* to Boas' stated method for the discipline, the following discussion will compare Steward's and Boas' theory and method. To make this comparison, I draw on three themes identified by Bunzl in the quotation above.¹²⁰

A. Aesthetic vs. Affective Impulse.

In his analysis of Boas' method, Bunzl notes that Boas' supported a desire to understand phenomena "for their own sake," and shunned the aesthetic impulse of deducing general laws. As noted above, Steward, too, acknowledged this methodological dichotomy between the aesthetic and affective impulses, but chose to foreground the aesthetic and shun the affective. This is stated explicitly where he notes, "[m]y purpose in this collection of essays is to develop a methodology for determining regularities of form, function, and process which recur cross-culturally among societies found in different culture areas" (Steward 1955: 3); or, where he states, "[c]ultural evolution, then, may be defined broadly as the quest for cultural regularities or laws..." (*ibid*: 14).

Chapter 1 of *Theory of Culture Change*, "Multilinear Evolution: Evolution and Process," lays out Steward's methodological project to establish his distinct theoretical position. As in the original essay (Steward 1953), Steward attacks Kroeber, as well as the approach of Leslie White stating that "multilinear evolution" is "essentially a methodology based on the assumption that significant regularities in cultural change occur, and it is

¹²⁰ See Verdon (2006a, 2006b) for a critique of the neo-Boasian historicisation of Boas exemplified by Lewis, Darnell, Bunzl, and Handler.

concerned with the determination of cultural laws" (1955: 18-19). He argues his is a method about gaining "concreteness and specificity" for comparison and for understanding culture change. Its concern is the generation of taxonomic features, conceptions of historic change, and cultural causality (1955: 11). Attempting to distinguish his project from the relativism of Boas, Steward explains in terms reminiscent of Spencer,

Just as simple unicellular forms of life are succeeded by multicellular and internally specialized forms which distinctive kinds of total organization, so social forms consisting of single families and lineages are succeeded by multifamilial communities, bands, tribes, and these, in turn, by state patterns, each involving not only greater internal heterogeneity and specialization but wholly new kinds of over-all integration. Thus evolutionism is distinguished from relativism by the fact that the former attributes qualitative distinctiveness to successive stages, regardless of the particular tradition, whereas the latter attributes it to the particular tradition or culture area rather than to the developmental stage (Steward 1955: 13).

Continuing, Steward explains,

We must conclude that cultural evolution is not distinguished from cultural relativism or historical particularism by any essential similarity of its developmental scheme with that of biological evolution, by the

characteristic of increasing complexity, or by the attribute of progress... The methodology of evolution contains two vitally important assumptions. First, it postulates that genuine parallels of form and function develop in historically independent sequences or cultural traditions. Second, it explains these parallels by the independent operation of identical causality in each case. The methodology is therefore avowedly scientific and generalizing rather than historical and particularizing. It is less concerned with the unique and divergent (or convergent) patterns and features of culture – although it does not necessarily deny such divergence – than with parallels and similarities which occur cross-culturally. It endeavors to determine recurrent patterns and processes and to formulate the interrelationships between phenomena in terms of “laws.” The nineteenth-century evolutionists are important to contemporary studies more because of their scientific objective and preoccupation with laws than because of their particular substantive historical reconstructions (Steward 1955: 14).

This essay, first published in Kroeber’s (1953) *Anthropology Today* as “Evolution and Process,” is the stated foundation for his entire theoretical project, resting on the notion that societies exist on a true evolutionary continuum that can be discerned and ranked through scientific means. Though he claims that his method is not deductive, stating that “[m]ultilinear evolution, therefore, has no a priori scheme or laws” (*ibid*: 19), he offers a comparative, explicitly deductive “scientific” description of his theoretical project, and

offers it in direct opposition to Boas' stated method, and naively assumes that his science is not hampered somehow by *a priories* or laws.¹²¹

B. Comparisons (with the age of Enlightenment) and Methods.

Boas adhered to the German counter-Enlightenment method that did not permit comparisons with Enlightenment standards or comparisons between peoples based upon Enlightenment ideals (Bunzel 2004: 437). Bunzl notes that this approach led to a distinctive methodology that searched for particular histories which were propelled by a presumed *Volksgeist* (Bunzl 2004). In response to Boas' stated method, Steward offers "The Concept and Method of Cultural Ecology," Chapter 2 of *Theory of Culture Change* to promote "science" and rational action for anthropological research. Here, Steward adduces a method for discerning worldwide social development, and a system for ranking the discrete lines of social groupings in his theory of multilinear evolution. He asserts that the method of "cultural ecology" permits the recognition "of the ways in which cultural change is induced by adaptation to environment" (Steward 1955: 5); and argues that the determinable and relevant characteristics of cultures are those which are most closely linked with the physical environment and the subsistence technologies used to

¹²¹ It is of note that Steward ends his introductory essay to the book by distinguishing his project from that of his contemporary, Leslie White. He states,

It is difficult to conceive the kinds of understandings that Leslie White, an uncompromising culturologist, hopes to gain by dealing with culture in general rather than with cultures in particular. Yet White, like so many social scientists, seems to believe that a truly scientific formulation must explain all modes of behavior. I conclude this Introduction therefore by emphasizing that my own objective is to formulate the conditions determining phenomena of limited occurrence. The category of nature to which these belong is known generically as culture and is found among all mankind, but no cultural phenomena are universal (1955: 8).

exploit it. These elements make up what he calls the “culture core,” a concept that is the basis of comparison and the general description of evolutionary theory.

Schematically, the techno-environment becomes the base upon which the culture core rests. The subsistence technologies that are adapted to the environment provide the most important cultural features, and the social organization and superstructure arise as epiphenomena, or determinants of this relationship between the environment and the subsistence technologies. In this manner, the techno-environment is the base that allows for the cultural expression and the advent of new technologies; thus, ecological adaptations are the driving force for cultural causations – and in the manner that Steward describes, human evolution (1955: 11). Following from his essay “Cultural Causality and Law” (1949), Steward alleges he has accomplished the ability to discern, through an objective method, scientifically identifiable characteristics of the culture core that explain the relationship between environment and culture (Steward 1955: 163); and cultural causation, or laws, are a function of the technological and ecological adaptations to a given environment and recur cross-culturally (Steward 1936; 1940; 1952; 1955).

He states that his method of cultural ecology has three steps: The first is the analysis of the methods of subsistence production in relation to the environment. He states that the relevant environmental features “depend upon the culture,” and that the “simpler cultures are more directly conditioned by the environment than advanced ones” As an example, for the Shoshone of the Great Basin, he states that their material technology is minimal and only includes basics such as “weapons and instruments for hunting and fishing;

containers for gathering and storing food; transportational devices used on land and water; sources of water and fuel; and, in some environments, means of counteracting excessive cold (clothing and housing) or heat” (*ibid.*: 40).

The second step is to discern the pattern of human behaviour that is part of these methods of production. He says this process involves the analysis of “the behavior patterns involved in the exploitation of a particular area by means of a particular technology...” (*ibid.*: 40). He suggests that the techno-environment of a hunter-gatherer society is at least as important as cultural history in determining the nature of subsistence exploitation by referring to the organisation of labour in food production. From this point, he argues that gathering, which is usually done by women, is accomplished working alone or in small groups, because “[n]othing is gained by co-operation and in fact women come into competition with one another (*ibid.*: 40). However, men may hunt either co-operatively or individually because “[d]eer cannot be hunted advantageously by surrounds, whereas antelope and bison may best be hunted in this way” (*ibid.*: 41).

Steward says that steps one and two give scientists the ability to understand the relationship of production techniques to the other elements of the culture (Steward 1955: 41). Thus, he says the third step is “to ascertain the extent to which the behavior patterns entailed in exploiting the environment affect other aspects of culture” (*ibid.*: 41). As he says, “[a]lthough technology and environment prescribe that certain things must be done in certain ways if they are to be done at all, the extent to which these activities are functionally tied to other aspects of culture is a purely empirical problem” (*Ibid* 41).

Thus, “the occurrence of patrilineal bands among certain hunting peoples and of fragmented families among the Western Shoshone is closely determined by their subsistence activities, whereas the Carrier Indians are known to have changed from a composite hunting band to a society based upon moieties and inherited statuses without any change in the nature of subsistence” (*Ibid*: 41).

To “operate” his method, he states that “[c]ultural development therefore must be conceptualised not only as a matter of increasing complexity but also as one of the emergence of successive *levels of sociocultural integration*” (*ibid.*: 5, emphasis his). Thus, in Chapter 3, “The Levels of Sociocultural Integration,” Steward explicitly combines the framework of multilineal evolution and the methodology of “cultural ecology” to establish his schema for comparing and evaluating the level of a specific society. He shows how his concept of the “levels” of social forms categorises groups into a framework of simple to complex societies; and, although he claims that the evolutionary trajectory of each society is not unilinear, in this piece he hypothesises that the “family represents a level that is lower in a structural sense, and in some cases it appears to have been historically antecedent to higher forms” (1955: 53-4). He says, a determination of the sociocultural levels is representative of the “growth continuum” of increasingly complex and newly emergent forms, and, as discussed above, he patterns this schema specifically after the biological understanding of evolution (Steward 1955: 51; Murphy 1981). For him, the bottom level of the scale of sociocultural integration is known as the “family-level.” In this level the “family was the reproductive, economic, educational, political, and religious unit” (1955: 54), and is typological unique to

Indigenous peoples. In his schema, each level is marked by increasing complexity, as evidenced by inventories of cultural traits, increasing heterogeneity and formal political structures.

C. History and the Common Bond of Humanity.

Finally, Bunzl notes the importance of the notion of particular histories and the refusal to rely on a notion of the uniform development of human civilisation in Boas' method (2004: 437). Steward, too, seems to offer this distinction, and he is careful to stress that his theory of multilinear evolution does not presuppose that it is possible for all peoples to evolve through the same developmental stages. Instead, he argues that some cultures naturally die out or assimilate to a "higher" form (Steward 1950, 1951, 1955).

Clearly, Steward does not offer this distinction of culture history for the same reason as Boas. This is because, as Bunzl argues, Boas emphasised the manifestation of history through the celebration of *Volksgeist* (genius of a people) a commonality that all people share; in this manner, his focus on particular histories, presupposes an agency of the people in question. Beginning with his work very early on in his career, Steward does not concede to history as an explanation for cultural forms and nor does he appreciate the *Volksgeist* or even the agency of all peoples. Countless examples demonstrate this point. The Uintah Ute Bear Dance (1932), his BIA report (1936), Shoshoni Polyandry (1937), and in *Basin-Plateau* (1938) Steward claimed that Native American peoples were of a low culture originally and that contact with Euro-American people destroyed or greatly altered their original cultural form leaving no aboriginal elements in the face of contact.

His fieldwork method at this time took no effort to explore any continuation of “particularities” of the peoples in question post-contact, and in fact it was shown that he lied about the work he was doing and seriously overrepresented the amount of time he spent doing fieldwork preferring instead to encounter, experience and understand the spirit of the peoples with a presumption of failure and only second-hand.

iii. APPLICATION OF THEORY AND METHOD

These base-representations of the Shoshone from early in his career formed the basis for Steward’s theoretical later pronouncements; specifically noting in *Theory of Culture Change*, for example, that his ethnography, the publication where he famously referred to the Shoshone as “gastric” and driven entirely by the quest for food to stave off imminent starvation (1938) is condensed to form Chapter 6 of the text, newly entitled, “The Great Basin Shoshone Indians: An Example of the Family Level of Sociocultural Integration” (1955: 101fn1). In that chapter, Steward further develops his theoretical and methodological project, calling its development the “substantive application” of his methodology. There, Steward states his opinion that the Shoshone were living in relative isolation, and demonstrates how this description fits with his more developed notion of the levels of sociocultural integration. To this point, he says,

Owing to the natural environment of the Great Basin area and to the simple hunting and gathering techniques for exploiting it, it was inevitable that the individual family or at the most two or three related families should live in isolation during most of the year. “Family” in this case signifies the nuclear,

biological, or bilateral family, consisting of mother, father, and children.

Unlike many primitive peoples, the Shoshoneans were not organized in extended family or lineage groups ... (1955: 102).

Therein, Steward describes "The typical Shoshonean family ... [as] independent and self-sufficient during the greater part of the year, perhaps during 80 or 90 percent of the time. It subsisted and fulfilled most cultural functions with little assistance from other families. It probably could have survived in complete isolation" (1955: 108). Furthering this point, he says,

I classify the Shoshoneans as an exemplification of the family level of sociocultural integration because in the few forms of collective activity the same group of families did not co-operate with one another or accept the same leader on successive occasions (1955:109).

In Steward's view, the family level of sociocultural integration arises when a society is organised so that each family exists in virtual isolation from all others, where there are few forms of collective activity, and a lack of task specialisation. Among other characteristics the features include the absence of a permanent, on-going leadership so that each family remains independent and self-sufficient; and, "the absence of property claims of local groups to limitable areas of natural resources upon which work had not been expended..." (*ibid*: 108). Another crucial factor in determining whether a society is at the family level, at least under 'pristine' conditions, is the paucity of their trait or

element lists. Another is that ‘the food quest was of overwhelming importance, but, owing to the differences in environment and exploitive techniques, it entailed very unlike activities and associations between families’ (*ibid.*: 120).

Within *Theory of Culture Change* Steward theorises Shoshone social organisation as if from a neutral position, alleging that the bottom level of the scale of sociocultural integration is known as the “family-level.” At this level, he says, the “family was the reproductive, economic, educational, political, and religious unit” (1955: 54); and he explains that the family-level naturally yields to the middle, or folk, level, which in turn yields to the national level; or, put another way, “[n]o one doubts that hunting and gathering preceded farming and that the last two were preconditions of ‘civilization’...” (*ibid.*: 28). Though he claims that the evolutionary trajectory of each society is not unilinear, he hypothesises that the “family represents a level that is lower in a structural sense, and in some cases it appears to have been historically antecedent to higher forms” (*ibid.*: 53-4). Fully invoking a Spencerian biological analogy, he alleges the parallel between his understanding of social organisation to biological evolution mainstays such as growth, complexity, and death, stating that

[j]ust as simple unicellular forms of life are succeeded by multicellular and internally specialized forms which have distinctive kinds of total organization, so social forms consisting of single families and lineages are succeeded by multifamilial communities, bands, tribes, and these, in turn, by

state patterns, each involving not only greater internal heterogeneity and specialization but wholly new kinds of over-all integration (*ibid*: 13).

Though not well known, but of tremendous disciplinary consequence, Ronaasen *et al* state that Chapter 6 of Steward's *Theory of Culture Change*, "The Great Basin Shoshonean Indians: An Example of a Family Level of Sociocultural Integration," was also the Department of Justice's statement of defence, *in verbatim*, before the ICC, Dockets 87 & 88 (1999: 176-7; JHSP Boxes 2, 3). That is, in *Theory of Culture Change* where Steward explains that the "concept of the levels of sociocultural integration" is the "substantive application" of his theoretical and methodological project (1955: "Contents," 99, 101-21), he does not divulge within the book that this was his explicit testimony to deny Indian interests in lands for the US government in legal proceedings. Nor does the discipline acknowledge this involvement.

Thus, Steward's example of the operation of the levels of sociocultural integration, operated through his science explicitly, is also the US Department of Justice's statement of defense in ICC proceedings, *in verbatim*. Therefore, given the diametrical opposition between Boas' and Steward's stated methodologies for the discipline, it is necessary to consider Steward not in relationship to Kroeber (c.f. Kerns 2003), but in answer to Boas. It is my assertion that *Theory of Culture Change* is Steward's explicit statement of his scientific method, the hegemonic notion of an objective, scientific method in American anthropology, and the clearest opposition to Boas' method offered within the discipline.

It is also my assertion that Steward's work, as epitomised by *Theory of Culture Change* but not limited to it, is explicitly a science of colonialism.

Chapter Eight. **Conclusions**

The relationship between anthropological theory and colonialism in North America has been widely neglected in the historiography of the discipline.¹²² This omission occurs despite the increasing call for a greater disciplinary self-reflection on our work and on our relationships with those with whom we work. Over the past three and half decades, calls for disciplinary self-reflection have been best articulated by those scholars working explicitly on the critique of the relationship between anthropology and colonialism (Hymes 1969; Asad 1973; Goody 1995; Stocking 1990, 1995), and those working with an “interpretivist” method (Geertz 1973, Rabinow 1977, Marcus and Fischer 1986). The two approaches are not mutually exclusive by any means, and the interpretivists, as a rule, have been influenced by the critiques of colonialism offered by Said, Asad, Fanon, and Deloria.

i. Anthropology and Colonialism

Within anthropology, the critique of colonialism is represented most authoritatively in the work of Talal Asad, and most prominently in his edited collection, *Anthropology and the Colonial Encounter* (Asad 1973). Asad’s essentially hopeful message about the promise for anthropology to “transcend itself” is predicated on focusing a new anthropological method on

the historical power relationship between the West and the Third World and
to examine the ways in which it has been dialectically linked to the practical

¹²² I am accepting the OED’s definition of colonialism: “1. a policy of acquiring or maintaining colonies.”

conditions, the working assumptions and the intellectual product of all disciplines representing the European understanding of non-European humanity (Asad 1973: 18-9).

In his introductory essay, Asad explains that imperial forces permitted anthropological studies to take place since “[t]he colonial power structure made the object of anthropological study accessible and safe – because of its sustained physical proximity between the observing European and the living non-European became a practical possibility” (ibid: 17). In his analysis of British social anthropology, Asad articulates what would later become a focal point in the discipline regarding the embedded structural relationship between anthropology and colonialism by situating this relationship within the power of worldwide political economy. He explains that,

[t]he reason for this asymmetry is the dialectic of world power. Anthropologists can claim to have contributed to the cultural heritage of the societies they study by a sympathetic recording of indigenous forms of life that would have been left to posterity. But they have also contributed, sometimes indirectly, towards maintaining the structure of power represented by the colonial system (Asad 1973: 17).

Asad’s text is complemented by several other representative works on the topic: Kathleen Gough’s slightly earlier cry that anthropology was the “child of imperialism” in her “New Proposals for Anthropologists” (1968); Diane Lewis’s concurrent essay,

“Anthropology and Colonialism,” in *Current Anthropology* (1973); George Stocking’s edited, *History of Anthropology*, Volume 7, *Colonial Situations* (1990); and finally, Peter Pels’ *Annual Review of Anthropology* survey, “The Anthropology of Colonialism” (1997). In each of these complementary works, the author engages in critical analyses of the discipline to expose and hopefully de-couple anthropology’s relationship with colonial practices. Taken together these scholars demonstrate that the discipline of anthropology is deeply intertwined with politics of imperialism; and, they agree that the discipline needs to address this history fully, because the relationship is at least two-fold. They contend it has a structural history enmeshed with the foundations of Western Enlightenment thought and the basis of anthropological enquiry; and, this deep structure manifests itself in the individual theories, methods, and agency of the practitioners of the discipline.

For example, in her account, Gough personalises this relationship, reflecting that “[w]e tended to accept the imperialist framework as given, perhaps partly because we were influenced by the dominant ideas of the time, and partly because at the time there was little anyone could do to dismantle the empire” (1968: 404). Lewis, likewise, concludes that “[a]nthropology emerged from the colonial expansion of Europe,” and continues, stating explicitly that “[c]olonialism structured the relationship between anthropologists and the people they studied and had an effect on methodological and conceptual formulations in the discipline” (1973: 591).

ii. A New Disciplinary Method

To overcome the unequal social relations embedded in the methods of anthropology, these authors advocate for greater self-reflexive techniques within the discipline's methodology. In turn, they focus on the necessity for generating a method of disciplinary self-reflection; and, consistently, they offer this need for a new method as a means to acknowledge and level power imbalances between ethnographers and subjects so as to improve the basic anthropological project. Pels (1997) explains that this was because

from the point of view of anthropology the study of colonialism presents a unique view and commands a peculiar sense of engagement. For anthropologists, more than for any other type of scholar, colonialism is not an historical object that remains external to the observer. The discipline descends from, and is still struggling with, techniques of observation and control that emerged from the colonial dialectic of Western governmentality (Pels 1997: 164).

Importantly, although Lewis found the discipline in "crisis" and asserted that the old "anthropology has contributed to the gulf between Western and non-Western culture by providing information which supports the mental constructs developed by those in power" (1973: 584), encouragingly she viewed the situation as improving. She concluded this was because anthropologists were becoming more aware and critical of their political location with respect to colonialism in the recent past. In fact, Lewis found solace in the emerging anthropological method when she observed that the "critical self-examination among anthropologists has appeared concomitantly with the growing self-

awareness of non-white people” (ibid. 581).¹²³ Pels’ (1997) recent review-article confirms Lewis’ positive sentiment of this effect on the discipline, when he records that “[o]nly in the last 25 years, however, have such critique and reflexivity become structural [to the discipline], owing to the increasing stress on the third view of colonialism, as a struggle that constantly renegotiates the balance of domination and resistance” (165).

Specifically responding to these critiques, “interpretivists” such as Rabinow (1977) and Marcus and Fischer (1986) acknowledge anthropology’s historical links to colonial practices, and sought to create a new ethnographic and anthropological project to address its shortcomings. Manganaro (1990) links these authors’ approach, when he says that

In what one could call the age of Foucault, it is impossible not to recognize that anthropology as a field, like any other, is rife with power relations. Any analysis of the workings of the discipline cannot stay on a discursive level that wholly divorces verbal play from the context of domination. Citing the work of Talal Asad, Edward Said, and others, Rabinow (1986: 251) states that it has only been within the last decade that scholars have addressed in any serious form the power relations, on the one hand, between world power structures and anthropology, and, on the other, between anthropology and its subjects. The result has been that ‘both the macro- and microrelations of power and discourse between anthropology and its other are at least open to

¹²³ Undoubtedly, attributing the consciousness of colonised peoples to coinciding with the coloniser’s acknowledgment of the relationship is incredibly naïve.

inquiry. We now know some of the questions worth asking and have made asking them part of the discipline's agenda' (26-7).

This work on anthropology and colonialism motivated interpretivist anthropologists, and ultimately their close relationship formed one of the dominant methodological trends in anthropological theory in recent decades (Rabinow 1977, Fabian 1983, Marcus and Fischer 1986, Marcus 1999, Clifford 1997). The anthropology of colonialism has been backward looking focusing on a critique of the historical relationship, both structural and personal, that anthropologists and anthropological theory has to colonialism. Heeding this important critique the interpretivists developed a new purpose and method for anthropological enquiry that integrates "the hermeneutic insight that the investigator is always situated and must understand the meaning of his cultural practices from within them" (Dreyfus and Rabinow 1983: xii).

Though all of these authors clearly and importantly identify the deep structural relationship between anthropology and colonialism, and note the importance for a personal and a disciplinary reflexivity, they miss an important area of analysis. George Stocking's Volume 7 of the History of Anthropology series, *Colonial Situations*, is a case in point. In his edited book, where he was greatly assisted by Asad's "substantial editorial role" (Stocking 1991: 7), he begins by acknowledging that the topic of colonialism is too immense to "comprehensively" cover all encounters in one book (ibid.: 5-6). As if to reinforce this point, he lists a few topics that are worthy of considering, but were not "realizable" in the present text, including topics such as: Evans-Pritchard

among the Nuer; Boas among the Kwakiutl; Grey in New Zealand; Peron in Australia; and Las Casas's writings about South American Indians (*ibid.*: 6). With this list of omissions added to the topics actually covered in the text, a list which Stocking says "cover[s] the range of modern ethnography in its major phases," not one article focuses on the colonisation of the United States or on the connection of the discipline of anthropology to US colonial policy.¹²⁴

Tellingly, Asad, Lewis, and Pels also make no mention of the processes of colonialism within the US or of the involvement of American anthropology within their representative articles. As observers, they see colonialism as distant from North America and somehow exotic, and most fully represented by British social anthropology and its preoccupation with Africa and Polynesia. As analysts working from a self-reflexive project to stave off the "crises" in the discipline,¹²⁵ and even often situated in the US themselves, these authors have focused their gaze away from North America. An example from Pels illustrates this approach:

The social scientific study of colonial society predates the 1960s. *After decolonization*, however, a set of interests started to converge that can now be regarded as constituting a new departure. Ethnohistory questioned the boundaries between anthropology and history. The *formerly colonized* raised

¹²⁴ The closest article to touch on this relationship is Feit's piece on Algonquian hunting territories and the resolution of the Speck/Leacock debate. Feit's focused article makes only a slight reference to the greater colonial situation, and it is inaccurate to contend that this one example "covered" the *colonial situation* in North America.

¹²⁵ As alleged in the opening line of Dianne Lewis' (1973) article, "Anthropology is in a state of crisis" and repeated a quarter-century later by Herb Lewis (1999, 2005).

doubts about the relevance of anthropology. Neo-marxist and feminist approaches to peasant societies and their modes of production, and the economy of the household fuelled an interest in economic change, and consequently, in colonialism. Critical approaches to classical anthropology questioned the nature of the knowledge required for colonial rule and the involvement of anthropologists in its production, and paved the way for some of the analytics of knowledge and power that matured later on. And the Kuhnian, historical and sociological, turn in the philosophy of science helped raise doubts about the claim to scientific independence from colonial circumstances that had been made by anthropologists since the early twentieth century (Pels 1997: 165, my emphases).

Within these accounts there is virtually no recognition that North America continues to be colonised (Asch 2002) and no acknowledgment of the role that anthropology has played in this on-going project. This denial occurs despite the protestations of activists such as Deloria (1969), who, throughout his career, questioned the role of anthropology in Native Americans' lives. This omission also occurs despite two explicit statements in Asad's *Anthropology and the Colonial Encounter*. For one, Lackner states that "[t]he United States has replaced Britain and France as the main imperialist nation, their own cultural relativism (their leading anthropological theory previously) has developed into evolutionism, a theory which provides a good basis for a high level of interventionism under the guise of 'civilising the natives'" (149) before switching back to discussing Europe. And two, the topic is acknowledged again, but stuck in the "Bibliographic

Notes,” where Moore’s *Perspectives for a Partisan Anthropology* is referenced, reporting that he “traces the development of both British and American anthropology in relation to the changing needs of the two imperialisms. The former aided the colonial administration as well as providing the rationales needed in an exploitative system. The latter, when involved in the colonisation of Amerindian lands provided a similar set of settler ideologies” (Marfleet 1973: 277), but with no further discussion of substance or hint of the significance of these statements offered in the remainder of the entire text.

Moreover, this omission occurs despite the empirical research offered by numerous anthropologists of North American Indigenous peoples, social scientists who have repeatedly documented the connection and implication of anthropological theory to colonial practices in North America (Lurie 1956, Stewart 1985, Clemmer 1969, Asch 1979, 1984, 1992, Feit 1982, 1994, n.d., Kehoe 1981, Biolsi 1995). Indeed, there is a strong demonstration from the historiography of the discipline that American anthropology proceeded on the assumption that it was an objective, scientific manner completely decontextualised from the politics of state relations with Native Americans, a representation that does not bear fidelity to history. That is, as Velaquez (1980: 56) points out specifically in reference to the Puerto Rico Project, there is reluctance amongst Americans to admit that their country has colonial practices.

Following the spirit of the critique of anthropology and colonialism offered by Asad to examine the “structure of power represented by the colonial system” (1973: 17), this dissertation is offered as an entrée into the analysis of the connection between

anthropology and colonialism in North America. It is undertaken through an examination of one such structure of power: the connection between colonial law regarding rights and title and the representation of Indigenous peoples as minimally socially evolved.

This analysis follows the trail broken by Ronaasen *et al* (1999), who first identified the relationship between Steward's concept of "the levels of sociocultural integration" and his testimony before the ICC. They demonstrate that Steward's theory was at the centre of the ethno-legal argument before the ICC, and they stress its applicability to the legal argument before the Commission by acknowledging that it

conceptualized property rights as a variable of land tenure, which in turn was a variable of sociopolitical organization. According to Steward's theory, only sociopolitical entities that reached a certain level of organization on an evolutionary scale ... could have developed concepts of holding land as property (1999: 172).

Secondly, though not well known but of tremendous disciplinary consequence, Ronaasen *et al* also state that Chapter 6 of Steward's *Theory of Culture Change*, "The Great Basin Shoshonean Indians: An Example of a Family Level of Sociocultural Integration," was also the Department of Justice's statement of defence, *in verbatim*, before the ICC, Dockets 87 & 88 (*ibid.*: 176-7; University of Illinois Archives, Julian H. Steward Papers (Record Series 15/2/21) [hereafter JHSP], Box 3). That is, in *Theory of Culture Change* Steward explains that the "concept of the levels of sociocultural integration" is the

“substantive application” of his theoretical and methodological project (1955: “Contents,” 99, 101-21); however, he does not divulge within the book that this was his explicit testimony to deny Indian interests in lands for the US government in legal proceedings. Nor does the discipline acknowledge this involvement.

I have provided information to show Steward’s involvement in the Ute cases, and that he took a leading advocacy role on behalf of the colonial project by locating himself as an advisor to and expert witness for the US Government’s Department of Justice, that he helped to develop an ethnographic image and legal opinion that the Indians of the Great Basin were of the lowest order of social evolution, and that his academic, proclaimed and celebrated “objective” work, is, in places, his testimony *in verbatim* before the ICC that had the explicit goal of creating a jurisdictional vacuum in the Great Basin; specifically creating a social evolutionary ladder, in the concept of “the levels of sociocultural evolution,” that had exact applicability for undermining the rights to land of the people he was testifying against in court.

This dissertation shows that contrary to the standard references on him, Julian Steward played a crucial role in the US colonial project, working on the side of the colonial authorities to undermine the land rights of Indian nations. As such, this analysis begins to fill a deep gap in the discipline’s self-examination of our relationship to colonial practices. To support my conclusions, I have provided information to show that Steward took a leading advocacy role on behalf of the colonial project by locating himself as an advisor to and expert witness for the US Government’s Department of Justice, that he

changed his conclusions without additional data in an effort to develop an ethnographic image and legal opinion that the American Indians of the Great Basin were of the lowest order of social evolution, and that his academic, proclaimed and celebrated “objective” work, is, in places, his *verbatim* testimony before the ICC that had the explicit goal of creating a jurisdictional vacuum in the Great Basin; specifically creating a social evolutionary ladder, in the concept of “the levels of sociocultural evolution,” that had exact applicability for undermining the rights to land of the people he was testifying against in court.

Finally, I have shown that Steward’s science follows a Spencerian evolutionism and is the inverse of Boas’ stated method for the discipline. Through this examination of Steward’s work, this dissertation is offered as a contribution to the critical analyses that have been undertaken on the relationship between anthropology and the colonial project in the United States and Canada, and in particular the roles our discipline plays in the ongoing struggle over the rights of those colonial authorities to erase the legitimate rights of Indigenous peoples who find themselves located within their borders. In light of Steward’s intimate connection and positioning within the colonial project, as I have demonstrated, I ask the discipline to consider where it positions itself in this struggle when it valorises his work as foundational for the development of a value-free, objective science and accepts the methodological “advancements” offered in the past few decades as sufficient for addressing this relationship.

- Abrams, M.H. (1993) *A Glossary of Literary Terms*. Fort Worth: Harcourt Brace.
- American Anthropologist (1934) Notes and News, in *American Anthropologist*, 36(1), p. 143.
- Asad, Talal (1990) 'From the History of Colonial Anthropology to the Anthropology of Western Hegemony', in G. W. Stocking (ed) *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*, pp. 314-24. Madison: University of Wisconsin Press.
- Asad, Talal, ed (1973) *Anthropology and the Colonial Encounter*. London: Ithaca Press.
- Asch, Michael (1979) 'The Ecological-Evolutionary Model and the Concept of Mode of Production', in David Turner *et al* (eds), *Challenging Anthropology*, pp. 81-97. Toronto: McGraw Hill.
- Asch, Michael (1984) *Home and Native Land: Aboriginal Rights and the Canadian Constitution*. Toronto: Methuen.
- Asch, Michael (1992) 'Errors in *Delgamuukw*: An Anthropological Perspective', in Frank Cassidy (ed) *Aboriginal Title in British Columbia: Delgamuukw vs. The Queen*, pp. 221-243. Vancouver: Oolichan Books.
- Asch, Michael (1999) 'From Calder to Van der Peet: Aboriginal Rights and Canadian Law, 1973-96', in Paul Havemann (ed) *Indigenous Peoples' Rights in Australia, Canada, and New Zealand*, pp. 428-446. Melbourne: Oxford University Press.
- Asch, Michael (2000) 'The Judicial Conception of Culture after *Delgamuukw*', *Review of Constitutional Studies* 5(2), 119-37.
- Asch, Michael (2002) 'From Terra Nullius to Affirmation: Reconciling Aboriginal Rights with the Canadian Constitution', *Canadian Journal of Law and Society* 17(2): 23-39.
- Asch, Michael (2004) 'Les Structures Élémentaires de la Parenté et la Pensée Politique occidentale,' *Les Temps Modernes* (Special issue dedicated to Claude Lévi-Strauss, edited by Boris Wiseman) 628: 201-231.
- American Anthropological Association (1947) Statement on Human Rights, *American Anthropologist*, 49(4), part 1, pp. 539-43.
- Bailey, Gordon and Nora Gayle (1993) *Sociology an Introduction: From Classics to Contemporary Feminists*. Oxford: Oxford University Press.

- Barnard, Alan (2000) *History and Theory in Anthropology*. Cambridge: Cambridge University Press.
- Barney, Ralph (1955) 'Legal Problems Peculiar to Indian Claims Litigation', *Ethnohistory*, 2(4), pp. 315-25.
- Beals, Ralph (1985) 'The Anthropologist as Expert Witness: Illustrations from the California Indian Land Claims Case', in Imre Sutton (ed) *Irredeemable America: The Indians' Estate and Land Claims*, pp. 139-56. Albuquerque: University of New Mexico Press.
- Beck, Charlotte, ed (1999) *Models for the Millennium: Great Basin Anthropology Today*. Salt Lake City: University of Utah Press.
- Bell, Catherine and M. Asch. (1997) 'Challenging Assumptions: The Impact of Precedent in Aboriginal Rights Litigation', in Michael Asch (ed) *Aboriginal and Treaty Rights in Canada: Essays on Law, Equality and Respect for Difference*, pp. 38-74. Vancouver: UBC Press.
- Bettinger, Robert (1998) 'Cultural, Human, and Historical Ecology in the Great Basin: Fifty Years of Ideas About Ten Thousand Years of Prehistory', in William Balee (ed) *Advances in Historical Ecology*. New York: Columbia University Press, pp. 169-89.
- Biolsi, Tom (1995) 'Bringing the Law Back In: Legal Rights and the Regulation of Indian White Relations on Rosebud Reservation', *Current Anthropology*, 36(4), 543-71.
- Blackhawk, Ned (1999) 'Julian Steward and the Politics of Representation', in Richard Clemmer et al (eds.) *Julian Steward and the Great Basin: The Making of an Anthropologist*. Salt Lake City: University of Utah Press, pp. 203-18.
- Bohannon, Paul and Mark Glazer, eds (1988) *High Points in Anthropology*. New York: McGraw-Hill.
- Bowler, Peter (2003) *Evolution: The History of an Idea*. Berkeley: University of California Press.
- Bunzl, Matti (1996) 'Franz Boas and the Humboldtian Tradition: From Volksgeist and Nationalcharakter to an Anthropological Concept of Culture', in George Stocking Jr. (ed) *Volksgeist as Method and Ethic: Essays on the Boasian Ethnography and the German Anthropological Tradition*. Madison: University of Wisconsin Press, pp. 17-78.
- Bunzl, Matti (2004) 'Boas, Foucault, and the "Native Anthropologist": Notes Toward a Neo-Boasian Anthropology', *American Anthropologist*, 106(3), pp. 435-42.

- Carneiro, Robert (2003) *Evolutionism in Cultural Anthropology: A Critical History*. Boulder: Westview Press.
- Clemmer, Richard O. (1969) 'The Fed-Up Hopi: Resistance of The American Indian and the Silence of the Good Anthropologists', *Journal of the Steward Anthropological Society* 1: 18-40.
- Clemmer, Richard O. (1999) 'Steward's Gap', in Richard O. Clemmer et al (eds) *Julian Steward and the Great Basin: The Making of an Anthropologist*. Salt Lake City: University of Utah Press.
- Clemmer, Richard O., L. Daniel Myers, and Mary Elizabeth Rudden, eds (1999) *Julian Steward and the Great Basin: The Making of an Anthropologist*. Salt Lake City: University of Utah Press.
- Cole, Sally (2003) *Ruth Landis: A Life in Anthropology*. Lincoln: University of Nebraska Press.
- Crum, Steven (1999) 'Julian Steward's Vision of the Great Basin: A Critique and Response', in Richard O. Clemmer, L. Daniel Myers, and Mary Elizabeth Rudden (eds) *Julian Steward and the Great Basin: The Making of an Anthropologist*, pp. 117-127. Salt Lake City: University of Utah Press.
- Cruikshank, Julie (1992) 'Invention of Anthropology in British Columbia's Supreme Court: Oral Tradition as Evidence in *Degamuukw v. B.C.*', *BC Studies* 95: 25-42.
- Culhane, Dara (1992) 'Adding Insult to Injury: Her Majesty's Loyal Anthropologist', *BC Studies* 95: 66-92.
- Culhane, Dara (1998) *The Pleasure of the Crown: Anthropology, First Nations and the Law*. Burnaby, BC: Talonbooks.
- Dailey, David (2004) *Battle for the BIA: G.E.E Lindquist and the Missionary Crusade Against John Collier*. Tucson: University of Arizona Press.
- Daly, Richard (2005) *Our Box was Full*. Vancouver: University of British Columbia Press.
- Darnell, Regna (1990) *Edward Sapir: Linguist, Anthropologist, Humanist*. Berkeley: University of California Press.
- Darnell, Regna and Lisa Valentine, eds. (1999) *Theorizing the Americanist Tradition*. Toronto: University of Toronto Press.

- Darnell, Regna (2000) *And Along Came Boas: Continuity and Revolution in Americanist Anthropology*. Amsterdam: John Benjamins.
- Darnell, Regna (2001) *Invisible Genealogies: A History of Americanist Anthropology*. Lincoln: University of Nebraska Press
- Deloria, Vine Jr. (1969) *Custer Died for your Sins: An Indian Manifesto*. New York: Macmillan.
- Deloria, Vine Jr. (2004) 'Philosophy and Tribal Peoples', in Anne Waters (ed) *American Indian Thought*, pp. 3-12. Malden: Blackwell Publishing.
- Dreyfus H. L. and Paul Rabinow (1982) *Michel Foucault: Beyond Structuralism and Hermeneutics*. Brighton: Harvester.
- Erickson, Paul and Liam Murphy (1998) *A History of Anthropological Theory*. Peterborough: Broadview Press.
- Fabian, Johannes (1983) *Time and the Other: How Anthropology Makes its Object*. New York: Columbia University Press.
- Fagen, Brian (1998) *People of the Earth: An Introduction to the World Prehistory*. New York: Longman.
- Farb, Peter (1968) *Man's Rise to Civilization, As Shown by the Indians of North America from Primeval Times to the Coming of the Industrial State*. New York: E.P. Dutton.
- Feit, Harvey A. (1982) 'The Future of hunters within nation states: anthropology and the James Bay Cree', in Eleanor Leacock and Richard Lee (eds) *Politics and History in Band Societies*, pp. 373-412. Cambridge: Cambridge University Press.
- Feit, Harvey A. (1991) 'The Construction of Algonquian Hunting Territories: Private Property as Moral Lesson, Policy Advocacy, and Ethnographic Error', in George W. Stocking, Jr. (ed) *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*, pp. 109-134. Madison: University of Wisconsin Press.
- Feit, Harvey A. (1994) 'The Enduring Pursuit: Land, Time, and Social Relationships in Anthropological Models of Hunter-Gatherers and Subarctic Hunter's Images', in Ernest R. Burch, Jr. and Linda J. Ellanna (eds), *Key Issues in Hunter-Gatherer Research*, pp. 421-439. Oxford: Berg.
- Feit, Harvey A. (n.d.) 'Objectivity, Advocacy, and Trust in Shepard Krech's *The Ecological Indian*', in Marc Pinkoski (ed) *Just Relations: Essays in Anthropology and the Law in Canada*.

- Foucault, Michel (1997) 'Technologies of the Self', in Paul Rabinow, (ed) *Ethics, Subjectivity and Truth*, pp. 223-52. New York: The New Press.
- Fried, Morton, ed (1968) *Readings in Anthropology, Volumes 1 and 2*. New York: Thomas Y. Crowell Company.
- Friedman, Jonathan (1987) 'An Interview With Eric Wolf', in *Current Anthropology* 28(1), 107-118.
- Geertz, Clifford (1973) *The Interpretation of Cultures*. New York: Basic Books
- Goody, Jack (1995) *The Expansive Moment: The Rise of Social Anthropology in Britain and Africa 1918-1970*. Cambridge: Cambridge University Press.
- Gough, Kathleen (1968) 'New Proposals for Anthropologists', *Current Anthropology* 9(5), 403-435.
- Haenn, Nora and Richard Wilk (2006) *The Environment in Anthropology: A Reader in Ecology, Culture and Sustainable Living*. New York: New York University Press.
- Handler, Richard, ed. (2000) *Excluded Ancestors, Inventible Traditions: Essays Toward a More Inclusive History of Anthropology*. Madison: University of Wisconsin Press.
- Harris, Marvin (1968) *The Rise of Anthropological Theory: A History of Theories of Culture*. New York: Crowell.
- Harrison, Julia and Regna Darnell, eds. (2006) *Historizing Canadian Anthropology*. Vancouver: University of British Columbia Press.
- Helm, June (1986) 'Horde, Band and Tribe: Northern Approaches', Presidential Lecture, University of Iowa, <http://sdrc.lib.uiowa.edu/preslectures/helm86/index.html> (last visited October 10, 2006)
- Hymes, Dell, ed. (1969) *Reinventing Anthropology*. New York: Pantheon.
- Indian Claims Commission (1974) *Ute Indians II, Commission Findings*. New York: Garland.
- Ingold, Tim (2000) 'On the Social Relations of the Hunter-Gatherer Band', in Richard B. Lee and Richard Daly (eds) *The Cambridge Encyclopedia of Hunters and Gatherers*, pp. 399-410. Cambridge: Cambridge University Press.
- Johnson, Allen W., and Timothy Earl (1987) *The Evolution of Human Societies: From Foraging Group to Agrarian State*. Stanford: Stanford University Press.
- Kehoe, Alice (1981) 'Revisionist Anthropology: Aboriginal North America', in *Current Anthropology*, 22(5), pp. 503-517.

- Kelly, Lawrence (1983) *The Assault on Assimilation: John Collier and the Origins of Indian Policy Reforms*. Albuquerque: University of New Mexico Press.
- Kerns, Virginia (2003) *Scenes from the High Desert: Julian Steward's Life and Theory*. Urbana: University of Illinois Press.
- Kroeber, Alfred, ed. (1953) *Anthropology Today: An Encyclopedic Inventory*. Chicago: University of Chicago Press.
- Kroeber, Alfred (1955) 'The Nature of the Land Holding Group', *Ethnohistory*, 2(4), pp. 303-14.
- Kuper, Adam (1999) *Culture: The Anthropologist's Account*. Cambridge: Harvard University Press.
- Lackner, Helen (1973) 'Colonial Administration and Social Anthropology: Eastern Nigeria 1920-1940', in Talal Asad (ed) *Anthropology and the Colonial Encounter*, pp. 123-52. London: Ithica Press.
- Lauria-Perricelli, Antonio (1989) *A Study in Historical and Critical Anthropology: The Making of "The People of Puerto Rico."* Unpublished Ph.D. Dissertation, New School for Social Research.
- Layton, Robert (1997) *An Introduction to Theory in Anthropology*. Cambridge: Cambridge University Press.
- Lee, Richard B., and Irven DeVore, eds (1968) *Man The Hunter* Chicago: Aldine.
- Lesser, Alexander (1981) 'Franz Boas,' in Sydel Silverman (ed) *Totems and Teachers: Perspectives on the History of Anthropology*. New York: Columbia University Press, pp. 1-27.
- Lewis, Diane (1973) 'Anthropology and Colonialism', in *Current Anthropology*, 14(5), pp. 581-91.
- Lewis, Henry T. (2002) 'An Anthropological Critique', in by Omer C. Stewart, H.T. Lewis and M.K. Anderson (eds) *Forgotten Fires: Native Americans and the Transient Wilderness*, pp. 17-36. Norman: University of Oklahoma Press.
- Lewis, Herbert S. (1999) 'The Misrepresentation of Anthropology and its Consequences.' *American Anthropologist*, 100(3): 716-731.
- Lewis, Herbert S. (2005) 'Anthropology, the Cold War, and Intellectual History', *Histories of Anthropology*, Annual 1: 99-113.

- Lurie, Nancy Oestreich (1955) 'Problems, Opportunities, Recommendations', *Ethnohistory* 2(4), pp. 357-75.
- Lurie, Nancy Oestreich (1956) 'A Reply To "The Land Claims Cases: Anthropologists in Conflict"', *Ethnohistory* 3(3), pp. 256-279.
- Lurie, Nancy Oestreich (1957) 'The Indian Claims Commission Act', *Annals of the American Academy of Political and Social Science*, 311: 56-70.
- Lurie, Nancy Oestreich (1970) 'Anthropologists in the U.S. and the Indian Claims Commission', paper presented to the American Ethnological Society-Northeastern Anthropological Association Meetings, Ottawa, Ontario, May 7-9.
- MacDonald, Kevin (1998) *The Critique of Culture: An Evolutionary Analysis of Jewish Involvement in Twentieth-Century Intellectual Political Movements*. Westport: Praeger.
- Manganaro, Marc (1990) 'Textual Play, Power and Cultural Critique: An Orientation to Modernist Anthropology', in Marc Manganaro (ed) *Modernist Anthropology: From Fieldwork to Text*, pp. 3-50. Princeton: Princeton University Press.
- Manners, Robert (1973) 'Julian Haynes Steward, 1902-1972,' *American Anthropologist* 75(3): 886-97.
- Marcus, George E. and Michael J. Fischer (1986) *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences*. Chicago: University of Chicago Press.
- Marfleet, Philip (1973) 'Bibliographic Notes on the Debate', in Talal Asad (ed) *Anthropology and the Colonial Encounter*, pp. 273-84. London: Ithica Press.
- McGee, R. Jon and Richard Warms, eds. (1996) *Anthropological Theory: An Introductory History*. Mountain View: Mayfield.
- Mead, Margaret (1959) 'Apprenticeship under Boas', W. Goldschmidt (ed) *The Anthropology of Franz Boas. Memoir 89*, American Anthropological Association, pp. 29-45.
- Miller, Sterling (1978) 'Araucanian Population Density: An Error in the Handbook of South American Indians', *American Anthropologist*, 80(4), pp. 940-942.
- Moore, John (1971) 'Perspective for a Partisan Anthropology', in *Liberation*, November, pp. 34-43.
- Moore, Jerry D. (1997) *Visions of Culture: An Introduction to Anthropological Theories and Theorists*. Walnut Creek: AltaMira Press.

- Murphy, Robert F. (1977) 'Introduction', in Jane C. Steward and Robert F. Murphy (eds) *Evolution and Ecology: Essays on Social Transformation*, pp. 1-39. Urbana: University of Illinois Press.
- Murphy, Robert F. (1981) 'Julian Steward', in Sydel Silverman (ed) *Totems and Teachers*, pp. 171-208. New York: Columbia University Press.
- Murphy, Robert F., and Julian H. Steward (1956) 'Tappers and Trappers: Parallel Processes in Acculturation', *Economic Development and Culture Change* 4: 335-355.
- Myers, L. Daniel (2004) 'Subtle Shifts and Radical Transformations in Hunter-Gatherer Research in American Anthropology: Julian Steward's Contributions and Achievements', in Alan Barnard (ed) *Hunter-Gatherers in History, Archaeology and Anthropology*, pp. 175-186. Oxford: Berg.
- Nadasdy, Paul (2003) *Hunters and Bureaucrats: Power, Knowledge, and Aboriginal-State Relations in the Southwest Yukon*. Vancouver: University of British Columbia Press.
- Ortner, Sherry (1984) 'Theory in Anthropology Since the Sixties', *Comparative Studies in Society and History* 26(1): 126-166.
- Peace, William (2004) *Leslie A. White: Evolution and Revolution in Anthropology*. Lincoln: University of Nebraska Press.
- Pels, Peter (1997) 'The Anthropology of Colonialism: Culture, History and the Emergence of Western Governmentality', in *Annual Review of Anthropology*, 26, pp. 196-83.
- Philp Kenneth R. (1977) *John Collier's Crusade for Indian Reform: 1920-1954*. Tucson: University of Arizona Press.
- Pinkoski, Marc and Michael Asch (2004) 'Anthropology and Indigenous Rights in Canada and the United States: Implications in Steward's Theoretical Project', in Alan Barnard (ed) *Hunter-Gatherers in History, Archaeology and Anthropology*, pp. 187-200. Oxford: Berg.
- Price, David (2004) *Threatening Anthropology: McCarthyism and the FBI's Surveillance of Activist Anthropologists*. Durham: Duke University Press.
- Rabinow, Paul (1977) *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press.
- Ray, Vern (1955) 'Introduction', *Ethnohistory*, 2(4), pp. 287-91.

- Reynolds, Henry (1992) *The Law of the Land*. Ringwood: Penguin.
- Ricoeur, Paul ([1968] 1987) 'On Interpretation,' in Barnes et al (ed) *After Philosophy: End or Transformation?*, pp. 357- 380. Cambridge: MIT Press.
- Ronaasen, Sheree (1993) 'Cultural Ecology and Functionalism: Social Theory Framed by a Colonial Political Context', Unpublished MA Thesis, University of Alberta, Edmonton.
- Ronaasen, Sheree, Richard O. Clemmer, and Mary Elizabeth Rudden (1999) 'Rethinking Cultural Ecology, Multilinear Evolution, and Expert Witnesses: Julian Steward and the Indian Claims Commission Proceedings', in Richard O. Clemmer, L. Daniel Myers, and Mary Elizabeth Rudden (eds) *Julian Steward and the Great Basin: The Making of an Anthropologist*, pp. 170-202. Salt Lake City: University of Utah Press.
- Rosenthal, Harvey (1985) 'Indian Claims and the American Conscience: A Brief History of the Indian Claims Commission', in Imre Sutton (ed) *Irredeemable America: The Indians' Estate and Land Claims*. Albuquerque: University of New Mexico Press.
- Rosen, Lawrence (1977) 'The Anthropologist as Expert Witness', *American Anthropologist* 79(3), pp. 555-78.
- Rosenthal, Harvey (1990) *Their Day in Court: A History of the Indian Claims Commission*. New York: Garland.
- Rouse, Irving (1953) 'The Circum-Caribbean Theory, an Archeological Test', *American Anthropologist* 55(2), Part 1, pp. 188-200
- Rusco, Elmer (1999) 'Julian Steward, the Western Shoshones, and the Bureau of Indian Affairs: A Failure to Communicate', in Richard O. Clemmer, L. Daniel Myers, and Mary Elizabeth Rudden (eds) *Julian Steward and the Great Basin: The Making of an Anthropologist*, pp. 85-116. Salt Lake City: University of Utah Press.
- Sahlins, Marshall and Elman Serice, eds. (1960) *Evolution and Culture*. Ann Arbor: University of Michigan Press.
- Shimkin Demitri (1964) 'Julian H. Steward: A Contributor to Fact and Theory in Cultural Anthropology', in R. Manners (ed) *Process and Pattern in Culture: Essays in Honor of Julian H. Steward*, pp. 1-17. Chicago: Aldine.
- Sidky, H. (2005) *Perspectives on Culture: A Critical Introduction to Theory in Cultural Anthropology*. Upper Saddle River: Pearson.

- Silverman, Sydel, ed. (1981) *Totems and Teachers: Perspectives on the History of Anthropology*. New York: Columbia University Press.
- Silverman, Sydel (2005) "The United States", in *One Discipline, Four Ways: British, German, French and American Anthropology*, The Halle Lectures. Chicago: University of Chicago Press.
- Slattery, Brian (1979) *The Land Rights of Indigenous Canadian Peoples, as Affected by the Crown's Acquisition of Their Territory*. Unpublished PhD dissertation. University of Oxford.
- Spencer, Herbert (1969 [1876]) *Principles of Sociology*, Stanislav Andreski (ed). Hamdon: Archon.
- Sponsell, Leslie (2006) "Steward, Julian H. (1902-1972)", in H. James Birx (ed) *Encyclopedia of Anthropology*, Vol. 5, pp. 2128-2130. Thousand Oaks: Sage Publications.
- Steward, Julian H. (1929) 'Diffusion and Independent Invention: A Critique of Logic', *American Anthropologist*, 31: 491-5.
- Steward, Julian H. (1931) 'Notes on Hopi Ceremonies in their Initiatory Form in 1927-1928', *American Anthropologist*, 33: 56-79.
- Steward, Julian H. (1932) 'A Uintah Bear Dance, March 1931', *American Anthropologist*, 34: 262-73.
- Steward, Julian H. (1936a) 'The Economic and Social Basis of Primitive Bands', in Robert Lowie (ed) *Essays on Anthropology in Honor of Alfred Louis Kroeber*, pp. 311-350. Berkeley: University of California Press.
- Steward, Julian H. (1936b) 'Shoshonean Indian Tribes: Utah, Idaho, Nevada, Eastern California. Bureau of Indian Affairs'. JHSP Box 10, Indian Reorganization.
- Steward, Julian H. (1936c) 'Shoshoni Polyandry', *American Anthropologist*, 38: 561-64.
- Steward, Julian H. (1937a) 'Ecological Aspects of Southwestern Society', *Anthropos*, 32, pp. 87-104.
- Steward, Julian H. (1937b) 'Linguistic Distribution and Political Groups of the Great Basin Shoshoneans', *American Anthropologist*, 39: 625-634.
- Steward, Julian H. (1938) *Basin-Plateau Aboriginal Sociopolitical Groups*. Bureau of American Ethnology Bulletin 120. Washington, DC: United States Government Printing Office.

- Steward, Julian H. (1941a) 'Determinism in Primitive Society?', *Scientific Monthly*, 53: 491-501.
- Steward, Julian H. (1941b) 'Recording Culture Changes Among the Carrier Indians of British Columbia. *Explorations and Field-Work of the Smithsonian Institution in 1940*, pp. 83-90.
- Steward, Julian H., ed. (1946a) *The Marginal Tribes, Handbook of South American Indians, Volume 1*. Bureau of American Ethnology Bulletin 143.
- Steward, Julian H., ed. (1946b) *The Andean Civilizations, Handbook of South American Indians, Volume 2*. Bureau of American Ethnology Bulletin 143.
- Steward, Julian H. (1948a) *The Circum-Caribbean Tribes, Handbook of South American Indians, Volume 4*. Bureau of American Ethnology Bulletin 143
- Steward, Julian H., ed. (1948b) *The Tropical Forest Tribes, Handbook of South American Indians, Volume 3*. Bureau of American Ethnology, Bulletin 143.
- Steward, Julian H. (1948c) 'Comment on the Statement of Human Rights', *American Anthropologist*, 50: 351-2.
- Steward, Julian H. (1949) 'Cultural Causality and Law: A Trial Formulation of the Development of Early Civilizations', *American Anthropologist*, 51: 1-27.
- Steward, Julian H. (1950) *Area Research: Theory and Practice*. New York: Social Science Research Council.
- Steward, Julian H. (1951) 'Levels of Sociocultural Integration: An Operational Concept', *Southwestern Journal of Anthropology*, Vol 7, pp. 374-390.
- Steward, Julian H. (1953b) 'Aboriginal and Historic Groups of the Ute Indians of Utah: An Analysis. Washington: United States Department of Justice; JHSP Box 10.
- Steward, Julian H. (1953b) 'Evolution and Process', in Alfred Kroeber (ed) *Anthropology Today*. Chicago: University of Chicago Press, pp. 313-25.
- Steward, Julian H. and Erminie Wheeler-Voegelin (1954) 'The Northern Paiute Indians'. Washington: United States Department of Justice.
- Steward, Julian H. (1955) *Theory of Culture Change: The Methodology of Multilinear Evolution*. Urbana: University of Illinois Press.
- Steward, Julian H. (1955b) 'Theory and Application in a Social Science', *Ethnohistory* 2(4), pp. 291-302.

- Steward, Julian H. and Robert F. Murphy (1956) 'Tappers and Trappers: Parallel Processes in Acculturation', in *Economic Development and Cultural Change*, 4: 335-55.
- Steward, Julian H. (1961) Alfred Louis Kroeber, 1876-1960, *American Anthropologist*, 63: 1938-60.
- Steward, Julian H. (1968) 'Scientific Responsibility in Modern Life', *Science* 159(3811): 147-148.
- Steward, Julian H. (1969) 'Limitations of Applied Anthropology: The Case of the American Indian New Deal', *Journal of the Steward Anthropological Society*, 1(1), pp. 1-17.
- Steward, Julian H. (1970) 'The Foundations of Basin-Plateau Shoshonean Society', in Earl Swanson (ed) *Languages and Cultures of Western North America: Essays in Honor of Sven S. Lilijeblad*. Pocatello: Idaho State University Press, pp. 113-51.
- Steward, Julian H. (1973) *Alfred Kroeber*. New York: Columbia University Press.
- Stewart, Omer (1959) 'Anthropology and the Indian Claims', *Delphian Quarterly*, 42(3): 8-13.
- Stewart, Omer (1985) 'The Shoshone Claims Cases', in Imre Sutton (ed) *Irredeemable America: The Indians' Estate and Land Claims*, pp. 187-206. Albuquerque: University of New Mexico Press.
- Stocking, George W., Jr. (1968a) 'Franz Boas and the Culture Concept in Historical Perspective', in *Race, Culture, and Evolution: Essays in the History of Anthropology*. New York: The Free Press.
- Stocking, George W., Jr. (1968b) 'The Scientific Reaction Against Cultural Anthropology', in *Race, Culture, and Evolution: Essays in the History of Anthropology*. New York: The Free Press
- Stocking, George W., Jr. (1974) *A Franz Boas Reader: The Shaping of American Anthropology, 1883-1911*. Chicago: The University of Chicago Press.
- Stocking, George W., Jr., ed (1991) *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*. Madison: University of Wisconsin Press.
- Stocking, George W., Jr. (1995) *Volksgeist as Method and Ethic: Essays on the Boasian Ethnography and the German Anthropological Tradition*. Madison: University of Wisconsin Press.

- Sutton, Imre, ed. (1985) *Irredeemable America: The Indians' Estate and Land Claims*. Albuquerque: University of New Mexico Press
- Tax, Sol, ed. (1962) *Anthropology Today: Selections*. Chicago: Aldine.
- Thomas, David Hurst (1983) 'On Steward's Models of Shoshonean Sociopolitical Organization: A Great Bias in the Basin?', in Elisabeth Tooker (ed) *The Development of Political Organization in Native North America*. Proceedings of the American Ethnological Society 1979, pp. 59-68. Washington: American Ethnological Society.
- Trencher, Susan (2000) *Mirrored Images: American Anthropology and American Culture, 1960-1980*. Westport: Bergin & Garvey.
- Trencher, Susan (2002) 'The American Anthropological Association and the Values of Science, 1935-70', *American Anthropologist*, 104(2), pp. 450-62.
- Trigger, Bruce (1989) *A History of Archaeological Thought*. Cambridge: University of Cambridge Press.
- Trigger, Bruce (1996) *Sociocultural Evolution: Calculation and Contingency*. Oxford: Blackwell.
- Turney-High (1940) Review of Basin-Plateau Aboriginal Sociopolitical Groups, by Julian Steward, *American Anthropologist*, 42(1), pp. 136-8.
- Turner, Jonathan et al (2002) *The Emergence of Sociological Theory*. Belmont: Wadsworth Publishing.
- Velazquez, Rene (1980) 'Julian Steward's Perspective on Puerto Rico', in Ronald Duncan et al (eds) *The Anthropology of the People of Puerto Rico*. San German: Caribbean Institute and Study Center for Latin America and the Inter American University of Puerto Rico, pp. 50-8.
- Verdon, Michel (2006a) 'Boas and Holism: A Textual Analysis', *Philosophy of the Social Sciences*, 36(3), pp. 276-302.
- Verdon, Michel (2006b) 'The World Upside Down: Boas, History, Evolutionism, and Science', *History and Anthropology*, 17(3), pp. 171 - 187.
- Wallace, Pamela (2002) 'Indian Claims Commission: Political Complexity and Contrasting Concepts of Identity,' *Ethnohistory*, 49(4): 743-766.
- Webbink, Paul (1950) 'Foreward', in Julian Steward *Area Research: Theory and Practice*. New York: Social Sciences Research Council.

- White, Leslie (1963) 'The Ethnography and Ethnology of Franz Boas', in *Texas Memorial Bulletin* 6. Austin: University of Texas.
- Wishart, David J. (1985) 'The Pawnee Claims Case, 1947-64', In Imre Sutton, ed., *Irredeemable America: The Indians' Estate and Land Claims*, pp. 157-186. Albuquerque: University of New Mexico Press.
- Wilkins and Lomawaima (2001) *Uneven Ground: American Indian Sovereignty and Federal Law*. Norman: University of Oklahoma Press.
- Wolf, Eric (2001) *Pathways of Power: Building an Anthropology of the Modern World*. Berkeley: University of California Press.