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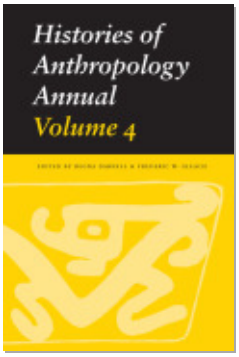
Marc Pinkoski



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> Tribal peoples have traditionally been understood by Westerners as the last remnants of a hypothetical earlier stage of cultural evolution, and this so-called "primitive" stage of human development is a necessary preamble to any discussion of human beings and the meaning of their lives. Indeed, the stereotype of primitive peoples anchors the whole edifice of Western social thought. We need the primitive so that we can distinguish Western civilization from it and congratulate ourselves on the progress we have made. John Locke and Thomas Hobbes may have articulated the idea formally by beginning their theories of the social contract with the hypothetical stage wherein primitive people established a society, but subsequent generations of Western people have wholeheartedly accepted the image without any critical examinations of its validity. Thus the attitude of many philosophers is that American Indians must represent the stage of human development in which superstition and ignorance reigned supreme.
>
> Vine Deloria 2004:3–4

Introduction

The relationship between anthropological theory and colonialism in North America has been widely neglected in the historiography of the discipline.[1] 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-a-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 (Asad 1973; 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.

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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 conditions, the working assumptions and the intellectual product of all disciplines representing the European understanding of non-European humanity. (Asad 1973:18–19)

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 it sustained physical proximity between the observing European and the living non-European became a practical possibility" (1973:17). 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: Essays on the Contextualization of Ethnographic Knowledge* (1991); and finally, Peter Pels's *Annual Review of Anthropology* survey, "The Anthropology of Colonialism" (1997). In each of these 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 colonial practices; and, they agree that the discipline needs to address this history fully, because the relationship is at least twofold. 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 personalizes 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).

A New Disciplinary Method

To overcome the unequal social relations embedded in the methods of anthropology, these authors advocated 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 sup-

ports 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" (1973:581).[2] Pels's recent review article appears to confirm Lewis's 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" (1997:165).

Specifically responding to these critiques, "interpretivists" such as Rabinow (1977) and Marcus and Fischer (1986) acknowledged anthropology's historical links to colonial practices, and sought to create a new ethnographic and anthropological project to addresses its shortcomings. Manganaro links these authors' approaches, when he says,

> 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 inquiry. We now know some of the questions worth asking and have made asking them part of the discipline's agenda." (1990:26–27)

This work on anthropology and colonialism motivated interpretivist anthropologists, and ultimately the close relationship between the two "schools" formed one of the dominant methodological trends in anthropological theory in recent decades (Rabinow 1977; 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 anthro-

pological theory have 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 *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 (1991:5–6). 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 (1991:6). 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 colonization of the United States or on the connection of the discipline of anthropology to U.S. colonial policy.[3]

Tellingly, Asad, Lewis, and Pels also make no mention of the processes of colonialism within the United States 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, and even often situated in the United States themselves, these authors have focused their gaze away from North America.[4] 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 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; emphases added).

Within these accounts there is virtually no recognition that North America continues to be colonized (Asch 2002) and no acknowledgment of the role that anthropology has played in this ongoing 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; Kehoe 1981; Biolsi 1995). Indeed, as I will show below, there is a strong demonstration from the historiography of the discipline that American anthropology proceeded on the assumption that it was an objective, scientific matter completely decontextualized from the politics of state relations with Native Americans, a representation that does not

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bear fidelity to history. That is, as Velazquez (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.

Julian Steward and the Indian Claims Commission (ICC)

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), the remainder of this paper 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. My singular case example will be the iconic figure in American anthropology, Julian Steward, and I will further contextualize the connection between the "substantive application" of his theory and method—what he calls the "levels of sociocultural integration" (Steward 1950, 1951, 1955a[1954]),[5]—and his testimony to deny Native American claims to land for the U.S. government before the Indian Claims Commission (ICC) (JHSP, Record Series 15/2/21, Boxes 1–4).

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 defense, *in verbatim*, before the ICC, Dockets 87 and 88 (1999:176–177; 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 (1955a[1954]: "Contents," 99, 101–121);[6] however, he does not divulge within the book that this was his explicit testimony to deny Indian interests in lands for the U.S. government in legal proceedings. Nor does the discipline acknowledge this involvement. Below, I will show that his hidden testimony, what he calls the "substantive application" of his theory and method in his opus and what the discipline calls apolitical theory, demonstrates a propinquity to colonialism more intimate than any of the British social anthropologists implicated in their own nation's colonial projects.

Julian Steward

Julian Steward (1902–1972) maintains a disciplinary stature in American anthropology similar to that of his more critiqued contemporaries in the British school. In fact, internationally, Steward is recognized as one of the faces of American anthropology (Sponsell 2006). This is because of his ethnographic and archaeological work in the American Great Basin, work that resulted in the seminal ethnography, *Basin-Plateau Aboriginal Sociopolitical Groups* (1938); also because of his edited monumental six-volume collection, *The Handbook of South American Indians* (1946a, 1946b, 1948b, 1948a, 1949, 1950) and *The People of Puerto Rico* (1956); and, enhanced by his edited three volume-collection on modernization and development (Steward 1967). Beyond his ethnographic purview, Steward was a prolific writer during his forty-four-year academic career, a characteristic demonstrated, for example, by his dozens of contributions to *American Anthropologist* and his *Theory of Culture Change: The Methodology of Multilinear Evolution* (1955a[1954]).

Through his oeuvre, he developed and articulated one of the major sub-fields of anthropology, *cultural ecology*, a theoretical and methodological approach in which he supervised numerous prominent scholars' PhD dissertations, sat on their graduate committees, and greatly influenced their careers (Kerns 2003; Silverman 2005).To this point, it has been reported that Steward supervised the completion of thirty-five doctoral dissertations in his six-year tenure at Columbia University (Murphy 1981), with some of his most prominent students during this time being Eric Wolf, Elman Service, Morton Fried, Robert Murphy, Robert Manners, Stanley Diamond, Louis Faron, and Sidney Mintz (Manners 1973; Kerns 2003).[7]

Steward's theoretical project is presented most fully in *Theory of Culture Change: The Methodology of Multilinear Evolution* (1955a[1954]), a text that continues to be among the most influential in the discipline

today. The book is the culmination of his preceding articles, with nine of its twelve chapters previously published, either explicitly or substantively. In fact, the text is the restatement of his published work in reverse chronological order, with the addition of his most oft-reproduced essay, "The Concept and Method of Culture Ecology," as chapter 2. Republishing his essays in this manner gave Steward the chance to reformulate his work-to-date and recast his papers within his newly stated method, "multilinear evolution;"[8] a concept that he explicitly states "constitutes the methodological position of the present collection of essays" of the book (1955a[1954]:4). This format also allowed Steward the opportunity to demonstrate how his theory and method were "elaborated" and "substantively applied" through his new operational concept, "the levels of sociocultural integration" (1955a[1954]:5, 43–63, 101–121).

Steward's work, as exemplified by *Theory of Culture Change*, has created a foundation for the discipline by purporting to offer a scientific method to the study of society in relation to environment (Haenn and Wilk 2006). 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. (Ortner 1984:132)

Typically, Steward's method is understood to offer an objective ethnographic portrayal of the American Great Basin. It is believed that his fieldwork led to strong, objective descriptions, and that extensive analysis, through his rigorous method of cultural ecology, generated "nomothetic" rules of culture change—a method that prompted Harris, 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 1968:666–667)

The cultural laws or rules of evolution that Steward devised are well recorded as foundational in the history of American anthropology because of their objective, scientific method for understanding social organization. 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 it 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).

With similar sentiment, in reproducing Steward's seminal essay "Concept and Method of Cultural Ecology" in *High Points in Anthropology*, Bohannan and Glazer (1988) introduce 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" (1988:322). 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" (1997:188).

At a minimum, one of the foundational claims that has become canonized within the discipline is that Steward's theoretical paradigm and the representations of Indigenous societies that flow from it are the result of objective, scientific analysis and, therefore, represent a value-free foundation for the study of society in general and of Indigenous societies in particular.[9] In contrast, I will provide an analysis of Steward's testimony before the ICC as an expert witness and strategist for the US Department of Justice. To demonstrate the connection of his work to colonial practices, I will discuss its relationship to the Ute (Nos. 44 and 45) and Paiute (Nos. 87, 88, 17, 100) cases before the Indian Claims Commission (ICC). I base this analysis on my archival research of Steward's papers and reports written during this time, an analysis that has been facilitated immeasurably by the recent biography of Steward (Kerns 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.

The Indian Claims Commission (ICC)

The Indian Claims Commission Act was passed by the U.S. Congress in 1946. The commission founded under this Act was organized 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), and explicitly marks the end of John Collier's twelve-year reign as Superintendent of Indian Affairs (1933–46)—and of one of the greatest anomalies in American Indian policy (Philp 1977).[10]

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 just such a mechanism to deal with the multitude of legal claims that Indian Nations had with the U.S. 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 to the resolution of outstanding land claims, this progress was tenuous since 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 1959, 1985).

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, based entirely on social evolutionary conjecture, and thus, as a consequence, had no legal standing before the Commission because of some sort of "ethnological difference" (e.g., *In Re: Southern Rhodesia*, 1919. 210 [PC]).

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As it happened, in frequent and continual practice, the U.S. Department of Justice questioned the level of social organization of the Indigenous peoples before the court (Wallace 2002) following a line of argument in the common law regarding the colonization of new territories by limiting the aboriginal interest in the land based on social evolutionism (Asch 1999, 2002).[11]

As cases appearing before the ICC took greater shape, both the Native American claimants and the federal government solicited expert testimony on the contemporary, ethnohistorical, and aboriginal areas in question before the court, thus placing anthropologists at the forefront of the legal discourse for determining Aboriginal interests to land (e.g., Steward 1955b, 1970; Kroeber 1955; Manners 1955; Lurie 1956, Stewart 1985; Ray 1955; Barney 1955). Both the renewed acceptance of "neo-evolutionary" developmental stage-theories within the discipline and anthropologists' newly accepted "scientific" expertise outside the discipline led Ronaasen et al. 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" (1999:171; e.g., Barney 1955).

Julian Steward and the Indian Claims Commission

Reams of documents in his archives demonstrate that Steward had an intimate relationship with the U.S. 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 (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," and asks him if he "would be willing to assist the Government in this case with regard to the aboriginal occupancy of the area in question" (Vanech to Steward, April 21, 1949, JHSP, Box 2).

Within a week, Steward replied succinctly and with positive interest to Vanech, requesting a clarification of duties, expectations, and recompense. From his initial work on the Ute cases, Steward remained with the Department of Justice offering testimony and strategy for much of the next seven years. 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 representa-

tives from the Department of Justice, began laying out the legal and anthropological framework to identify characteristics of recognizable social organization in law. The distinctions created by their enquiry led to the legal argument that there were non-identifiable groups of humans in the Great Basin, thus creating the representation of a jurisdictional vacuum in the area (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 [JHSP, Box 2]).

Steward and the Uintah Ute

In his first case, Steward presented a seventy-one-page expert report on behalf of the Department of Justice to the ICC entitled, "Aboriginal and Historic Groups of the Ute Indians of Utah: An Analysis."[12] His report identifies a three part "problem" regarding the "identification, characterization, and localization of aboriginal groups" in the Ute's territory (1953a:1); "facts" crucial for establishing Native American interests in and rights to the land. The first was that the concept of "tribe" has no relevance to the Ute; and, he asserts that the "customary division into tribes is a 'white man's' classification system."[13] The second, closely related, was 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.[14] The third was that the data to enable any analysis was too scarce to make any reasonable judgment, and thus all analyses would simply be an interpretation or "guesswork" (Steward to Williams, July 10, 1953, JHSP, Box 2). 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" (1953a: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 (1953a:5–7), but that after acquiring the horse, a new mode of subsistence and new social organization emerged (1953a:3). Explaining, he says that because there was no farming, families depended entirely upon their local environment (1953a:7), and as among other American Indians, the legal concept of exclusive ownership of soil was very rare (1952:10–11). This meant, for him, that because of limited territorial rights land could be readily sold by the

state for white settlement and could be used in new ways to develop the area (1953a:10).

He claims that political consolidation of any kind could occur only after the horse was introduced to the area. He states that by 1776 the Ute of Western Colorado were well mounted (1953a:13), leading to a great disruption to the aboriginal cultural form (1953a:16). In essence, he argues that horses permitted political organization through mounted groups that hunted the bison to extinction (1953a:9), predatory raiding bands emerged from this new mode of production that menaced settlers, and a general change in social organization occurred rendering the Ute "qualitatively" changed. The effect of the horse was so great on the Aboriginal peoples of the area that the white settlers, when they arrived in the area afterwards, made a significant mistake in their signing of Treaties with them. 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. (1953a:6)

For Steward, simply, in the face of the existing Treaties, there could not have been chiefs or legitimate signatories to those international covenants because the Ute had no horse aboriginally; moreover, when they did acquire horses they were no longer "aboriginal"—attempting to render their claim invalid in both instances.

He ends his report with a summation of then present-day Ute social organization. He says that because of the "unrestricted nomadism" there was significant movement and, therefore, confusion about membership aboriginally (1953a: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. (1953a:71)

The Paiute Cases and the University of Illinois

After the completion of the hastily organized initial testimony for the Ute cases, Steward left Columbia to a take a research professorship at the University of Illinois (UIUC) in August 1952. His new position offered fewer teaching and administrative duties, and his reduced workload at the university coincided exactly with his increased role for the Department of Justice in preparation for the much larger, and certainly much better organized, cases against the Paiute (Nos. 87, 88, 17, 100) and the combined case, *The Indians of California* (Nos. 31 and 37).[15] In addition to acting as an expert witness and strategist for the Paiute cases, as for the Ute cases, Steward also supervised and heavily edited the work of Erminie Wheeler-Voegelin and had several high-profile research assistants helping him on the cases.[16]

An example of Steward's influence on Wheeler-Voegelin is shown in this extract of a letter, where he explains that it would "simplify" the government's defense if she were to drop her report and adopt his account. 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 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 [Yost] 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. Steward to Yost, Feb 12, 1954 [copied to Erminie Wheeler-Voegelin], JHSP, Box 2)

As well, several of Steward's most prominent students from Columbia were also involved with the Paiute cases, a relationship that Steward initiated and directed throughout the procedure. These students include Elman Service, Robert Murphy, and Robert Manners.[17] His involvement in their work is evident, for example, in this set of correspondence with the Department of Justice to recruit Elman Service to replace Bob Manners as an expert witness:

> Other costs besides [Elman] Service's fee will depend somewhat upon what kind of job you want done. If he follows to some extent the California pro-

cedure, I would suggest a 4' x 6' general distribution map that would cover not only the area conventionally assigned the Southern Paiute but would extend north to Great Salt Lake and east to the Colorado border. I suggest this because in Escalante's time, 1776, there was no difference between the Ute of this area and the Southern Paiute. . . .[18]

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.
(letter to Ralph Barney, December 10, 1955, JHSP, Box 2)

As well, because of the increased demand placed on him he required help "to assemble pertinent data" in the case, he suggested to the Department of Justice that "an advanced graduate student" be added as his assistant. 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" to assist him. Steward also says 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" (Steward to Lee Yost, May 7, 1951, JHSP, Box 2).

> 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 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. (Steward to Lee Yost, May 7, 1951, JHSP, Box 2)

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Unmistakably defining his role, 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."

The Levels of Sociocultural Integration

For the Paiute cases, the Department of Justice framed their legal defense to deny Indian Title on Steward's ethnographic account, theorized as the "levels of sociocultural integration," as a way to represent that the Paiute had no cohesion, leadership, or common identity (1955a[1954]). Steward first began developing the concept of "the levels of sociocultural integration" in the Social Sciences Research Council's (SSRC) Bulletin, *Area Research: Theory and Practice* (1950) soon after accepting his appointment with the Department of Justice.

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 (Steward 1950:7). Recognizing that an "area" could be anything left to the fancy of the scientist, he says, however, the purpose of the study is of 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. 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 (Steward 1950:2; emphasis in original).

In the book, Steward is overly concerned with what he perceives as the plight of anthropology due to its traditional subject disappearing (1950:151). As a remedy, he offers the Puerto Rico Project as the exemplar of a relevant anthropological research project on what he calls "complex and changing societies;" heralding the Project as a strong contribution for the growth of the social sciences (1950:154), and for highlighting anthropology's role in this growth (1950:95). 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 says, that 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" (1950:155).

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" (1950:155). Importantly, he advocates for a paramount position for anthropology to synthesize the analyses of other social science disciplines, such as economics, religion, and political science, creating an interpretive hierarchy and structure for the social sciences with anthropology at the top liaising with the government. Specifically, he says, 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" (1950:154). That is, he explicitly acknowledges that the project was set up to examine the change that occurred in Puerto Rican society under the exertion of U.S. sovereignty in service of the U.S. government.

For undertaking area studies he introduced a new concept, "the levels of sociocultural integration," that would permit anthropology to contribute more fully to a useful social science amidst the perceived 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 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. Steward reiterates this evolutionary teleology in *Theory of Culture Change*, further developing the concept of "the levels of sociocultural integration" and acknowledging it is the "substantive application" of his theoretical and methodological project (1955a [1954]:5,99–121). Here, he contextualizes 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" (1955a[1954] 11). This application combines "multilinear evolution" as an hypothesis that is "based on the assumption that significant regularities in cultural change occur, and it is concerned with the determination of cultural laws" (1955a[1954]:18–19), and "cultural ecology," a "methodology," 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 the subsistence technologies that were adapted to the environment provide the most important cultural features, and the social organization and superstructure arise as epiphenomena of the culture core (1955a[1954]:37).

Approaching the representation of human social organization as if from an objective position, Steward alleged that the bottom level of the

scale of sociocultural integration was known as the "family-level." At this level, he says, the "family was the reproductive, economic, educational, political, and religious unit" (1955a[1954]:54). 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' . . . " (1955a[1954]:28). Though he claims that the evolutionary trajectory of each society is not unilinear, he hypothesizes 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" (1955a[1954]:53–54). Fully invoking a Spencerian biological analogy, he alleges the parallel between his understanding of social organization 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. (1955a[1954]:13)

Paiute Testimony and the Government's Defense

In its defense for the Paiute cases, the U.S. Department of Justice relied entirely on Steward's reports 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 recognized 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 1955a[1954]:102–103).

In a detailed letter to Attorney General Williams, dated July 10, 1953, Steward describes the specifics of what his testimony for the Paiute cases for the Department of Justice will be, outlining his interpretation of the aboriginal social organization of the Paiute and his strategies 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" (Steward to Williams, July 10, 1953, JHSP, Box 2). Though he admits that some areas of the Northern Paiute's territory had an abundance of food and a band-level organization could have emerged there, he surmised that "aboriginally," for the most part scarce resources determined that no chiefs, authority, or social organization 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), he reports that they had a changing leadership of shamans, but no consistent form of authority or office of leadership; and 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 is no chief, and as a result no political consolidation could have formed. Steward 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. 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 organization.[19]

In his testimony, presented to the ICC and published later, without this contextual information in *Theory of Culture Change*, as "The Great Basin Shoshonean Indians," 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. (1955a[1954]:102)

Steward claimed that the Paiute, a "Shoshoean" people, were at the family level of sociocultural integration in his testimony, alleging that pre-contact Shoshone families lived in isolation with no formal ties between groups of families, and few informal ones (1955a[1954]:101–121).

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He likened the family to a net without any social knots to establish connections between groups; a model that, he says, ultimately permitted "liberty" for each individual biological unit (1955a[1954]: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" (1955a[1954]: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" (1955a[1954]: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" (1955a[1954]: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 organization of Indigenous peoples.

Steward's political location unravels entirely when his ICC testimony is contrasted with his original statements concerning Shoshone political organization. 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 organization, at a minimum. In this early paper, written directly after his fieldwork in the Great Basin 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 recognizable group with a degree of central control and common interests (1936:338). 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)

Therein, not only did he say that all peoples live in an organized, rule-based society, but he also notes,

> 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 vegetable foods communally. This provides a kind of subsistence insurance or greater security than individual families could achieve. (1936:332)

It is important to note that although the ICC rejected Steward's testimony and that he "lost" every case where he was an expert witness (Stewart 1985; Lewis 2002), notwithstanding his futility before the courts, Steward's description of the Shoshone remains steadfast in anthropology (e.g., Johnson and Earl 1987; Farb 1968; Haenn and Wilk 2006; Clemmer et al. 1999; Beck 1999) and his approach is represented in the discipline as a scientific and objective method for the comprehension of Indigenous societies, as described by numerous representative authors above.[20]

Finally, it is not that there has been little written about Steward's career.

Indeed, there are numerous biographical accounts of him that serve to reinforce the general disciplinary lacuna regarding colonialism, anthropology, and North America. This misrecognition of Steward's work for the U.S. Department of Justice is codified, for example, in a recent four-hundred-page biography of him, where only four scattered pages in the entire book address his work for the U.S. Department of Justice in the ICC cases, though he performed this work for at least seven years (Kerns 2003:247, 259, 282–283). Moreover, the author specifically 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, helps explain his decision . . . to testify for the federal government in the Indian Claims Commission cases" (Kerns 2003:247).

Kerns's 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* (Sponsel 2006), where space is allotted to absolve Steward's relationship to nineteenth century evolutionary theory, but none is devoted to examination of his work before the ICC. In fact, and tellingly, Sponsel 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 added). The topic of the ICC is also excluded in the biographies of Steward by his student, Robert Murphy (1977, 1981), neither of which mention the ICC or Steward's work with the government. So too is the topic overlooked in Steward's extensive obituary, published in the *American Anthropologist* and written by another of his students, Robert Manners (1973). All of these omissions are significant. First, because of the vast amount of time Steward spent in his relationship with the Department of Justice, that employment reasonably should be included in any biography. Second, because the volumes of academic material that Steward published while he was working for the Department of Justice had a direct and sometimes verbatim relationship to his testimony in claims cases. Third, and incredibly, because both Robert Murphy and Robert Manners, the authors of three of the biographies, worked for and appeared on behalf of the U.S. government in ICC proceedings at Steward's behest and under his direction.

Conclusion

This paper is part of a larger project in which we are examining the relationship between anthropology and processes of colonialism in the United States and Canada. Contrary to the standard references on him, Julian Steward played a crucial role in the U.S. 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 this proposition, 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 U.S. 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 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.

Through this examination of Steward's work, this paper is offered as a contribution to the critical analysis we are undertaking 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 in this paper, I ask the discipline to consider where it positions itself in this struggle when it valorizes 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.

Notes

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In citing works in the text the following abbreviation has been used:

JHSP Julian H. Steward Papers (Record Series 15/2/21), University of Illinois Archives

1. I am accepting the *Oxford English Dictionary*'s definition of colonialism: "1. a policy of acquiring or maintaining colonies." I understand fully that the term has wider implications that will be very important for the discipline to address. They are beyond this scope of this simple demonstration of Julian Steward and American anthropology.

2. Undoubtedly, attributing the consciousness of colonized peoples to coinciding with the colonizer's acknowledgment of the relationship is incredibly naïve.

3. 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.

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

5. The embracement of the concept of "levels of sociocultural integration" by some anthropologists well depicts the belief that Steward's work was objective. For example, Harris writes in *The Rise of Anthropological Theory*, that "[c]omparison of culture areas from the point of view of levels of intensity can be achieved with greater objectivity and theoretical significance by employing Steward's concept of 'Levels of socio-cultural integration'" (1968:341). Furthermore, Wolf identifies Steward's "levels of sociocultural integration" as one of the guiding concepts of the famous Puerto Rico Project, explaining that it "is an extremely simple concept, and at the same time it is a concept which—in all its simplicity—recommends itself then because it allowed recognition of a certain measure of complexity in society" (Wolf 2001:42).

6. One could also easily follow Crum (1999) and his demonstration of Steward's promotion of assimilation as a Federal Indian policy early in his career, and expose the connection of his theory to nineteenth-century evolutionary theory, and in particular the connection to biological evolution of Herbert Spencer (e.g., Steward 1950), to expose another of Steward's techniques of colonialism.

7. Steward also had a strong influence on Marvin Harris and Marshall Sahlins, and on the representation of Eleanor Leacock within the literature.

8. Chapter 1, "Multilinear Evolution," had been previously published as "Evolution and Process" in Kroeber's *Anthropology Today* (1952).

9. 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" (2006:3). To reinforce Steward's place within their text, chapter 1 is a reproduction of Steward's "The Concept and Method of Cultural Ecology."

10. I am accepting the U.S. government's "Historical Chronology" of U.S. 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*

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(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 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; and (6) *Termination (1944–58)*, "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." (United States Department of Energy, n.d.)

11. It should be noted, however, that in establishing its original colonial law, the U.S. courts did not follow a doctrine of *terra nullius*, but instead relied on the doctrine of discovery (Wilkins 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 Slattery (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" (Asch 2002).

12. A copy of the report with handwritten annotations appears in Steward's archive (Vanech to Steward, April 11, 1950 [handwritten annotations, April 16, 1950], JHSP Box 2); a published version of the report has been published as Steward 1974.

13. A topic specifically taken up by Fried (1975).

14. A line followed by interpretivists presently (e.g., Nadasdy 2003).

15. 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 as expert witnesses.

16. C.f. Ethnohistory 1(1), 2(1); JHSP, Box 2, Jay Jones to Steward, August 21, 1953. This was at the precise time Wheeler-Voegelin and Jay Jones founded the American Society for Ethnohistory.

17. Eric Wolf was also Steward's research associate for three years at UIUC (1953–55), but did not work on the ICC material. Clearly he was aware of this relationship, however. This is evidenced in his interview with Friedman (1987).

18. The significance of this argument is that if the Ute and the Southern Paiute

were found to have an overlapping claim, original Indian title would be impossible to determine.

19. 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, an 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, JHSP, 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
> (May 27, 1957, JHSP, Box 1)

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

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