WHY A PUBLIC ANTHROPOLOGY?
NOTES AND REFERENCES

While the placement of notes and references online is still relatively rare, they are quite easy to examine in a PDF online format. Readers should use find (or control F) with a fragment of the quote to discover the cited reference. In a number of cases, I have added additional material so advanced readers, if they wish, can explore the issue in greater depth. Find can also be used to track down relevant citations, explore a topic of interest, or locate a particular section (e.g. Chapter 2 or 3.4).

May I make a request? Since this Notes and References section contains over 1,300 references, it is likely that, despite my best efforts, there are still incomplete references and errors. I would appreciate readers pointing them out to me, especially since I can, in most cases, readily correct them. Thank you. Readers can email me at Borofsky@publicanthropology.org.

Quotes from Jim Yong Kim: On Leadership, Washington Post (April 1, 2010, listed as March 31, 2010)
http://www.washingtonpost.com/wp-dyn/content/video/2010/03/31/VI2010033100606.html?hpid=smartliving

CHAPTER 1

1.1

Bronislaw Malinowski, a prominent early twentieth century anthropologist, famously stated his goal in anthropology was "to grasp the native's point of view, his relation to life, to realize his vision of his world" (1961 [1922]:25) To do this, he lived as a participant-observer for roughly two years – between 1915-1918 – among the Trobriand Islanders of Papua New Guinea (in the South Pacific). Quoting him (1961[1922]:7-8):

There is all the difference between a sporadic plunging into the company of natives, and being really in contact with them. What does this latter mean? . . . It means that his life in the village, which at first is a strange, sometimes unpleasant, sometimes intensely interesting adventure soon adopts quite a natural course very much in harmony with his surroundings. Soon after I had
established myself in [the village of] Omarakana (Trobriand Islands), I began to take part, in a way, in the village life, to look forward to the important or festive events, to take personal interest in the gossip and the developments of the small village occurrences; to wake up every morning to a day, presenting itself to me more or less as it does to the native.

I ceased to be a disturbing element in the tribal life . . . altering it by my very approach, as always happens with a new-comer to every [such] . . . community. In fact, as they knew that I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished by regarding me as part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco. . . . Whatever happened was within easy reach, and there was no possibility of its escaping my notice. . . .

really important quarrels and rifts within the community, cases of illness, attempted cures and deaths, magical rites . . . all these I had not to pursue, fearful of missing them, but they took place under my very eyes, at my own doorstep, so to speak.

It clarifies, to quote Clifford Geertz again, a sense of "what goes on in such places . . . what manner of [people] are these?" (Geertz 1973:16).

In relation to contextual analysis, Alfred Kroeber writes that Franz Boas insisted "that phenomena can properly be dealt with only in their adhering context" (Kroeber 1943:6)

Examples from Making History:

As Molingi was trying to make a particular string figure (waiwai), Nimeti, her husband, jokingly criticized her efforts. When she failed to do it right the first time and had to try over again, he turned to me and stated she did not know how to make such things. Here was the proof; she could not do a string figure. Molingi appeared to ignore his comments. She seemed absorbed in trying to work out where she had gone wrong in making the figure. Again Nimeti criticized her efforts. Finally Molingi turned to him and stated that he was getting senile. (Both of them are in their seventies.) Didn't Nimeti recognize, she rhetorically asked, that she was an expert on traditional matters?

As a result of Molingi’s comment, Nimeti picked up a string and started making a figure himself. Molingi scoffed at his efforts, My daughter, Amelia, came by and asked Nimeti what he was doing. He proudly showed her his figure. Molingi criticized Nimeti’s string figure as something any child could do. Finally Molingi finished her figure and showed it to me. She pointedly noted that Nimeti did not know how to make one like hers. Nimeti laughed at the implied challenge and began work on a different string figure. Here was another one, he commented, that Molingi did not know (Borofsky 1987:78)

One day, after gathering some poles in Loto’s public reserve for building the roof of my cook house, I stopped at a pule guard house to rest and talk with two of the
guards. They were both women, one in her late thirties and the other in her late twenties. One thing led to another and we started discussing whether it was the legendary figure Waletiale or Malangaatiale who possessed an enlarged penis. Both of them asserted that it was Malangaatiale. They admitted uncertainty as to exactly who Waletiale was, but basically felt that he was another character entirely. I, on the other hand, asserted that Waletiale possessed the enlarged penis and that the legend of Malangaatiale concerned a man struck by lightning.

We discussed our differences of opinion for a while without coming to any agreement. Then the younger of the two women asked me how I knew my version of the two legends was correct. I replied that this was what several old people, especially Petelo and Molingi, had told me.

As I listened to them, they again discussed the whole issue between themselves. What I had said did not really seem right to them, but then they themselves, they admitted, were not that sure of either legend. Finally, they decided that I might indeed be right. Unlike them, I had discussed the issue with Petelo and Molingi, both recognized experts on Pukapukan legends (Borofsky 1987:111)

The first involves British anthropologist S. F. Nadel's study of four African societies regarding the dynamics of witchcraft (Nadel 1952).

In the second example, Eric Wolf used historical material to compare responses to colonization in Meso-America and Central Java (Wolf 1957).

NOTE: The comparative method exemplified by Nadel and Wolf is less used than it once was. Nader and Holy comment:

“The sheer number of comparative articles and books published in that period are reminders that energetic debates about the intellectual place of comparison are missing from today's anthropological agendas” (Nader 1994:85).

Holy (1987:8, 13), in a book entitled *Comparative Anthropology*, observes "these days, a great proportion of empirical research is distinctly non-comparative" and "comparisons aimed specifically at generating cross-culturally valid generalizations seem to be conspicuous by their absence" (Holy 1987:8, 13).

1.2

One might cite a number of examples of anthropologists serving the broader common good: Nancy Scheper-Hughes disclosing how the international buying and selling of kidneys operates (see e.g. Organs Watch, [http://www.threemonkeysonline.com/als/ organ Trafficking interview nancy schepper-Hughes.html](http://www.threemonkeysonline.com/als/organ Trafficking interview nancy schepper-Hughes.html), [http://en.wikipedia.org/wiki/Nancy_Scheper-Hughes](http://en.wikipedia.org/wiki/Nancy_Scheper-Hughes)) Carolyn Nordstrom (2004; 2007) illuminating the illegal networks that perpetuate conflicts; Alex Hinton’s (2004) explaining the killing fields of Cambodia; or Harri Englund (2006) describing why many human rights NGO’s (non-governmental organizations) are often less effective in the field than they might be in the field.
**Regarding Franz Boas:**

“As Nazism swept across Germany, he graced the cover of Time Magazine in May 1936. The article referred to Boas’s *The Mind of Primitive Man* as “the Magna Carta of self-respect” for non-Western peoples” (Baker 2004:42).

Based on years of study, Boas emphasized that “physiological, mental and social functions are highly variable, being dependent upon external conditions so that an intimate relation between race and culture does not seem plausible” (Boas 1938:145).

Many of Boas’ writings in the New York Times, Nation, and Dial are collected in (Boas 1945).

“He was the catalyst behind a 1938 ‘Scientists’ Manifesto’ opposing a connection between race and intelligence signed by 1284 scientists from 167 universities” (Baker 2004:43).

NOTE: Boas was also involved with the NAACP, and wrote the lead article for its journal second issue. (Lewis 2001:455). He publicly challenged Columbia University’s president and trustees when they sought to fire a faculty member for opposing American entry into World War I (Lewis 2001:457).

**Regarding Margaret Mead:**

There were tributes at her death in 1978 not only from the President of the United States, Jimmy Carter, but the Secretary-General of the United Nations, Kurt Waldheim (Whitman 1978).

In 1979 she was posthumously awarded the United States’ highest civilian honor, the Presidential Medal of Freedom (Ciano 2001).

Mead “brought the serious work of anthropology into the public consciousness . . . A deeply committed activist, Mead often testified on social issues before the United States Congress and other government agencies” (History, American Museum of Natural).

Her intellectual output was staggering. She wrote 44 books and more than a thousand articles. She was a monthly columnist for the popular magazine *Redbook* from 1961 until 1978. She reputedly gave up to 110 lectures a year (Mace 1999).

**Regarding Paul Farmer:**

Paul Farmer was interviewed on CBS’s *Sixty Minutes* (Sixty 2008).

The New York Times reports, “if any one person can be given credit for transforming the medical establishment’s thinking about health care for the destitute, it is Paul Farmer” (Cohen 2003). Working through Partners in Health, he and others have been
able to lower the price of drugs for the sick in Third World countries as well as change the World Health Organization’s guidelines for treating the poor. The New York Times article continues, “Dr. Farmer and his partners in health have shown that a small group of committed individuals . . . can change the world.”

Quoting from the 2008 Director’s Statement on the Partners in Health Website (Dahl 2008): “we build on the strengths of the communities by working within public health systems and serving where there are gaps, and . . . we invest directly in the communities we serve by training and employing a cadre of local community health workers to accompany our patients and their families through their care.”


1.3

Let me present four quotations from Furner’s book so that she herself can explain what transpired.

(1) Establishing scientific authority [in the 1870s and 1880s] was . . . difficult for . . . [amateur social scientists] because many of them were publicly connected with controversial political positions. No matter how hard preacademic social scientists tried to change their image . . . anyone who resented their findings . . . could easily cast doubt on their objectivity by hurling the reliable epithet, “reformer (Furner 1975:3-4).

(2) By the end of the 1890s professional status and security competed with ideological . . . considerations as values [for social scientists] . . . . Direct appeal to the public . . . was retained as a theoretical right but . . . [social scientists] were expected to channel most of their reform efforts through government agencies or private organizations where scholars could serve inconspicuously as technical experts, after the political decisions had been made, rather than as reformers with a new vision of society. . . . Rarely did academic [social scientists] . . . deal publicly with the controversial normative questions that had preoccupied the emerging profession[s] in the 1880s (Furner 1975:259).

(3) Objectivity . . . [became] part of . . . [an] emerging professional identity, but . . . [university] leaders defined it in a special way. It restricted open public advocacy of the sort that allied . . . [social scientists] with reforms which threatened the status quo (Furner 1975:290-291).

(4) The tension between advocacy and objectivity which characterized the professionalization process altered the mission of social science. Only rarely [as the twentieth century proceeded] did professional social scientists do what no
one else was better qualified to do: bring expert skill and knowledge to bear on cosmic questions pertaining to the society as a whole. Instead, studies and findings tended to be internal, recommendations hedged with qualifiers, analyses couched in jargon that was unintelligible to the average citizen. A fundamental conservatism developed in the academic social science professionals . . . The academic professionals, having retreated to the security of technical expertise, left to journalists and politicians the original mission – the comprehensive assessment of industrial society – that had fostered the professionalization of social sciences [in the first place] (Furner 1975:324).

Regarding the trial of Richard Ely (Furner 1975:147-58). Ely, as Furner remarks, “was more active than anyone else [in economics] in taking his findings directly to the people and advocating specific reforms” (Furner 1975:147).

Marcia Angell, the former editor of the New England Journal of Medicine, writes “the idea that breast augmentation caused connective tissue disease has a superficial plausibility... Several good epidemiological studies have failed to show an association between implants and a host of connective tissue diseases, symptoms, and abnormal test results”(1996:104, 195-196).

For material relating to cold fusion, refer to: Economist 2009b; Lewenstein and Baur 1991; Wikipedia n.d.c).

Regarding Robert Redfield and Oscar Lewis, refer to: Lewis 1951; Lewis 1960; Redfield 1930; Redfield 1956.


Readers interested in further exploring topics covered in 1.3 may wish to peruse:

1.4

The famous American anthropologist Clifford Geertz observed in 1985: "One of the advantages of anthropology as a scholarly enterprise is that no one, including its practitioners, quite knows exactly what it is." People who watch baboons copulate, people who rewrite myths in algebraic formulas, people who dig up Pleistocene skeletons, people who work out decimal point correlations between toilet training practices and theories of disease, people who decode Maya hieroglyphics, and people who classify kinship systems into typologies in which our own comes out as "Eskimo", all call themselves anthropologists."
An important result of this breadth, he continued, "is a permanent identity crisis" (Geertz 1985:623).

"The result of anthropology's eclecticism is that the field continues to astound by its diverse and colorful activity" (Wolf 1980:20).

"Clyde Kluckhohn once said about anthropology -- that it's an intellectual poaching license" (Geertz 1983:21).

"Nondisciplinary intellectuals have difficulty reproducing themselves because the American open market for public intellectuals is incapable of supporting more than a tiny handful of nonacademic writers and has no organized means of reproduction and exchange beyond some tenuous referral networks. Academia is, to all intents and purposes, the only practical recourse for American intellectuals. And being an academic means willy-nilly, being a member of a discipline, there have indeed been great interdisciplinary geniuses, even within academia; Gregory Bateson is an obvious example. But they have no obvious mode of reproduction. They simply arise, revolutionize two or three disciplines and leave magical memories behind" (Abbott 2001:130).

“APPLIED ANTHROPOLOGY is designed not only for scientists, but even more for those concerned with putting plans into operation, administrators, psychiatrists, social workers, and all those who are as part of their responsibility have to take action in problems of human relations” (Applied 1941:2).

== On Defining Who Is and Is Not an Anthropologist:

One cannot use membership in the American Anthropological Association – the world’s largest professional association of anthropologists – to determine who is and is not an anthropologist. The association’s guide to anthropology departments indicates that only about 40 percent of the individuals teaching in American anthropology departments belong to the Association. That means that if we use American Anthropological Association membership as a measure of who is (and is not) an anthropologist, we have to exclude over half those teaching anthropology in academic departments in the United States.

Readers interested in further exploring topics covered in 1.4 may wish to peruse: Barnett 1940; Boettke 2002; Bulmer 2001; Chaney 1993; Collins 2002; Commission 1996; Eriksen 1992; Glenn 2002; Glenn 2008a; Lamont 2009; Mann 1993; Mills 2008; Petrie 2007; Pieters and Baumgartner 2002; Rensburger 1996; Sica 2001; Smith 1993; Taylor 1993; Wallerstein 2001; Wikipedia n.d.a.

1.5

You see reference to Boas as the “father of American anthropology” in textbooks, in various disciplinary journals, book advertisements, even in Wikipedia. For example, see: Cole 1995; helpme.com n.d.; Holloway 1997; Hyatt 1990; Paredes 2006;
Schneider n.d; Williams 1991; Wikipedia n.d.e. An obituary for Franz Boas makes this point: Boas “found anthropology a collection of wild guesses and a happy hunting ground for the romantic lover of primitive things; he left it a discipline in which theories could tested and in which he had delimited possibilities from impossibilities” (Lowie 1947:311). NOTE: Lowie indicates Boas would have disagreed with the above statement -- Boas appreciated earlier contributors to the discipline.

The nineteenth century had a number of prominent anthropologists that were not trained in university settings; they were mostly self-taught. These included important theorists such as Lewis Henry Morgan; significant ethnographers such as James Mooney, Frank Hamilton Cushing (http://en.wikipedia.org/wiki/Frank_Hamilton_Cushing) and Henry Rowe Schoolcraft (http://en.wikipedia.org/wiki/Henry_Schoolcraft). Lewis Henry Morgan, a lawyer turned anthropologist, has been described as one of “the most important social scientists in nineteenth–century America” (Fenton in Morgan1962:viii). Lewis Henry Morgan’s *League of the Iroquois*, (Morgan 1962) was a precedent–setting ethnography, as was James Mooney’s *The Ghost-Dance Religion and Wounded Knee* (Mooney 1965 [1896]). Morgan’s book was first published in 1851 under the title *League of the Ho-de-no-sau-nee, Iroquois*. Mooney’s was originally published in 1896 as Part 2 of the Fourteenth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1892-93. Washington: Government Printing Office. Ewers refers to Morgan as the “Father of American anthropology” (Ewers 1960:703).


Alfred Kroeber, the foremost anthropologist of the post-World War II period, divided anthropology into race, language, culture, psychology, and prehistory (Kroeber 1948).

Ralph Linton wrote, “the two great divisions of anthropology . . . are known as physical anthropology and cultural anthropology” (Linton 1945:5). Cultural anthropology he divided into archeology, ethnology, and linguistics. After noting that linguistics was “the most isolated and self-contained” of anthropology’s “subsciences” he dropped further discussion of it (Linton 1945:7).

Carol and Melvin Ember and Peter Peregrine write in the twelfth edition of Anthropology (Ember, et al. 2007:3): “defining anthropology as the study of human beings [as Kroeber did] would appear to incorporate a whole catalog of disciplines: sociology, psychology, political science, economics, history, human biology, and perhaps even the humanistic disciplines of philosophy and literature." Seeking to describe what makes anthropology distinct, they suggest: “Anthropology is concerned explicitly and directly with all the varieties of people throughout the world, not just those close at hand or within a limited area. It is also interested in people of all periods." They divide anthropology into two broad classifications: biological (or physical) anthropology and cultural anthropology. Cultural anthropology is further
divided into archaeology, linguistics, and ethnology. “Cross-cutting” these four fields is a fifth field, applied anthropology.

Linda Colley, “The inhabitants of Wales, of Scotland, and of England were separated from each other . . . [by] different folklores, different sports, different costumes, different building styles, different agricultural practices, different weights and measures, and different cuisines” (Colley 1992:13-14).

Erika Bourguignon states: “Before Boas and for Boas himself, the essence and primary task of American Anthropology was the study of American Indians . . . How can questions of the origins and the true nature of Native Americans be answered?” she asks. “By the study of the physical types of the people, their archaeological remains, their languages, and their customs - the four fields of anthropology” (Bourguignon 1996:7).

As George Stocking observed, “any movement in ethnology [or sociocultural anthropology] away from historical reconstruction could not help but have implications for the unity of anthropology” (Stocking 1976:24).

The anthropologist Eric Wolf expresses this myth in an often cited introduction to the field: “In contrast to the anthropological traditions of other countries, anthropology in the United States always prided itself upon its role as the unified and unifying study of several subdisciplines. In combining the pursuits of human biology, linguistics, prehistory, and ethnology, American anthropology put a premium on intellectual synthesis, upon the tracing out of connections where others saw only divergence” (Wolf 1974:x).

However, if we examine the 3,252 articles published in the American Anthropologist, the discipline’s flagship journal, from 1899 to 1998, perhaps only 308 substantially draw on more than one anthropological subfield in the analysis of their data. That is to say, over a hundred-year period, only perhaps 9.5 percent of the articles published in the American Anthropologist bring the discipline’s subfields together in any significant way. Most of the articles focus on narrow subjects and use the perspectives and tools of only one subfield. The 3,252 articles are narrowly framed and narrowly presented, with relatively little synthesis across subfields—just like today. Up until the 1970s, the total number of collaborative subfield articles decade by decade in the American Anthropologist was always lower than the 9.5 percent figure cited. Only eight times in the last hundred years has the number of collaborative articles—across subfields—reached at least 20 percent of the total articles published in the journal— in 1974, 1975, 1979, 1980, 1981, 1984, 1986, and 1989. Early anthropologists often published articles in more than one subfield. But the critical point is that they rarely brought the subfields together in the same article, using different sub-22 field perspectives to provide a broader synthesis. In terms of the American Anthropologist from 1899 to 1998, collaboration across the subfields was a distinctly minority affair (Borofsky 2002).
The contrast between what presently is and what could be in cultural anthropology is not simply a tale (to borrow from Shakespeare) full of sound and fury, signifying little. Serious issues are involved. Paraphrasing Shakespeare: Macbeth, Act 5, Scene 5.

Readers interested in further exploring topics covered in 1.5 may wish to peruse: Economist 2005c; Leerssen 2007; Powell 1888; Rogge 1976.

1.6

The Hippocratic dictum "As to disease make a habit of two things—to help, or at least, to do no harm" (Epidemics 1. 11).

The famous French anthropologist Claude Lévi-Strauss observes, "It is an historical fact that anthropology was born and developed in the shadow of colonialism." He then adds, "However, contrary to the colonial enterprise, anthropologists have sought to protect beliefs and ways of life that cultures were forgetting at an increasing rate" (Levi-Strauss 1994:425).

Talal Asad states, "It is not a matter of dispute that social anthropology emerged as a distinctive discipline at the beginning of the colonial era, that it became a flourishing academic profession towards its close, or that throughout this period its efforts were devoted to a description and analysis – carried out by Europeans, for a European audience – of non-European societies dominated by European power" (1973:14-15). Anthropology is, he continues, "rooted in an unequal power encounter . . . that gives the West access to cultural and historical information about the societies it has progressively dominated" (16-17).

The anthropologist James Clifford observes that while colonial domination framed most anthropological accounts of times past, anthropologists "adopted a range of liberal positions within it. Seldom 'colonists' in any direct instrumental sense, ethnographers accepted certain constraints while, in varying degrees, questioning them" (1983:142).

The 1998 Anthropological Association statement on ethics asserts that "anthropological researchers must do everything in their power to ensure that their research does not harm the safety, dignity, or privacy of the people with whom they work, conduct research, or perform other professional activities" American Anthropological Association 1998:III, A.2).

The back cover of the 1987 paperback edition of Turnbull's book explains: "In The Mountain People, Colin M. Turnbull . . . describes the dehumanization of the Ik, African tribesmen who in less than three generations have deteriorated from being once-prosperous hunters to scattered bands of hostile, starving people whose only goal is individual survival. . . . Drought and starvation have made them a strange, heartless people, . . . their days occupied with constant competition and the search for food."
How does one respond to a situation such as this? The philosopher Kwame Anthony Appiah (2000:58) ponders why "the former general secretary of Racial Unity [i.e., Colin Turnbull] had done so little to intervene? Why had he not handed over more of his own rations? Taken more children to the clinic in his Land Rover? Gone to the government authorities and told them that they needed to allow the Ik back into their hunting grounds or give them more food?"

As Turnbull himself explains, he took a group-dictated letter to government authorities at Moroto regarding the Ik's plight. "I delivered the letter and a report of my own, without much conviction that either would carry any weight" (1987:109). And when they apparently did not, he went off to the capital, Kampala, to stock up with fresh supplies for himself. That was it: no insistence, no pleading, no seeking to bring pressure on local authorities with letters to those higher up in the government, no public exposé with the hope of helping the Ik (see also Grinker 2000:166). What Turnbull did in his book, instead, is offer a general reflection on the state of humanity: "Most of us are unlikely to admit readily that we can sink as low as the Ik, but many of us do, and with far less cause. . . . Although the experience was far from pleasant, and involved both physical and mental suffering, I am grateful for it. In spite of it all . . . the Ik teach us that our much vaunted human values are not inherent in humanity at all, but are associated only with a particular form of survival called society, and that all, even society itself, are luxuries that can be dispensed with" (1987:12, 294; see also Grinker 2000:156, 163).

1.7

"The fact that the Yanomamö live in a state of chronic warfare," Napoleon Chagnon writes, "is reflected in their mythology, values, settlement pattern, political behavior and marriage practices" (1968:3). He continues: "Although their technology is primitive, it permits them to exploit their jungle habitat sufficiently well to provide them with the wherewithal of physical comfort. The nature of their economy – agriculture – with the fact that they have chronic warfare, results in a distinctive settlement pattern and system of alliances that permits groups of people to exploit a given area over a relatively long period of time. . . . The Yanomamö explain the nature of man's ferocity . . . in myth and legend" (1968:52-53).

The French anthropologist Jacques Lizot, in Tales of the Yanomami, writes: "I would like my book to help revise the exaggerated representation that has been given of Yanomami violence. The Yanomami are warriors; they can be brutal and cruel, but they can also be delicate, sensitive, and loving. Violence is only sporadic; it never dominates social life for any length of time, and long peaceful moments can separate two explosions. When one is acquainted with the societies of the North American plains or the societies of the Chaco in South America, one cannot say that Yanomami culture is organized around warfare as Chagnon does" (1985:xiv-xv).

On one side of these issues is Bruce Albert, an anthropologist who has spent decades working with the Yanomami:
Nobody maintains that the Yanomami do not practice warfare or that Yanomami individuals are not occasionally violent (true for most societies, including the United States). . . . But many people do maintain that it is unethical and politically damaging to reduce the richness of Yanomami society and culture to the stereotypical image of "the barbaric violence [that] Chagnon documented" [in the words of a 1995 Time magazine article]. . . . It requires only a minimal ethical sensibility and political awareness to understand that such long-term pejorative labeling and its apparent scientific authority can be (and have been) used by anti-Indian agitators to rationalize and encourage violations of Yanomami rights – nobody ever said such labeling caused them. . . . We need to ask why Napoleon Chagnon never publicly came out [in Brazil] to condemn the use of his work by sensationalist journalists and unscrupulous politicians, or to support the international movement in defense of Yanomami survival (in Borofsky 2005:81).

On the other side is Ray Hames, a student of Napoleon Chagnon who has also worked with the Yanomami:

No matter what precautions ethnographers take to qualify or even sanitize their ethnographic accounts of indigenous populations, ethnographic accounts can always be used against them. At the same time, I would emphasize that such accounts are insignificant explanations of why governments and other powerful interests seek to destroy indigenous peoples. . . . [The] belief that government officials are swayed by ethnographic reports rests on a number of assumptions that I believe are faulty. It . . . assumes that generals and others not only read scientific reports on indigenous peoples but such that such reports affect their decision-making processes. By implication it means that if the Yanomamö were described as peaceful, then military and economic interests would be inhibited from taking indigenous land because they could not rationalize control, partitioning, or seizure of Yanomamö land. . . . What is completely ignored by those who criticize Chagnon's alleged lack of interest in what the press has to say about the Yanomamö is the way in which he has utilized the press to portray the plight of the Yanomamö. . . . Given the enormous readership of his ethnography, my best guess is that his writings have done more to reach the educated public about the serious problems faced by the Yanomamö than those by any other individual or organization (Borofsky 2005:81-82).

1.8
For information on CCPY (Comissão Pró Yanomami) and HAY (Hutukara Associação Yanomamí), please refer to: [http://www.proyanomami.org.br/](http://www.proyanomami.org.br/)

Ray Hames: “For a year before Neel's arrival [Neel was the researcher who organized the collection of blood samples] and during the collection phase he [Chagnon] told the Yanomamö [or Yanomami] in all the villages to be sampled that Neel's team wanted to examine their blood in order to determine whether there were
things that indicated whether or not they had certain kinds of diseases, especially shawara (epidemic diseases) and that this knowledge would help treat them more effectively if they became ill" (Borofsky 2005:77).

Bruce Albert: "To this day, I still do not see how his [Neel's] blood sampling or research significantly helped the Yanomami in treating their epidemic diseases, as they were promised if they agreed to let their blood be drawn (a promise that, in their eyes, was reinforced by the delivery of trade goods). The Venezuelan and Brazilian Yanomami have kept on dying in the same way for three decades after Neel's project" (Borofsky 2005: 88).

The Yanomami clearly want their relatives' blood samples back. Davi Kopenawa, a prominent Yanomami activist, asserts the Yanomami provided the blood samples because they were told that the findings would help not only the researchers but also the Yanomami. He states: "We want to know the findings. What did they find in the blood – information regarding disease?" The Yanomami José Seripino makes the same point: "I was only ten years old. I thought 'Okay. This will help us.' But what happened? We haven't seen the outcome'." The Yanomami Julio Wichato observes, "The problem is that they studied it [the blood] and didn't send us the results. If they help us it's different. . . . It's important that they send the results." The Yanomami Ivanildo Wawanawetery asserts that the researcher "created fear . . . when he [a particular Yanomami] didn't give up his blood, the guy was going to get sick, right? If he didn't give blood the guy was going to get sick, he was going to die. Those who were donating blood would live" (see Borofsky 2005:63-65, the quotes used are from interviews, see 2005:343-344).

Kopenawa notes, "The American didn't help to explain . . . 'Look, this blood is going to stay many years.' He didn’t say that. . . . The Yanomami were thinking that he would take the blood and then read it and then throw it away. That’s what the Yanomami thought. That’s why they gave the blood. . . . They thought it was to see some disease, malaria, tuberculosis, flu, or some other disease."

Davi Kopenawa comments, “My mother gave blood. Now my mother is dead. Her blood is over there [in the United States]. Whatever is of the dead must be destroyed. Our custom is that when the Yanomami die, we destroy everything. To keep it, in a freezer, is not a good thing.” Toto Yanomami states that the doctors “collected these things: blood, urine [inaudible], saliva, and feces. I want it to come back to the Yanomami. . . . I want all of it returned. . . . Blood is important in shamanism. . . . All the blood of the Yanomami belongs to [the deity] Omami. . . . Those people have died! . . . Yanomami never take blood to keep. Yanomami don’t . . . take blood to study and later keep [it] in the refrigerator. . . . The doctors have already examined this blood [for forty years]; they’ve already researched this blood. Doctors already took from this blood that which is good—for their children, for the future. . . . So we want to take all of this Yanomami blood that’s left over." (in Borofsky 2005:63-65).

For a copy of the letters by Dr. Joseph Fraumeni, Director of the Division of Cancer Epidemiology and Genetics at the National Cancer Institute and by Dr. Rodney Erickson, Provost of Pennsylvania State University, please refer to:
We might add that a teacher at Binghamton University quietly allowed a graduate student to unfreeze some of the blood samples for certain research. Again, there were no complications (see http://www.publicanthropology.org/WaPA/Broadrose-10Aug.pdf).

As various Yanomami made clear (you can see them say this on YouTube at http://www.youtube.com/watch?v=7608Vu-D_9U), they planned to ceremonially pour the blood samples into the river.

Certain details still need to be finalized. The Brazilian authorities still have to indicate how they want the blood samples shipped back to the Yanomami. But the return of the samples has been formally announced in the media. The journal Science has reported “Researchers to Return Blood Samples to the Yanomamô” (4 June 2010) – see Couzin-Frankel 2010. The British newspaper The Independent entitled a recent article on the subject “The tribe that won its blood back” (May 25, 2010) – see Adams 2010. And the Brazilian newspaper Folha de S. Paulo notes “The Yanomami leader Davi Kopenawa said he was ‘very pleased’ with the news that more than 2,000 blood samples from his people, who since 1967 lie in research centers in the United States, shall be returned to the tribe” (12 May, 2010) – see Da Redação 2010.

CHAPTER 2

2.1

In a New York Times Bestseller, *The End of Poverty*, the well-known scholar-activist Jeffrey Sachs challenges us to end third-world poverty in “our time.” Sachs suggests the goal is possible given the resources at our command: “The wealth of the rich world, the power of today’s vast storehouses of knowledge, and the declining fraction of the world that needs help to escape from poverty all make the end of poverty a realistic possibility by the year 2025” (Sachs 2005:3). Ending global poverty, Sachs writes, “will require concerted actions by the rich countries as well as the poor . . . The poor countries must take ending poverty seriously, and will have to devote a greater share of their national resources to cutting poverty rather than to war, corruption, and political infighting. The rich countries will need to move beyond the platitudes of helping the poor, and follow through on their repeated promises to deliver more help. All this is possible. Indeed, it is much more likely than it seems” (2005:266). He calls for “networks of mutual accountability . . . [to] run alongside the networks of financing” (Sachs 2005:243).

Sachs notes that the types of interventions he is suggesting have a proven track record. He names “The Green Revolution in Asia,” “The Eradication of Smallpox,”

The solution is in hand, he writes. It just needs to be applied on a broad, regional scale. Poverty ends when:

foreign help, in the form of official development assistance (ODA), helps to jump-start the process of capital accumulation, economic growth, and rising household incomes. The foreign aid feeds into three channels. A little bit goes directly to households, mainly for humanitarian emergencies such as food aid in the midst of a drought. Much more goes directly to the budget to finance public investments, and some is also directed to private businesses (for example, farmers) through microfinance programs and other schemes in which external assistance directly finances private small businesses and farm improvements. If the foreign assistance is substantial enough, the capital stock rises sufficiently to lift households above subsistence. At that point, the poverty trap is broken . . . Growth becomes self-sustaining (Sachs 2005:246).

A number of reviews praise Sachs’s courage and commitment. “Sachs writes as passionately as he speaks,” a BusinessWeek reviewer notes. “The End of Poverty is superb when describing the dire circumstances of the 1 billion people subsisting on less than a dollar a day. It is hard not to share Sachs’ anger after reading his firsthand reporting on the miserly Western aid to African villages ravaged by AIDS, malaria, and hunger. At relatively little expense, Sachs insists, the West could provide medicines and fertilizers that could save millions of lives annually” (BusinessWeek 2005a).

William Easterly, in a Washington Post review states:

Sachs pays surprisingly little attention to the history of aid approaches and results. He seems unaware that his . . . plan is strikingly similar to the early ideas that inspired foreign aid in the 1950s and '60s. Just like Sachs, development planners then identified countries caught in a "poverty trap," did an assessment of how much they would need to make a "big push" out of poverty and into growth, and called upon foreign aid to fill the "financing gap" between countries' own resources and needs. . . . Spending $2.3 trillion (measured in today's dollars) in aid over the past five decades has left the most aid-intensive regions, like Africa, wallowing in continued stagnation; it's fair to say this approach has not been a great success” (Easterly 2005).

Daniel Drezner, in the New York Times review, states:

“Longtime experts in the field who read this book may feel a strong sense of déjà vu. They should. Much of Sachs's argument can be summed up in this passage from Walt W. Rostow's book The Stages of Economic Growth, written in 1960:
"The creation of the preconditions for takeoff was largely a matter of building social overhead capital -- railways, ports and roads -- and of finding an economic setting in which a shift from agriculture and trade to manufacture was profitable." Sachs neglects to mention the extent to which the Rostow model dominated discussions of development in the 1950's, 60's and 70's. But in that era, waste and corruption fattened up United Nations agencies and recipient governments while doing very little for the poor. . . . Sachs’s sales pitch has been made in the past, and the results were meager" (Drezner 2005).

John Cassidy in the *New Yorker* observes:

Back in the nineteen-fifties and sixties, many economists were confident that newly independent African countries, such as Ghana, Kenya, and Uganda, would be able to escape poverty if only Western donors filled the “financing gap” between what they could afford and the resources they needed to invest in factories, roads, railways, and other forms of infrastructure. Once these nations developed modern industrial sectors, the thinking went, the rest of their economies would be pulled along. In a 1960 book, “The Stages of Economic Growth,” W. W. Rostow, an economic historian at M.I.T., popularized a version of this argument, saying that if underdeveloped nations doubled investment rates, they would soon “take off” into self-sustained growth. During the Kennedy and Johnson Administrations, in which Rostow served, America’s foreign-aid budget reached an all-time high of 0.6 per cent of G.D.P. [Gross Domestic Product], and during the seventies and eighties significant amounts of aid continued to flow to poor countries. . . . [But] as William Easterly, an economist formerly with the World Bank, has observed, many countries that received a significant amount of aid, such as Ghana, Zambia, Chad, and Zimbabwe, had economies that either failed to grow much or actually shrunk. Meanwhile, a number of places that received very little foreign assistance, such as Singapore, Hong Kong, and Malaysia, expanded rapidly. Looking at the overall record, there appears to be no statistical correlation between aid and growth. (Cassidy 2005)

For additional reviews of Sach’s book, see: (Cooper 2005; Economist 2004b; Economist 2005f; Economist 2008k; Guardian 2005; Hamilton 2005; Wikipedia n.d.j).

Easterly also discusses the context in which granting agencies work. “As the awful examples in this chapter [Chapter 4] illustrate, the official aid agencies simply don’t know how to change bad governments into good governments with the apparatus of foreign aid. . . . To make matters worse, the aid agencies need the poor-country government, even a bad government, to fill the role of aid recipient to keep money flowing” (Easterly 2006:156). “Since aid agencies need to please the electorate in rich countries, the agencies often strive to produce side effects for rich countries at the same time they are transforming the rest . . . The United States requires recipients to spend the aid receipts on products from American companies for about three quarters of its aid. Other donor nations have similar restrictions” (Easterly 2006:192).

The Nobel Laureate Amartya Sen, reviewing Easterly’s book in the prominent journal *Foreign Affairs*, entitles his review “The Man Without a Plan (Sen 2006:March/April

2.2

Easterly emphasizes the importance of being searchers (in his terminology), “for aid agency staff . . . to listen to the poor [rather] than costing out a Big Plan.” He stresses the importance of paying attention “to the searchers with knowledge of local conditions . . . and [getting] feedback from the poor. . . [regarding] all the variable and complicated answers to how to make aid work” (2006:369). Easterly suggests that academics – anthropologists, for example, “could do public service by applying their techniques to evaluate the projects, programs, and approaches taken by aid agencies” (Easterly 2006:205).

Rather than belonging to the 21st century, their reality “is the fourteenth century: civil war, plague, ignorance” (Collier 2007:3). “Change is going to have to come from within the societies of the bottom billion, but our own policies could make these efforts more likely to succeed, and so more likely to be undertaken” (Collier 2007:12).

To ferret out the factors inhibiting development among the bottom billion, Collier compares what has gone amiss in various countries. He highlights four traps that these countries frequently fall into to varying degrees (2007:17-37).

(1) Having violent conflicts, usually in the form of civil wars but also in the form of coups. Collier calculates that the typical civil war costs a country roughly $64 billion. Given that two civil wars tend to start within the bottom-billion countries each year, he calculates the cost at over $100 billion per year for these conflicts.

(2) Natural resource traps: the ability to export valuable natural resources (such as oil or diamonds). The royalties that accrue from these exports allow political leaders to ignore the demands of their countrymen. The leaders are freed from electoral accountability; they can establish systems of patronage that reinforce their power. Rather than spending the profits to develop their countries, they store this wealth in Swiss bank accounts.

(3) Not having direct access to the sea and, as a result, not being able to ship goods overseas easily. In contrast to Switzerland, for example, which uses the infrastructure of Germany, Italy, and France to ship its goods overseas, Uganda must depend on a country with a less developed infrastructure, Kenya.

(4) Poor, corrupt governance that prevents development from being successful. Collier notes that “the leaders of many of the poorest countries are themselves among the global super rich. They like things the way they are.” He writes unfortunately “many of the politicians and senior public officials in the countries of the bottom billion are villains” (2007:66-67).

In respect to the bottom billion, Collier observes: “Seventy-three percent . . . have been through civil war, 29 percent . . . are dominated by the politics of natural resource revenues, 30 percent are landlocked, resource-scarce, and in bad
neighborhoods [i.e. with poor neighbors], and 76 percent have been through a prolonged period of bad governance ad poor economic policies” (2007:79).

Laurent Kabila, who overthrew the corrupt government of Mobutu Sese Seko of Zaire in 1997, famously told a journalist that all you needed to overthrow an African government was “ten thousand dollars and a satellite phone” (Collier 2007:21).

Collier notes: “Without aid, cumulatively the countries of the bottom billion would have become much poorer than they are today. Aid has been a holding operation preventing things from falling apart” (2007:100).

“Resource revenues to the bottom billion are bigger than aid, and far more poorly used,” Collier notes. “If we could raise the effectiveness of [how these] resource revenues [are spent] . . . the impact would be enormous” (Collier 2007:140).

Collier suggests giving the poorest countries a trading edge: “Goods and services exported from the bottom billion to the rich world markets would pay lower tariffs than the same goods coming from Asia . . . Privileging the bottom billion again low-income Asia is not just or fair; a more accurate word might be ‘expedient’” (Collier 2007:167).

Nial Ferguson, in a New York Times review of The Bottom Billion, suggests, “Collier’s is a better book than either Sachs’s or Easterly’s for two reasons. First, its analysis of the causes of poverty is more convincing. Second, its remedies are more plausible” (Ferguson 2007:July 1, 2007, internet version).


Quoting from a recent review in the Times Literary Supplement”: Because aid is politically accountable to Western electorates – which consume only the images and reports of its impact and not the real things – there are few incentives to make it work better” (Waal 2002:8).

Muhammad Yunus, the 2006 Nobel Peace Prize author, describes this phenomenon in Creating a World Without Poverty: “Many anti-poverty efforts are funded by well-intentioned people in the developed countries, either through NGOs, government grants, or international aid agencies. It’s sad to see much of this money being invested in ways that are wasteful. In many cases money that is supposed to help the poor ends up creating business for companies and organizations in the developed

Alex de Waals’s Famine Crimes: Politics and the Disaster Relief Industry in Africa (1997) phrases the problem in stronger terms. “Most humanitarian aid to Africa, while useful in keeping aid agencies afloat, is, for the victims of war and famine, either useless or counter-productive. Not only has aid repeatedly fueled violence, it has also distracted attention from the human rights violations that often underpin famines and, by taking on a welfare role, served to remove the responsibility for preventing famine from governments” -- see (Keen 1999:28).

== On The Difference Democratic Governance Can Make In Development:

Let me offer a comparative example of Collier’s point regarding corrupt governance. It reinforces Easterly’s assertion that “official aid agencies simply don’t know how to change bad governments into good governments.”

Contrasting Zimbabwe (once a democracy but now more of an oligarchy) with Hawaii (once an oligarchy but now a democracy), we see that good governance more than foreign aid may make the key difference in development.

Once Zimbabwe possessed one of the strongest economies in Africa. Today its economy is in shambles. The Economist (Economist 2007a) indicates that inflation is running at over 1,700 percent, the highest in the world. “Once the regional bread-basket, Zimbabwe is again going to go hungry this year. The economy has shrunk by half since 1999, and unemployment is thought to be at least 80 percent. Many people survive thanks to the black market and money or food sent by the 3m[illion] or so Zimbabweans who have left the country.” “Zimbabwe is expected to produce between 500,000 and 750,000 tonnes of maize this year,” the Economist reported, “but Zimbabweans need 1.8 m tonnes to stave off hunger . . . half a million are thought to be in danger of starving . . . Zimbabwe used to be an industrial powerhouse, second only to South Africa in the region. But the country is now undergoing an industrial revolution in reverse. With foreign currency scarce, factories are whimpering to a halt for want of fuel or imported spare parts. Zimbabwean manufacturing, which accounted for 27 percent GDP [Gross Domestic Product] in the early 1990s, is now only 13 percent of a much smaller economy” (Economist 2002). The World Health Organization indicates Zimbabweans now have the shortest life expectancy in the world – thirty-seven years for men, thirty-four for women. According to The Fund for Peace’s “Failed State Index, 2009”, Zimbabwe ranks second highest worldwide (behind Somalia) as a failed state – higher than the Sudan, Democratic Republic of the Congo, Afghanistan, or Haiti (Peace 2009).

In the Zimbabwean presidential election held in late March 2008, the opposition candidate Mr. Tsvangirai, according to exit polls, defeated the incumbent president Mr. Mugabe. (Mr. Mugabe has been Zimbabwe’s president since 1980.) After much delay, the Zimbabwean Election Commission confirmed that
indeed Mr. Tsvangirai had won the election, but not by a large enough margin to avoid a run-off. In the run-off election, key members of the opposition party were beaten, kidnapped and murdered. Mr. Tsvangirai was detained by the police for several hours and eventually withdrew from running. It came as no surprise, given that he was now running unopposed, that Mr. Mugabe won the run-off election.

An investigative reporter for the Washington Post uncovered the story behind this story. Apparently, on the afternoon of March 30, Mugabe had “informed the leaders of the state security apparatus that . . . he had lost the presidential vote held the previous day. Then Mugabe told the gathering that he planned to give up power in a televised speech to the nation the next day . . . but Zimbabwe’s military chief, Gen. Constantine Chiwenga, responded that the choice was not Mugabe’s alone to make. According to two first-hand accounts at the meeting, Chiwenga told Mugabe his military would take control of the country to keep him in office or the president could contest a runoff election, directed in the field by senior army officers supervising a military-style campaign against the opposition . . . the notes and interviews [relating to the meeting] make clear that . . . [Mugabe’s] military supporters, who stood to lose wealth and influence if Mugabe bowed out, were not prepared to relinquish their authority simply because voters checked Tsvangirai’s name on the ballots” (Timberg 2008).

What can be done to correct the problem? Not much, aside from military intervention, which no one seems to be contemplating. A recent headline from The Economist reads: “Getting away with murder, to their shame, African leaders fail to act against Zimbabwe’s Robert Mugabe” (Economist 2008c). The article continues: “nobody believes that the election in Zimbabwe was fair. Yet [at a summit of the African Union] Africa’s leaders, for the most part, . . . remained shamefully silent” (also see Economist 2008h).

The contrast with Hawaii is striking. Many may view Hawaii as a romantic, tropical paradise. In fact, up through the 1940s, it was an oligarchy ruled by a small number of Caucasian (or Haole) families who controlled the political and economic life of the islands. It was rule by the few for the few. Lawrence Fuchs in a classical work on Hawaii, *Hawaii Pono*, explains: “In Hawaii, the socially superior – the kamaaina [Hawaii-born] haole [or Caucasian] elite . . . governed . . . No community of comparable size on the mainland [i.e., the continental United States] was controlled so completely by so few individuals for so long. Rarely were political, economic, and social controls simultaneously enforced as in Hawaii. Rarely were controls so personal, and rarely were they as immune from . . . counterforces. . . . For forty years, Hawaii’s oligarchy skillfully and meticulously spun its web of control over the Islands’ politics, labor, land, and economic institutions without fundamental challenge” (Fuchs 1961:152)

The Japanese Americans – both Japanese-born (issei) and American-born (nisei) – were discriminated against. Brought over from Japan to work the plantations, they were the lowest group on the ethnic totem pole. There were proportionally fewer Japanese-American plantation foremen, for example, and
Japanese American foremen were paid lower wages. The plantation living conditions for Japanese-Americans were poor. Yet, up through the 1930s, there seemed few ways to escape from plantation labor. Fuchs writes: “The Japanese were generally scorned by others; and in Honolulu a concerted effort was made to destroy the Japanese language press and schools and the remnants of the Japanese labor movement” (Fuchs 1961:121). “In the early 1920s the oligarchy did everything that reasonably could be done to dishearten the Japanese. They were kept out of federal and territorial jobs. They were discouraged from opening homesteads” (1961:177).

By the 1970s all this had changed. Fifty-one percent of the state’s employees were now Japanese-Americans; for the Department of Education, the figures were 58 percent teachers, 57 percent in leadership positions. Fifty percent of the state’s legislators were Japanese American. Japanese Americans were doctors, lawyers, dentists, and engineers. The median income of families headed by Japanese Americans was now second only to those headed by Chinese Americans (Fuchs 1961:xiv-xv). (It was above Haole households.) Japanese Americans had the third-highest rate of home ownership in the state, 70 percent.

What had happened? What had happened is termed, in Hawaii, “the Democratic Revolution.” The Haole oligarchy had been a Republican affair. After World War II, returning Japanese American veterans, allied with liberal Haole Democratic politicians, were able to wrest control of the state legislature in 1954. In 1962, the first Democratic governor ever was elected. The first Japanese-American governor – in any state – was elected in 1974. During the 1920s, Japanese-Americans had constituted Hawaii’s largest ethnic group but, with those born in Japan (issei) not allowed to vote, they possessed relatively little political clout. With the passage of the national McCarran-Walter Act in 1952, issei became naturalized citizens in large numbers. By the 1954 election, the year of the Democratic Revolution, Japanese-Americans constituted the largest electoral group in the islands.

But the change was not only political. The newly elected Democrats faced a basic problem. The old oligarchy was not about to give up (or even share) its wealth. And, given that the Democratic Revolution was not a real revolution, only an electoral one, the state legislature could not simply take the oligarchy’s wealth away, especially its control over land (around which all other wealth revolved). Laws had to be followed.

The Democrats “enlarged the pie” so there was more wealth to go around. The oligarchy did not have to give up its wealth; instead, the Democrats created more wealth. To quote from another classical work, George Cooper and Gawan Daws’s Land and Power in Hawaii (Cooper and Daws 1990:7-8):

The idea was that development would create new wealth for new groups of people. . . . The living standards of middle- and working-class people would rise. Thus rather than cut up the old pie of landed wealth in a different way, the idea was to make the pie grow rapidly and continually
by developing land intensively, so that everyone could have more without anyone having to give up anything of significance.

Central to this equation was that major participants in the development process would be leading Democrats, plus their allies among the Islands' rising class of local-Asian businessmen.

Within as little as three years after winning control of the Legislature major land deals were being struck between Democratic developers/investors and Republican landowners. . . . though the landed rich had been the avowed enemies of the Democrats for decades, now, almost overnight, Democrats became their partners in mutually profitable land deals.

The contrast between Zimbabwe and Hawaii is clear. One involved a downward spiral of destruction, the other the upward spiral of development. Zimbabwe was given large amounts of foreign aid that were used, in part, to prop up the government. Hawaii generated new wealth from within through the electoral process.

Readers interested in further exploring topics covered in 2.1 and 2.2 may wish to peruse: Bolton 2007; Calderisi 2006; Cooper 2005; Cowen 2007; Economist 2000; Economist 2005a; Economist 2005b; Economist 2005d; Economist 2006a; Economist 2007b; Economist 2007c; Economist 2008b; Economist 2008i; Economist 2008j; Engardio 2006; Hansch n.d.; Harayda 1997; Kotkin 2007; Kristof 2006; Kristof 2007a; Kristof 2007b; Maren 1997; Postrel 2006; Riddell 2007; Rieff 1997; Rieff 2002; Secour 2002; Thomas 2006; Vaux 2001; Wikipedia n.d.d.

2.3

The Economist describes what occurred after communist North Vietnam won the war in 1975 and took control of the country:

In the 1980s Ho Chi Minh's successors . . . damaged the war-ravaged economy . . . by attempting to introduce real communism, collectivizing land ownership, and repressing private business. This caused the country to slide to the brink of famine. The collapse soon afterwards of its cold-war sponsor, the Soviet Union, added to the country's deep isolation and cut off the flow of rubles that had kept its economy going. Neighboring countries were inundated with desperate Vietnamese “boat people.”

Since then the country has been transformed by almost two decades of rapid but equitable growth, in which Vietnam has flung open its doors to the outside world and liberalized its economy. Over the past decade annual growth has averaged 7.5 percent. . . . An agricultural miracle has turned a
country of 85m once barely able to feed itself into one of the world's main providers of farm produce. Vietnam has also become a big exporter of clothes, shoes, and furniture, soon to be joined by microchips when Intel opens its $1 billion factory outside Ho Chi Minh City. Imports of machinery are soaring. Exports plus imports equal 160 percent of GDP, making the economy one of the world's most open. . . .

Vietnam's Communists conceded economic defeat 22 years ago, in the depths of a crisis, and brought in market-based reforms called doi moi (renewal), similar to those Deng Xiaoping had introduced in China a few years earlier. As in China, it took time for the effects to show up, but over the past few years economic liberalization has been fostering rapid, poverty-reducing growth. The World Bank’s representative in Vietnam, Ajay Chhibber, calls Vietnam a "poster child" of the benefits of market-oriented reforms. Not only does it comply with the catechism of the “Washington Consensus” — free enterprise, free trade, sensible state finances, and so on — but it also ticks all the boxes for the Millennium Development Goals, the UN's anti-poverty blueprint. . . . Vietnam has become the darling of foreign investors and multinationals (Economist 2008g) – see also (Economist 2008a; Economist 2008e; Templer 1999).

To quote Stanley Karnow’s *Vietnam: A History*, the companion book to the 1983 PBS series on the war: “Presidents from Harry S. Truman through Richard M. Nixon had justified America’s commitment to [South] Vietnam as part of its policy to ‘contain’ global Communism. They advanced the ‘domino theory,’ submitting that defeat in Southeast Asia would topple the other nations of the region – and even, as Lyndon Johnson warned, menace ‘the beaches of Waikiki’” (Karnow 1983:30). It was based on the perception that the allies, rather than appeasing Hitler in Munich in 1938 (as Neville Chamberlain, the British Prime Minister, did), should have drawn a line in the sand against Hitler and thereby averted World War II. (The perception, still common today, ignores two important facts: (1) it is unclear Chamberlain knew what he was doing since he came ill-prepared for his meeting with Hitler, not even bringing his own interpreter or record-keeper, and (2) the dynamics driving Hitler’s belligerency probably made war inevitable no matter what the allies did.)

The American struggle with Communism was actually more subtle and complicated than falling dominoes. To quote Neil Sheehan’s Pulitzer Prize-winning *A Bright Shining Lie*, the secretaries of state under U.S. Presidents Truman and Eisenhower were not naive enough to think they could export democracy to every nation on earth. . . . If American statesman saw a choice . . . they favored a democratic state or a reformist-minded dictatorship. Their high strategy was
to organize the entire non-Communist world into a network of countries allied with or dependent on the United States. They wanted a tranquil array of nations protected by American military power, recognizing American leadership in international affairs, and integrated into an economic order where the dollar was the main currency of exchange and American business was preeminent. The United States didn’t seek colonies as such . . . Americans were convinced that their imperial system did not victimize foreign peoples. ‘Enlightened self-interest’ was the sole national egotism to which Americans would admit (1988:131).

Here is how the only anthropologist who has written a book on the period, Neil Jamieson, phrases it in Understanding Vietnam: “over two and a half million Americans went to Vietnam, and over 55,000 . . . died there . . . Yet our understanding of this tragic episode remains superficial and, I believe, in many respects simply wrong. We have failed to understand our experience [in Vietnam] because, then and now, we have ignored the perspectives of the people most deeply concerned with the war in which we became involved: The Vietnamese . . . The images of Vietnam about which the controversy swirled in the United States arose from our own culture, not from Vietnamese realities or perceptions” (1993:ix).

In his acclaimed 1972 book The Best and the Brightest, David Halberstam asks how so many intellectually astute individuals in the upper levels of the Kennedy and Johnson administrations could have gotten things so wrong. The short answer, he suggested, was that they were arrogant: “An administration which flaunted its intellectual superiority and superior academic credentials made the most critical of decisions with virtually no input from anyone who had any expertise on the recent history of that part of the world, and it in no way factored in the entire experience of the French Indochina War” (1992:xv).

Sheehan describes John Paul Vann, a decorated war hero, as someone who “knew nothing of the Vietnamese and their culture and history. He did not consider this ignorance any more of an impediment to effective action than his lack of knowledge of counter-guerrilla warfare. . . . Vann was sure that he would be able to see what motivated the Vietnamese officers with whom he would be working and persuade them to do what was in their best interest and in the interest of the United States” (Sheehan 1988:42).

As David Halberstam writes in The Making of a Quagmire (Halberstam 1964), “it was “a classic example of seeing the world the way we wanted to, instead of the way it was” (1964:339).

The anthropologist Neil Jamieson records that when the North finally achieved unification in 1975, South Vietnam’s communist leaders were pushed aside. The politburo in Hanoi took complete control over the south, shutting out key South Vietnamese Communists. It has been suggested the massive losses suffered by the Viet Cong during the Tet offensive of 1968 – half of the Viet Cong’s forces were
destroyed – allowed the North Vietnamese to gain control over the communist movement in the south. After the Tet offensive, the war turned from being a fight among different groups of South Vietnamese espousing different political systems to a fight between northerners and southerners over control of the south. Truong Nhu Tang, a former minister of Justice in the South Vietnamese Communist National Liberation Front (N.L.F.) phrased it this way: “despite their collective efforts to defeat French and American colonialism, the North Vietnamese Communists became colonizers themselves” (Butterfield 1983:7).

Stanley Karnow writes that Ho Chi Minh in the 1940s and 1950s tried to “persuade the United States to underwrite his cause. . . . The United States might have plausibly encouraged Ho to emulate Marshal Tito, the Yugoslav Communist leader who was soon to defy Moscow” (1983:147).

David Halberstam writes in *The Making of A Quagmire*, “In Indochina, a colonial war turned the nationalism of a proud people against the West and into the hands of a very real enemy. That was the beginning of the downward cycle, for in those years when the Western nations were powerful they might have channeled this nationalism into a relatively neutral force” (1964:337). Butterfield agrees: “according to one school of thought, the Communists’ early capture of the nationalist issue made their eventual victory a foregone conclusion” (Butterfield 1983:4 - internet version).

Karnow writes, “Official U.S. communiqués and press reports . . . conveyed the idea that U.S. air strikes were devastating North Vietnam. . . . On my initial trip to the region, I expected to see it in ruins. Yet Hanoi, Haiphong and the nearby countryside were almost totally unscathed. I remembered General Curtis LeMay’s thunderous cry to ‘bomb them back into the Stone Age’ – but, scanning the north, I concluded that it had been in the Stone Age for decades” (1983:50).

Quoting Karnow again:

American strategists went astray by ascribing their own values to the communists. Westmoreland, for one, was sure that he knew the threshold of their endurance . . . Even after the war, he still seemed to have misunderstood the dimensions of their determination. “Any American commander who took the same vast losses as General Giap,” he said, “would have been sacked overnight.”

But Giap [the brilliant North Vietnamese general] was not an American confronted by a strange people in a faraway land. His troops and their civilian supporters, fighting on their own soil, were convinced that their protracted struggle would ultimately wear away the patience of their foes . . .

“We were not strong enough to drive a half million American troops out of Vietnam, but that wasn’t our aim,” Giap explained to me. “We sought to break the will of the American government to continue the conflict. Westmoreland was wrong to count on his superior firepower to grind us down. Our Soviet and
Chinese comrades also failed to grasp our approach when they asked how many divisions we had in relation to the Americans, how we would cope with their technology, their artillery, their air attacks. We were waging a people’s war [in the Vietnamese manner] . . . a total war in which every man, every woman, every unit, big or small, is sustained by a mobilized population. So America’s sophisticated weapons, electronic devices and the rest were to no avail. Despite its military power, America misgauged the limits of its power. In war there are two factors – human beings and weapons. Ultimately, though, human beings are the decisive factor.”

Ironically, many U.S. officers concurred . . . . The American army and its South Vietnamese allies,” wrote General Bruce Palmer after the war, “demonstrated a tendency to rely on superior firepower and technology rather than on professional skill and soldierly qualities . . . [The Viet Cong] had an extraordinary ability to recuperate,” wrote Palmer, “absorbing casualties in numbers unthinkable to us, replacing people, retraining and reindoctrinating them, and then bouncing back” (Karnow 1983:20-21).

But as a Vietnamese colonel commented after the war, when this was pointed out to him, “That may be so, but it is also irrelevant” (Butterfield 1983:11 – internet version).

Writing about how Robert McNamara, the American Secretary of Defense, often seemed to have supporting statistics to emphasize that an American victory would soon be at hand, David Halberstam, in The Best and the Brightest, states: “McNamara had invented . . . [statistics], he dissembled even with the bureaucracy . . . It was part of his sense of service . . . it was all right to lie and dissemble for the right cause. It was part of service, loyalty to the President” (1969:581).

Rajiv Chandrasekaran, in Imperial Life in the Emerald City, presents an account of how the United States administered Iraq during the year after the invasion. A reviewer of Chandrasekaran’s book writes: “The C.P.A.’s recruitment policy would have shamed Tammany Hall. Loyalty to George W. Bush and the Republican Party was apparently the prime criterion for getting work in the C.P.A” (New York Times, December 17, 2006, “Boys in the Bubble”). For example, a man named John Agresto was recruited to rehabilitate Iraq’s university system (with 22 campuses and more than 375,000 students). “Agresto had no background in post-conflict reconstruction and no experience in the Middle East,” Chandrasekaran writes. “But Agresto was connected: . . . Vice President Dick Cheney’s wife had worked with him at the National Endowment for the Humanities” (2006:5). James K. Haveman, a former community-health director for a Michigan Republican governor, attempted to revitalize the Iraq health system – through a U.S.-inspired model involving private providers and copayments. All the while, Iraqi hospitals lacked essential medical equipment and were overwhelmed by the need to care for the wounded. A twenty-four-year old, Jay Hallen, with little knowledge of American stock markets and few if any courses in economics or finance, was put in charge of reopening the Baghdad
stock market. He set up a stock market modeled after the New York Stock Exchange; but it didn’t work as planned. When Chandrasekaran asked the Iraqi who took over the stock market after Hallen what would have happened if Hallen had not been assigned to the task, he replied: “We would have opened months earlier. [Hallen] had grand ideas but those ideas did not materialize” (2006:262). As a member of the Coalition Provisional Authority confided to Chandrasekaran: “If this place succeeds, . . . it will be in spite of what we did, not because of it” (2006:329).

2.4

Fox Butterfield, in a review article for the New York Times Magazine, notes that a “black hole” existed in the American academy in respect to Vietnam during the War. The New York Times conducted a survey in 1970 and found not a single scholar focusing on North Vietnam; fewer than 30 students were studying Vietnamese (Butterfield 1983).

Until quite recently, Vietnam has been a giant black hole in American academia. A survey by The New York Times in 1970 found that there was no scholar in the United States who devoted most of his or her time to studying North Vietnam; there was no American university with a tenured professorship in Vietnamese studies, and there were fewer than 30 students in the country studying the Vietnamese language. By comparison, there were three times that many studying Thai. According to the Social Science Research Council in New York, from 1951 to 1981 a total of 820 Americans applied to it for financial aid to work on Ph.D. dissertations in Southeast Asia. Of these, only 33 were for Vietnam, almost the same number as for Burma.

John K. Fairbank, professor emeritus of history at Harvard and the man generally recognized as the founder of modern Chinese studies in the United States, refers to this lack of expertise on Vietnam as an academic Pearl Harbor. One reason for this blind spot, he suggests, was that the French excluded American missionaries and businessmen from Vietnam, and Americans therefore did not have a personal stake in Vietnam.

However, by the time the United States became militarily involved, many academics considered the war so immoral that dispassionate analysis of the conflict became almost impossible. Only a handful of scholars actually went to Vietnam to do research there.

It has been suggested by William Rodarmor (personal communication) that the military, however, was indeed quite active in language training (see http://www.dliflc.edu/historyofdl.html ). As noted, the anthropologist Neil Jamieson did write a book about Vietnam. But it came out years after the war was over – too late to assist policy planners and the general public.
In *The Best and the Brightest*, Halberstam noted that key government experts on Asia had been pushed out of government service during the anti-Communist campaigns of the 1950s and were never replaced (1969:xviii).

Not having an on the ground understanding of what was working and what wasn’t meant that the United States spent billions of dollars trying to assist development in South Vietnam and, by and large, being ineffective. Here is an example from Stanley Karnow’s *Vietnam*:

As long as the United States was willing to foot the bill for any given program, the Vietnamese rarely rejected it.

The difficulty, however, was to measure results. Or, as a U.S. official in Saigon explained it to me . . . “Say, for instance, that we hand them a plan to distribute ten thousand radios to villages so that peasants can listen to Saigon propaganda broadcasts. They respond enthusiastically, and we deliver the radios. A few months later, when we inquire, they tell us what we want to hear: peasants are being converted to the government cause, and we’re winning the war. But what had really happened? Have all the radios reached the villages, or have half of them been sold on the black market? Are peasants listening to Saigon or to Hanoi? We don’t know. . . . We report progress to Washington because Washington demands progress” (Karnow 1983:362).

According to David Halberstam:

When Colonel William Crossen, one of the top intelligence officers, put it [the report] together he was appalled: the number of men that Hanoi could send down the trails without seriously damaging its defense at home was quite astonishing. . . . When Crossen came up with his final figure he could not believe it, so he checked it again, being even more conservative . . . and still he was staggered by what he found; the other side had an amazing capacity and capability of reinforcing. When he brought the study to Westmoreland’s staff and showed the figure to a general there . . . “Jesus” said the general, “if we tell this to the people in Washington we’ll be out of the war tomorrow. We’ll have to revise it downward.” So Crossen’s figures were duly scaled down considerably . . . the staff intuitively protecting the commander from things he didn’t want to see and didn’t want to hear, never coming up with information which might challenge what a commander wanted to do at a given moment” (Halberstam 1969:545).

John Nagl, in *Learning to Eat Soup with a Knife*, examines why the British army succeeded in fighting a communist insurgency in Malaysia, while the United States army failed in Vietnam. He observes, in explaining the difference, that “the British army was a learning institution and the American army was not” (Nagl 2002:xxii). He writes: “despite the efforts of a large number of officers on the ground who knew that the army’s conventional approach was ineffective – and was in fact counterproductive in many ways – the U.S. Army continued to rely on a conventional approach to defeating the insurgents through an attrition-based search and destroy strategy”
(2002:191). He writes: “local forces have inherent advantages over outsiders in a counterinsurgency campaign. They can gain intelligence through the public support that naturally adheres to a nation’s own armed forces. They don’t need to allocate translators to combat patrols. They understand . . . tribal loyalties and family relationships. . . . It is perhaps only a slight exaggeration to suggest that, on their own, foreign forces can’t defeat an insurgency; the best they can hope for is to create the conditions that will enable local forces to win it for them” (2002: xiv).

A look at the recently published *Counterinsurgency Field Manual* (United States and Corps 2007) makes that clear. If readers were ever in doubt as to the importance of anthropological understanding in military operations, they need only read the book’s chapter on intelligence: “What makes intelligence analysis for [counterinsurgency] so distinct and so challenging is the amount of sociocultural information that must be gathered and understood. . . . Truly grasping the operational environment requires commanders and staffs to devote at least as much effort to understanding the people they support as they do to understanding the enemy. All this information is essential to get at the root causes of the insurgency and to determine the best ways to combat it” (2007:135).

The *Counterinsurgency Field Manual* stresses a “unity of command” structure – “exercised by a single leader through a formal command . . . system” (2007:56).

In 1922, Walter Lippman suggested that given the complexity of the problems that afflicted America, the general public was incapable of coming to reasoned, effective solutions (Lippman 1922).

Lippman’s point must be placed within the context of changes occurring in America at that time. With the industrializing of the country in the late 19th century, as Robert Weibe notes in *The Search for Order*, came the breakup of small-town, self-contained communities and a search for new units of social cohesion, communication, and order (Wiebe 1967). Lippman suggested public opinion – given its diverse and divisive character at the time -- was not able to effectively address the nation’s problems. He leaned, instead, toward a special class of experts – a group of professionals – who could sort through and order the mass of data relevant to a problem, presenting it to others – decision-makers – in an unbiased, disinterested way. Lippman asserted that social scientists were arriving on the scene after important decisions were made and assessing why someone “did or did not decide wisely” The order, he suggested, should rather be “the disinterested expert first finds and formulates the facts for the man of action” (1922:236).

Dewey questioned Lippman’s dependence on experts to order knowledge for others. For Dewey, professional social scientists should educate the broader public – not just the decision makers – about social issues. Rather than deferring to experts and expert opinion, the public should be actively engaged in deciding public issues. Building democratic communities, Dewey asserted, entailed the active involvement of
citizens. “The union of social science, access to facts, and the art of literary presentation,” Dewey wrote, “is not an easy thing to achieve. But its attainment seems to me the only genuine solution of the problem of an intelligent direction of social life” (Dewey 1983 [1922]:343). Building the new democratic communities of the 20th century, Dewey asserted, entailed the active involvement of citizens. Association was entwined with communication and vice-versa (Dewey 1927:158).

Cultural anthropologists should not be mere technicians, hired guns, or patrons of the powerful. Professionals, the prominent sociologist Eliot Freidson writes, should use their knowledge for the public good. They have “the duty to appraise what they do in light of . . . [the] larger good, a duty which licenses them to be more than passive servants of the state, of capital, of the firm, of the client . . . .[They should not be] mere technicians . . . [who] serve their patrons as . . . hired guns . . . [who] advise their patrons . . . but . . . don’t . . . violate their wishes” (Freidson 2001: 122, 217).


Readers interested in further exploring this topic (and its broader implications for a functioning democracy) may wish to peruse: Aboulafia 1992; Alterman 1999; Barros 1970; Brandeis 1914; Champlin and Knoedler 2006; Cmiel 1993; Conklin 1992; Creighton 191; Damico 1978; Dewey 1916; Dewey, et al. 1998; Diggins 1993; Hocking 1929; Hook 1939; King 1917; Kloppenberg 1992; Moore 1916; Moore 1917; Myers and Myers 2001; Otto 1920; Pepper 1928; Rueschemeyer 1995; Smith 1929; Steiner 1992; Westbrook 1991; Westhoff 1995; Whipple 2005.


2.5
There is no doubt that a college degree affects one's earning power. The U.S. Census Bureau News reported in 2005, a “college degree nearly doubles annual earnings” (Bergman 2005). A high school graduate, according to the Census Bureau, earns an average of $27,915 a year; a college graduate $51,206. In a forty-year career, a bachelor's degree will improve one’s income by $931,640. (With an advanced degree it is $1,867,480.) For most of the population, a college degree is the key to upward mobility or, for those is in the higher income brackets, the key to remaining there.

Taking a century-long perspective, we can see that colleges, once the province of the exclusive few, have become more inclusive. In 1900, only 2 percent of the population went to college. In 2000, according to the U.S. Census Bureau, 15 percent of the population had earned bachelor degrees and 21 percent of the population had taken some college courses (but not completed a degree). If we limit ourselves to students of college-age, 36 percent have degrees. Sixty-three percent of high school graduates go immediately on to college (though, unfortunately, just over half earn a degree).

For several years now, college costs have risen faster than the rate of inflation. From 2003 through 2005, the cost of attending college rose 10 percent, 13 percent, and 7.1 percent respectively (with inflation averaging only a bit over 3 percent during this period) (Dillon 2005b). U.S. News and World Report indicates that tuition at a typical public university has risen almost tenfold in the last 30 years (Clark 2007a). Andrew Hacker reports in the New York Review of Books that Stanford and Harvard, despite their enormous endowments, charge almost three times as much in real (i.e. adjusted for inflation) dollars as they did a generation ago (Hacker 2007). From 1970 to 1990, the average tuition at private universities rose 470 percent compared with the consumer price index of 280 percent. In 1980, covering college tuition took 26 percent of the median American family’s income, in 2004, it took 56 percent (Lang 2006).

There is also the issue of attrition. “For decades, the college graduation rate has hovered around 50 percent” of those entering college, report the authors of Student Success In College (Kuh 2005b:7) In 2006, U.S. News and World Report stated, “Nationally, the average six-year graduation rate for all students is 57 percent” (Graves 2008).

The Chronicle of Higher Education reports that during the 1970's a Pell Grant – a non-repayable federal grant for students with lower incomes (defined today as a family income of no more than $40,000) – covered between 50-80 percent of a student’s direct costs at a public university. It presently covers about 30 percent. In the 1970’s, 75 percent of federal aid involved non-repayable grants; 25 percent loans. Today it is roughly reversed: 63 percent in loans, and 37 percent in grants (Difelciciantonio 2008).

One might hope that, with their large endowments, the elite universities would take up the slack and fund large numbers of less affluent students. They do not. Andrew
Hacker reports that “even the richest institutions expect that at least half their students will pay full tab” (Hacker 2007:Section 4). At Yale, only 10 percent of the students qualify for Pell Grants; at Princeton, it’s only 8 percent.

BusinessWeek reports that university endowments grew by 17 percent in 2007, but colleges collectively decreased the amount of the money spent by 2 percent (Lavelle et al., 2008:13). Over the past five years, university endowments have grown by more than 10 percent on an annual basis. In the same period, spending from university endowments declined by roughly 10 percent annually. In other words, colleges are making more money through their endowments, but rather than spending it on helping needy students or defraying other expenses with that money, they are leaving it in their endowments to make a tidy tax-free profit (tax-free because they are “nonprofit” organizations).

Many elite and elite-aspiring institutions have moved away from providing financial aid on the basis of need. Financial aid is now frequently used to attract affluent students. George Winter (Winter 2004), quotes Sandy Baum who has analyzed recent data collected by the College Board: “It’s absolutely true that the biggest increase in institutional aid has been to the upper-income families.” Karin Fischer, in the Chronicle of Higher Education (Fischer 2006), notes that from 1995 to 2003, research-oriented universities increased the their own grant aid – as opposed to the federal government’s – in respect to students whose families made less than $20,000 by 29%. At the same time they increases grant aid to students with family incomes over $100,000 by 186%.

Andrew Delbanco in the New York Review of Books reports that one-third of American undergraduates work full-time jobs and that over half of American undergraduates attend college part-time (presumably because they are also working) (Delbanco 2005a). CNNMoney.com indicates that the average debt for graduating seniors is now just under $20,000 (Kelley 2005).

A special report in BusinessWeek entitled “Thirty and Broke” summarizes the problem:

Thirty years ago, when many of their parents attended school, it was entirely possible to get through college with modest family savings and steady work during the summers. Since the mid-1980s, though, tuition has been growing far faster than many families can afford. The price of public colleges, where about 80 percent of all students are enrolled, increased 28 percent in the past five years alone, far more than in any five-year period since 1975. At private colleges, the total cost increased 17 percent. Those figures, it should be noted, already take inflation into account. At the same time, outright grants have been shrinking as a proportion of total financial aid. "The costs of education are moving from the government to families, and in families from parents to kids," says Melanie E. Corrigan, associate director of national initiatives and analysis at the American Council on Education in Washington (BusinessWeek 2005b).
The Economist states, “America’s great universities are increasingly reinforcing rather than reducing these educational inequalities” (Economist 2004a). The result, is as Kati Haycock, director of the Education Trust, observes, “Overall, the nation's top public universities are getting ‘whiter and richer,’ even as high-school graduating classes grow more diverse” (Fischer 2006).

But did you know that as much as three-fourths of many universities’ payrolls consist of non-teaching personnel? As Andrew Hacker reports, “Statistics show that employees who aren’t teachers make up 71 percent of Stanford’s total payroll as do 73 percent at Columbia and 83 percent at Harvard. Many of these people do research. But even more are counselors, fund-raisers, groundskeepers, lawyers, security personnel, admissions officers, or coaches” (Hacker 2005), Cornell reports that its administrative staff has grown by 123 percent during the last 15 years (Lang 2006).

In Declining By Degrees, Julie Johnson Kidd quotes from a letter Amichai Kilchevsky wrote to the New York Times, October 22, 2004: “As a senior at Middlebury College, one of the nation's most expensive private colleges, I have seen the comprehensive fee increase nearly $10,000 in four years to reach its current level of $40,400. If the rise in tuition was correlated with a rise in academic vigor, most students would accept the additional cost. This, unfortunately, isn’t the case. Indeed, many colleges like Middlebury have used excess revenue to finance vast building projects, which, while aesthetically pleasing, do little to enhance the quality of a student's education” (Kidd 2005:201).

How are colleges able to keep it up? Sam Dillon in the New York Times, quoting Patrick Callan of the National Center for Public Policy and Higher Education, provides a succinct answer. "There's a belief that high cost equals high quality," Mr. Callan observes. "Underlying all this is that more kids are graduating from high school every year, most of them want to go to college, and so it's a seller's market. Universities raise tuition because they can" (Dillon 2005b). Schools charge, Andrew Hacker reports, “what the market will bear” (Hacker 2005)

== On Course Requirements In College:

Why do schools specify certain course requirements in order to graduate? The explanation generally offered is to draw students toward a breadth of understandings through a familiarity with a range of subject, before they settle in to a specialty or major. Actually, it is more complex than that. Derek Bok, the former president of Harvard writes in Our Underachieving Colleges that many introductory courses are not designed to excite students with the possibility for new insights regarding the world around them or the importance of democratic citizenship. Rather they are constructed to “build a foundation for students intending to major in the field and perhaps go on to obtain a Ph.D. Most of the readings impart large quantities of basic information, material essential for planning further work in the discipline but precisely the stuff that other students will soon forget” (Bok 2006:261). The professors involved, he notes, “will
normally attend to the needs of their discipline with little regard to the other aims of a proper liberal education" (2006:47). Andrew Delbanco, in the New York Review of Book (Delbanco 2005a:52, 5) adds that introductory courses “in various academic disciplines . . . are conceived by the guilds in charge of them. Any larger sense of purpose seems absent.”

The same holds true in respect to the major. Why, Bok asks, are so many courses required in a major, especially “when the empirical evidence suggests that the typical concentration actually yields such limited cognitive gains" (2006:142)? Couldn’t the additional courses be used to help draw students toward broader, more socially valuable or personally meaningful matters? He writes, “originally students had to complete only a few courses in their chosen field of concentration. Over time, as faculties grew in size and disciplines acquired more and more subspecialties, requirements for the major expanded to occupy one-third or even one-half of all the classes students were required to take in order to graduate” (2006:136). One sees the importance of departments in shaping the curriculum: more faculty requires more courses, and more courses means more requirements, to ensure satisfactory enrollments in those courses.

Looking at how most introductory courses are taught, we see a related problem. Bok writes that despite most college faculties’ “overwhelming support for critical thinking as the primary goal of undergraduate education, most professors do not teach . . . [by] discussing problems in class, or using group work to promote active learning . . . [instead,] they spend almost the entire hour lecturing to a passive student audience. According to one survey of 1,800 instructors from a wide variety of institutions, between 73 and 83 percent of the faculty teach in this fashion . . . rarely [do] professors challenge students with questions calling for higher-order reasoning” (2006:120).

Why are the large, introductory lecture classes the staple of most universities? Murray Sperber, a professor emeritus at Indiana University, writes in Declining by Degrees: “When I have asked faculty in psychology, political science, and other departments this question, some respondents have frankly stated that these course are a positive for the department because the tuition revenue they generate helps to fund graduate students and research projects” (Hersh and Merrow 2005:137). The famous sociologist, David Riesman, observes: “Departments and whole institutions sought to become more distinguished by using the expansion of their student enrollments to recruit more faculty members than would have been necessary on the basis of earlier teaching loads and student/faculty ratios, thus allowing the build-up of graduate and research programs” (Riesman 1980:2). Making introductory courses not only required courses, but lecture courses, means that departments can gain high overall enrollments, allowing them to hire more faculty and offer more courses (that these new hires can then teach) without an enormous amount of work on a faculty member’s part once his or her lecture notes have been written. (Some faculty draw on one introductory text for their notes while using another in class, reducing their workload still further.)
What do students gain from the time and money spent on a college degree? The short answer is that we don’t know. Writes Richard Hersh in *Declining by Degrees*: “Until now, no one has been able to measure with any accuracy what students actually learn during their four years on a campus” (Hersh and Merrow 2005:230).

The most comprehensive analysis of the subject is a two volume set by Ernest Pascarella and Patrick Terenzini, both entitled *How College Affects Students* (Pascarella and Terenzini 1991; Pascarella and Terenzini 2005). (The first volume summarizes research up through the 1980's; the second volume covers more recent research.) Based on the analysis of literally thousands of studies, they conclude that as a result of college, “students learn to think in more abstract, critical, complex and reflective ways; there is a general liberalization of values and attitudes combined with an increase in cultural and artistic interests and activities; progress is made toward the development of personal identities and more positive self-concepts; and there is an expansion and extension of interpersonal horizons, intellectual interests, individual autonomy, and general psychological maturity and well-being” (Pascarella and Terenzini 2005:577).

It sounds impressive. But there are two problems. First, the studies focus on what students gain during their years at college. They do not compare what students gain at college with a control group of individuals who do not go to college (2005:578).

Second, while few doubt that significant intellectual and emotional changes occur in most students who attend college – especially when they live away from home – Pacarella and Terenzini write, “the gains made during the undergraduate years on various dimensions of academic learning and intellectual sophistication reflection only relative advantages of seniors over beginning students . . . college graduates as a group don’t always perform particularly well in terms of absolute standards of knowledge acquisition or cognitive functioning” (2005:573).

Derek Bok, the former president of Harvard, states the point more directly in *Our Underachieving Colleges*: “Most studies do show evidence of growth, but almost all the findings leave ample room for improvement. Even among the selective colleges that are ranked so highly . . . fewer than half of the recent graduates believe that college contributed ‘a great deal’ to their competence in analytic and writing skills . . . Surveys of student progress . . . including writing, numeracy, and foreign language proficiency, indicate that only a minority of undergraduates improve substantially while some actually regress” (Bok 2006:311). Elsewhere in his book, which is subtitled *A Candid Look at How Much Students Learn and Why They Should be Learning More*, Bok writes “many seniors graduate without being able to write well enough to satisfy their [future] employers. Many can’t reason clearly or perform competently in analyzing complex, nontechnical problems, even though faculties rank critical thinking as the primary goal of a college of a college education” (2006:8). The *New York Times* recently reported that in a College Board survey of 120 leading corporations, one-third of the employees at these corporations wrote poorly and, further, that these companies were spending billions of dollars on remedial training, including training for new employees straight out of college (Dillon 2008).
Cathy Small’s ethnography My Freshman Year: What a Professor Learned by Becoming a Student supplements the above studies in a critical way. (To disguise the school involved she wrote under the name Rebekah Nathan.) She writes, “while most students I interviewed readily admitted that they were in college to learn, they also made clear that classes, and work related to classes, were a minor part of what they were learning” (Small 2005:101). More than anything, she observed, students learn time-management skills – how to balance various demands. “Most seniors will agree that they’ve forgotten much of what they learned from classes, even from the semester before,” she writes. “Looking back on college, they will claim to have learned more about themselves, their abilities, and their relationships than about subject areas” (Small 2005:131).

2.6

Based on thousands of studies, Pascarella and Terenzini write: “Clearly, the 3,000-plus postsecondary institutions in the United States differ substantially in size, [and] complexity. . . . Yet, with some notable exceptions, the weight of evidence . . . casts considerable doubt on the premise that the substantial structural, resource, and qualitative differences among postsecondary institutions produce correspondingly large differences in net educational effects on students. Rather, the great majority of postsecondary institutions appear to have surprisingly similar net impacts on student growth” (Pascarella and Terenzini 2005:590).

Social critic Andrew Hacker makes the point this way: “I am . . . convinced that despite differences in endowment and faculty salaries, as good an education can be had at Coe College in Iowa, Whitman College in Washington, and Knox College in Illinois as at brand-name schools like Williams and Swarthmore” (Hacker 2005:54).

Most elite schools strenuously opposed the efforts made by former U.S. Secretary of Education Margaret Spellings to carry out just such accountability studies (see e.g. Basken 2008k).

Stacy Dale and Alan Krueger, using controlled samples (so as to avoid differences in intellectual ability), report that “students who attended more selective colleges do not earn more than other students who . . . attended less selective colleges” (Dale and Krueger 1999:30). A recent Wall Street Journal article reaching a different conclusion did not use controlled samples and, hence, was likely measuring other variables (Neenleman 2008). The two researchers indicate that “the payoff to attending an elite college appears to be greater for students from more disadvantaged backgrounds” (Dale and Krueger 1999:abstract).

Dale and Krueger’s analysis fits with other data. To cite Andrew Hacker’s article “They’d Much Rather Be Rich” in the New York Review of Books, referring to a consulting firm’s compendium of the educational backgrounds of the CEO’s of the nation’s 500 largest companies, he writes, “it turns out that only 13 of. . . [the CEO’s]
didn’t attend or finish college . . . . Another 8 are heirs to family enterprises . . . leaving 479 who had completed college and more or less ascended on their own. Altogether, 68 of the 479 – 14 percent – were graduates of 12 highly competitive colleges [such as Amherst, Brown, Harvard, Yale, and Stanford] . . . Most of the 411 chief executives went to less prestigious schools like Worcester Polytechnic, Marymount College, and Idaho State . . . . while an Ivy League degree may help in the early years of a career, its cachet tends to fade when more stringent tests are set” (Hacker 2007).

== On The “Earning Power” of a College Degree:

The reason a college degree adds so much earning power is not necessarily because students have a range of important new skills that are needed in a whole new age of competition and globalization. Two other factors are also critical: First, many of the blue-collar manufacturing jobs – such as in steel or automobile production – that allowed high school graduates a decent middle class standard of living are now gone. The jobs have been “shipped overseas.” Second, many businesses and professions now ask for degrees where, in the past, they did not. Hacker writes that 37 percent of flight attendants, 35 percent of tour guides, and 13 percent of security guards and casino dealers now have degrees (Hacker 2006).

Before discussing the operation of higher education in Canada, let me describe a few traits that Canada and the United States share. First, in both, individuals with a university degree possess a higher standard of living. With respect to income, 33 percent of those with a university degree in Canada and 30 percent in the United States make more than twice the median income. In comparison to those with only a high school degree, those with a university degree make 11 percent more in Canada and 8 percent in the United States (Miller, et al. 2007).

Second, paralleling the recent increased enrollments in the United States, Canada has also seen significant increases in university-bound students. Between 2000 and 2006, Canadian full–time university enrollment increased by 31 percent (Canada 2007b:5).

Finally, the percentage of the Canadian population ages 25 to 34 that had university degrees in 2004 was 53 percent (Miller, et al. 2007). Canada possesses the second–highest percentage of university graduates in the world, just behind Russia.

The problem Canadian higher education faces today is that between 1990 and 2003 tuition grew by 107 percent. This makes it more expensive than in the United States, especially when the present limited financial-aid packages are calculated into costs. Financial aid at Canadian universities hasn’t kept pace with increases in tuition as closely as in the United States (which as we saw above, has not kept pace with need) (Review 2003:1-3). During the early 1980s, Canada had a $2,000 funding advantage per student over the United States; in 2008, the United States had an $8,000 funding
advantage per student (in U.S. dollars). In 1980, provincial governments contributed 84 percent of the educational/research costs per student; now it is 66 percent. Government funding per student dropped from $17,900 in 1980 to $13,600 in 1990, and to $9,900 in 2006 (Canada 2008:4-5).

Like the United States, Canada has moved away from need-based financial aid. Canadian universities now provide almost 60 percent of their aid to families with above-median incomes (Birchard 2007a). The amount of debt Canadian students owe at graduation doubled in the 1990s. Today, the average debt of Canadian students who take out loans to finance their education is $24,000 (Tamburri 2008).

While student enrollment grew by 56 percent from 1987 to 2006, full-time faculty increased by only 18 percent. This compares unfavorably with the United States where a 33 percent increase in enrollment was met with a 33 percent increase in full-time faculty. The result is that faculty/student ratio is higher in Canada than the United States.

The most interesting difference between Canada and the United States lies in the area of accountability. The limited data that exist suggest that Canadians are slightly more literate – able to read complex documents slightly better – than Americans in the 20-25 age range (Wagner 2006:22). MacLean’s magazine reports, “While undergraduate student satisfaction remains relatively high at Canadian institutions . . . many Canadian university campuses are not as engaging and may not be offering as good an educational experience as their American peers” (Macleans.ca 2008c).

To illustrate how NSSE works, let me quote from Student Success in College (Kuh 2005b). (Two of the book’s authors are involved with NSSE). “Voluminous research on college student development shows that the time and energy students devote to educationally purposeful activities is the single best predictor of their learning and personal development” (Kuh 2005b:8). NSSE assesses how much time and energy students are investing in “educationally purposeful activities” at their school. Student Success in College continues “Certain institutional practices are known to lead to high levels of student engagement . . . [such as] student-faculty contact, cooperation among students, active learning, prompt feedback, time on task, high expectations, and respect for diverse talents and ways of learning” (2005:7). NSSE assesses in what form and to what degree these are occurring at a school (see NSSE n.d.).

Working with the Commission on the Future of Higher Education, U.S. Secretary of Education Margaret Spellings tried to bring cross-campus assessments to the American system of higher education (Ashburn 2006). She largely failed, as a 2008 headline in The Chronicle of Higher Education notes: “Spellings Campaign Runs Low on Time, and on the Power to Persuade.” The article notes: “Education Secretary Margaret Spellings has made another high-profile attempt to convince colleges that they risk painful government interventions if they don’t improve the quality of their programs and help more students evaluate . . . them. With just six months until Ms.
Spellings leaves office, colleges seem increasingly willing to keep taking that risk” (Basken 2008h).

How did Canadian universities get involved with NSSE? Bob Rae, the former premier of Ontario, found critical data missing from his 2005 review of post-secondary education in Ontario. His conclusion, quoting *Maclean’s*, was “We simply don’t know enough about how we are doing or what others are doing.” (This is the point Margaret Spellings emphasized.) *Maclean’s* continues, “to this end, Rae recommended that all Ontario universities participate in the NSSE. All Ontario universities have done so over the past two years, and most universities in the rest of the country have joined them” (Macleans.ca 2008c).

== On German Higher Education:

Comparing higher education in the United States with higher education in Germany helps place the above discussion in perspective. The contrast between the United States and Germany is striking. Until recently, German students did not have to pay tuition and, moreover, frequently could go to the university of their choice.

German universities are all public, financed by their respective state governments, and did not, until recently, charge tuition. The universities were free to all who held an Abitur degree from a Gymnasium. (The Abitur degree was awarded to students upon completion of their exams at the end of their secondary education – after 12 or 13 years of schooling. Depending on one’s perspective, the Abitur can be seen as equivalent to a high school degree or an associate’s degree from a community college in the United States.) Only in the last two or three years have Germany’s 117 universities started charging tuition. The tuition at the University of Cologne, which might be seen as typical, is now $750 per term of studies (Landler 2006; Wilhelm 2008).

Students do have to pay for their room and board, usually finding some form of accommodation in the town near the university. Students who have clear financial need can get financial aid from the state that covers much if not most of these expenses for 4-5 years. The aid usually involves ½ loan, ½ grant.

Up to now – though this is about to change – there have not been the prestige markers that distinguish and hierarchically differentiate among universities as occurs with America’s 4,000 colleges and universities. Since at least the 1970’s, a strong egalitarian ethos has pervaded German higher education. Students are allowed to attend any university that offers the course of studies they wish (except in certain areas where enrollments are exceptionally tight). Students can change their universities frequently at will.

In the liberal arts, students tend to pick the lectures and seminars that interest them. Students usually need to gain three certificates – by passing exams in
certain subjects and demonstrating a reasonable degree of attendance at certain optional courses – to sit for their final exams to graduate. Frequently, only the final exam grades are kept in a student’s records following graduation. (Wikipedia n.d.l).

The down side of this freedom is that it takes an average of 5-6 years and may well take 10 years to be graduated. Because state funding has not kept up with the increase in enrollments, the faculty-student ratio hovers around 1:50 and sometimes rises as high as 1:180 (Esnault, et al. 1997; Wilhelm 2008:5 -- see also Chronicle, April 4,2008; OECD Publications, Thematic Review of the First Years of Tertiary Education Country Note: Germany, 1997:5). In 1970, prior to the dramatic increase in enrollments, the ratio was 1:9. Students rarely have contact with teachers. The Economist refers to the “blockseminar” – “an ingenious system whereby an academic turns up briefly at the university and delivers an entire term’s teaching in the space of a weekend” (Economist 2004c).

Usually, one finds other students with related interests to study with if one wishes interaction. Above and beyond the formal courses students take, many students frequently teach themselves. The university completion rate is among the lowest in the world, significantly lower than that of the United States which is, itself, quite low (Education 2006:8).

Many German universities lack the accoutrements of American universities. Buildings are frequently in need of repair. The rector (or head) of the University of Cologne reported “our university is rotten in some places . . . the toilets are rotten, the electrical system falls from the walls” (Wilhelm 2008).

The problem is basically, that increase in student enrollments, encouraged by various state governments, has far outstripped these state governments’ willingness to pay the increased costs. Spending per student is not only low but also declining. Faculty salaries are poor by Canadian and American standards. And in international rankings of universities, German universities, once the highest in the world, now rank well below comparable institutions in the United States and Canada. Germany has fewer people – in both the 25-34 and the 25-64 ages groups – with tertiary degrees (even when the vocational Fachhochschulen are included) compared to Canada and the United States.

As Supreme Court Justice Louis Brandeis famously wrote, "sunlight is said to be the best of disinfectants" for addressing social problems. "Publicity is justly commended as a remedy for social and industrial diseases. Sunlight is said to be the best of disinfectants; electric light the most efficient policeman. And publicity has already played an important part in the struggle against the Money Trust." (Other People’s Money -- and How Bankers Use It; Brandeis, 1914).

Readers interested in further exploring topics covered in 2.5 and 2.6 may wish to peruse: Anonymous 2007; Ashburn 2008; Basken 2008a; Basken 2008b; Basken 2008c; Basken 2008d; Basken 2008e; Basken 2008f; Basken 2008g; Basken 2008i; Basken 2008j; Benton 2008; Bergman 2003; Bergman 2005; Betts, et al. 2007;
3.1

The noted British historian Peter Burke in *A Social History of Knowledge* dates this academic ideal to the seventeenth and eighteenth centuries, highlighting Francis Bacon’s 1605 *The Advancement of Learning* as a prominent example. Burke writes that today “intellectual innovation, rather than the transmission of tradition, is considered one of the major functions of institutions of higher education, so that candidates for higher degrees are normally expected to have made a ‘contribution to knowledge,’ and there is pressure on academics . . . to colonize new intellectual territories rather than to continue to cultivate old ones” (Burke 2000:114).


The National Science Foundation, for example, considers “how important is the proposed activity to advancing knowledge and understanding” (National Science Foundation 2004). The Social Sciences and Humanities Research Council of Canada asks what is the “expected contribution to the advancement of knowledge” (2009). Applicants for Social Science Research Council Student Fellowships are judged on whether their research will constitute “an original contribution to the existing body of scholarship . . . [that] acknowledge[s] and build [s] on such scholarship” (2009).

“The intellectual adventure of cultural anthropology,” one noted historian of the discipline writes, “has exhibited a continuous . . . advance in perspective” (Voget 1975:795).

While “we do not consider the history of anthropology to be a linear tale of progress,” Eriksen and Nielsen assert, “we believe that there has been a steady, cumulative growth in knowledge and understanding within the subject” (Eriksen and Nielsen 2001:viii).

The author of *Theory and Progress in Social Science*, James Rule, writes:

> Pretensions of progress are pervasive in the images we project of our work. Conferences are convened, and volumes of studies commissioned, purporting to extend the “frontiers” of knowledge in one or another domain. Yearbooks are published documenting “advances” in the discipline. Journal submissions, books, and doctoral dissertations are assessed in terms of whether they constitute “contributions” to existing knowledge. Such language obviously presumes movement in the direction of fuller, more comprehensive, more *advanced* understanding. The notion of a “contribution” implies not just the sheer addition of another book, article, or research report to an ever-lengthening bibliography, but a meaningful step forward in a direction shared by all. And claims to participate in
such advances, I hold, are central to the justifications most of us would put forward for our work.

Rule continues, “the desire to ‘contribute’ to some form of intellectual progress is all but universal among students of social life. . . . Who would trouble to put words . . . to paper, if not in the hope that the result might leave some overall fund of understanding improved?” (Rule 1997:227)

"In anthropology, we are continuously slaying paradigms [or trends], only to see them return to life, as if discovered for the first time,” asserts Eric Wolf. “As each successive approach carries the axe to its predecessors, anthropology comes to resemble a project of intellectual deforestation” (Wolf 1990:588).

Kroeber phrases it more gently. He states that sciences such as anthropology "are subject to waves of fashion" (Kroeber 1948:391).

The noted Berkeley anthropologist Elizabeth Colson writes:

Rapid population growth and geographical dispersal [within cultural anthropology] have been associated with the emergence of a multitude of intellectual schools, each of which stresses both its own uniqueness and superiority and the need for the whole of the social/cultural community to accept its leadership. This never happens, and even the most successful formula rarely predominates for more than a decade: at the moment when it appears to triumph, it becomes redefined as an outmoded orthodoxy by younger anthropologists who are attempting to stamp their own mark upon the profession. This has the therapeutic effect of outmoding most of the existing literature, by now too vast to be absorbed by any newcomer, while at the same time old ideas continue to be advanced under new rubrics. The history of anthropology is a splendid example of what John Barnes once called structural amnesia, the creation of convenient myths that eliminate most details and celebrate a minimal number of ancestral figures. Selective forgetting, however, obscures the continuity of ideas that in fact continue to guide much of what is done (Colson 1992:51).

The Canadian anthropologist Philip Carl Salzman observes (Salzman 1994:34):

A well known and occasionally discussed problem is the fact that the vast multitude of anthropological conferences, congresses, articles, monographs, and collections, while adding up to mountains of paper (and subtracting whole forests), do not seem to add up to a substantial, integrated, coherent body of knowledge that could provide a base for the further advancement of the discipline. L. A. Fallers used to comment that we seem to be constantly tooling up with new ideas and new concepts and never seem to get around to applying and assessing them in a substantive and systematic fashion. John Davis, over two decades ago in The Peoples of the Mediterranean, seemed on the verge of tears of frustration during his attempts to find any comparable information in the available ethnographic reports that might be used to put individual cases into
perspective and be compiled into a broader picture. Nor is there confidence in
the individual ethnographic reports available: We cannot credit the accounts of I.
Schapera, because he was a functionalist, or that of S. F. Nadel because he was
an agent of colonialism, or J. Pitt-Rivers because he collected all his data from
the upper class señoritos . . . or M. Harris because he is a crude materialist, etc.
etc. So we end up without any substantive body of knowledge to build on, forcing
us to be constantly trying to make anthropology anew.

The prominent Chicago sociologist Andrew Abbott (referred to in Chapter 1) suggests
that the social sciences “pretend to perpetual progress while actually going nowhere
at all, remaining safely encamped within a familiar world of fundamental concepts.”
He writes: “The young build their careers on forgetting and rediscovery, while the
middle-aged are doomed to see the common sense of their graduate school years

*Readers interested in further exploring topics covered in 3.1 may wish to peruse:*
Campbell 1999; Colson 1989; Kuper 2007; Ortner 1984; Rensburger 1996; Voget

3.2

"There are in the field at the present time quite different ways of thinking about
time, about culture, and about cultural comparison," an account of the discipline
observes (Leaf 1979:3).

A prominent anthropologist highlights two perspectives: “The first [is] . . . concerned
with description and interpretation rather than with explanation, and with the particular
rather than with the general. . . . The second . . . seeks general principles and models
itself on the natural sciences rather than on the humanities” (Kuper in Borofsky

Another anthropologist, Roy Rappaport, observes: “Two traditions have proceeded in
anthropology since its inception.” One, he notes, “influenced by philosophy,
linguistics, and the humanities, and open to more subjectively derived knowledge,
attempts interpretation and seeks to elucidate meanings.” The other, “objective in its
aspirations and inspired by the biological sciences, seeks explanation and is
concerned to discover causes, or even, in the view of the ambitious, laws”
(Rappaport in Borofsky 1994: 154).

In the first tradition, often termed interpretivist, understanding an action "requires
reference to its larger context . . . the aim is not to uncover universals or laws but
rather to explicate context" (Rabinow and Sullivan 1987:14).
"Interpretive explanation," Clifford Geertz asserts, "trains its attention on what institutions . . . mean to those whose institutions . . . they are" (1983:22). "It is not against a body of uninterpreted data, radically thinned [or superficial] descriptions, that we must measure the cogency of our explications," he states, "but against the power of the scientific imagination to bring us into touch with the lives of strangers" (1973:16). Geertz is not anti-science. "I do not believe that anthropology is not or cannot be a science . . . that the value of anthropological works inheres solely in their persuasiveness" (in Carrithers 1990:274).

The positivist perspective is well expressed by Marvin Harris: Scientific knowledge, he states, "is obtained by public, replicable operations" (Harris in Borofsky 1994: 64). Furthermore, "the aim of scientific research is to formulate explanatory theories which are . . . testable (or falsifiable) . . . [and] cumulative within a coherent and expanding corpus of theories."

Boas wrote, “when we have cleared up the history of a single culture and understand the effects of environment and the psychological conditions that are reflected in it, . . . we can then investigate in how far the same causes . . . [are] at work in the development of other cultures” (Boas 1940 [1896]:279).

Kroeber book *Configurations of Culture Growth* (1969 [1944])
Steward, *Theory of Culture Change* (1955)

Weiner writes, “although Malinowski and I were in the Trobriands at vastly different historical moments and there also are many areas in which our analyses differ, a large part of what we learned in the field was similar. From the vantage point that time gives me, I can illustrate how our differences, even those that are major, came to be.” She states, “my most significant . . . departure from [Malinowski] . . . was the attention I gave to women’s productive work.” “My taking seriously the importance of women’s wealth not only brought women . . . clearly into the ethnographic picture [which was not the case in Malinowski’s accounts] but also forced me to revise many of Malinowski’s assumptions about Trobriand men” (Weiner 1988:5). Building on Malinowski’s classic writings, in other words, Annette Weiner was able to extend and refine an analysis of Trobriand society. Readers might wish to consult Weiner’s own ethnography as well (Weiner 1976).

Marshall Sahlins’ *Social Stratification in Polynesia*, his doctoral dissertation at Columbia University, offers another example of this standard. Building on accounts of individual Polynesian atolls, Sahlins was able to perceive a generalized pattern of social organization on many atolls. The atolls have, he writes, “a number of
interlocking social groups, each dedicated to the exploitation of a particular resource or resource area” (Sahlins 1958:245).

Leonard Mason, another Pacific specialist, subsequently took Sahlins’s account of Polynesian atolls and applied it to Micronesian atolls. He found that Micronesian atolls responded in a related way to many of the same pressures (Mason 1959).

3.3

As Mead recollected in her autobiography, Blackberry Winter, she went to the East Sepik region of Papua New Guinea “to study the different ways in which cultures patterned the expected behavior of males and females” (Mead 1972:196). She reasoned that “if those temperamental attitudes which we have traditionally regarded as feminine – such as passivity, responsiveness, and a willingness to cherish children – can so easily be set up as the masculine pattern in one tribe, and in another be outlawed for the majority of the women . . . we no longer have any basis for regarding such aspects of behavior as sex linked” (Mead 1950 [1935]:190). In the East Sepik, she “found three tribes all conveniently within a hundred mile area. In one [the Arapesh], both men and women act as we expect women to act – in a mild parental responsive way; in the second [the Mundugumor], both act as we expect men to act – in a fierce initiating fashion; and in the third [the Tchambuli], the men act according to our stereotype for women – are catty, wear curls, and go shopping, while the women are energetic, managerial, unadorned partners” (Mead 1950 [1935]:6).

These data led Mead to conclude that overall “male and female personality are socially produced” (Mead 1950 [1935]:210).

She [Cora du Bois] framed her analysis in terms of the Freudian psychiatrist Abram Kardiner’s assertion that certain primary institutions, such as economic organization, shaped child-rearing practices resulting in certain adult personality traits, which in turn shaped certain secondary institutions, such as religion and myth – “that given certain early and consistent conditioning during childhood (primary institutions) whether in terms of patterned child-rearing practices or the socio-economic environment, the individuals so conditioned would developed [sic] a configuration of adaptive mechanism (ego mechanism or mechanism of defense) that would, if not actually create, at least invest with high symbolic significance other (secondary) institutions in the society . . . predominately in the realm of myth and religion” (Du Bois 1961 [1944]:xxi-xxii).

Du Bois discovered that a mother’s economic responsibilities – taking care of the family’s horticulture gardens – meant she spent relatively little time with an infant child during the day. The child was mostly the responsibility of an older sibling who, often as not, was inconsistent in disciplining the child. Instead of offering love and
affection, the older child often emphasized ridicule and teasing. This sort of child rearing, Du Bois suggested, resulted in an emotionally shallow adult with limited self-confidence who distrusted deep relationships. The psychological test data were analyzed independent of Du Bois to avoid biasing the results. They confirmed Du Bois’s impressions of Atimelanger personality, thereby strengthening her assessment. Kardiner, who analyzed the autobiographies, noted, for example, that “all the men [examined] are equally envious and greedy . . . [and conceal their feelings] with lies and misrepresentations. With the exception of Fantan all are . . . devoid of self-confidence” (Du Bois 1961 [1944]:549). Oberholzer, who analyzed the Rorschach tests, described the Atimelangers as “suspicious and distrustful . . . not only toward everything that is unknown and new . . . but also among themselves. Not one will trust another” (Du Bois 1961 [1944]:596). Fitting with this pattern (and perpetuating it), Atimelang men were involved in a precarious status-wealth system based on chicanery and deceit. In accord with Kardiner’s perspective, the Atimelangers frequently framed relations with supernatural beings in terms of manipulation and exploitation.

When one looked closely at the various interpretations of the interpreters, did they all agree? Where Kardiner found much neurotic anxiety, Oberholzer found little. Du Bois, reflecting on her study “two decades later” wrote: “it seems highly probable that the range of trait distributions with any moderately complex society is greater than multimodal differences between societies” (Du Bois 1961 [1944]:xx) - that there may be as much diversity within as between groups. And there remained the question of whether certain cultural institutions did indeed engender certain personalities. In her review of The People of Alor, Powdermaker doubted whether the absence of the mother in infancy, common among Melanesian horticultural groups, would necessarily foster being perceived as a frustrating object by the child (Powdermaker 1945:160). Barnouw added that during the dry season, with less horticultural work, the mother likely spend considerably more time with her infant (Barnouw 1963:116).

Anthony Wallace addressed Du Bois’s sampling problem in a study of the Tuscarora Indians in upstate New York. Focusing on a Tuscarora reservation of roughly 600 people near Niagara Falls – a group perceived as being culturally homogeneous – Wallace found significant variation in personality traits (as judged by Rorschach protocols). Only 37 percent of the seventy interviewed possessed what might be termed a modal, or average, personality – their collective responses (in terms of twenty-one identifiable Rorschach categories) fell within a modal range. Another 23 percent fell within the modal range for some Rorschach categories but not others. Wallace referred to these as “submodal.” And a final 40 percent fell completely outside the model range. These he called “deviant.”

The results (published in The Modal Personality Structure of the Tuscarora Indians in 1952) led Wallace to make a critical distinction in a later book, Culture
Personality (1961). He differentiated between two models for conceptualizing the “relation between cultural systems and personality systems.” The first he termed “the replication of uniformity” in which “the society may be regarded as culturally homogeneous and the individuals will be expected to share a uniform nuclear character . . . [what researchers needed to study were the] . . . mechanisms of socialization by which each generation becomes, culturally and characterologically, a replica of its predecessors” (Wallace 1961:27). In “the organization of diversity,” the second model, a group’s psychological diversity was stressed, and researchers needed to examine “how . . . various individuals organize themselves culturally” given such diverse personalities. This second model, Wallace indicated, emphasized “when the process of socialization is examined . . . it becomes apparent that . . . it is not a perfectly reliable mechanism for [cultural] replication” (Wallace 1961:27-28).

Affirming this position, Wallace suggested that orderly relationships derived less from shared behaviors than from a capacity for mutual prediction (1961:28). “Advocates of togetherness may argue, whether or not it is necessary that all members of society share all cognitive maps, they must share at least one.” But, he suggested, many social systems “simply will not ‘work’ if all participants share common knowledge of the system” (Wallace 1961:37, 40).

One of the key figures in the field, George Spindler, in reviewing Culture and Personality, suggested that Wallace had unnecessarily dichotomized the issue (Spindler 1955a:1321-1322).

Decades later, working from a different perspective, two anthropologists did revisit one of the fieldsites Mead worked at, Tchambuli, calling them Chambri. But they addressed a set of problems different from those addressed by Mead (Gewertz and Errington. 1991).

“The Six Cultures Project” published by John and Beatrice Whiting in the early 1960s is probably the most comprehensive Culture and Personality research project ever undertaken (Whiting 1963; Whiting, et al. 1966). Rather than returning to any of the above studied sites, the six researchers chose new locations for their research. It is interesting to note that the key books describing the project’s theoretical and ethnographic underpinnings do not serious engage with any of the above authors’ writings, even though Du Bois worked in the same department (at Harvard) as the Whitings.

In respect to disruptive problems encountered in analyzing their data, Minturn and Lambert write, in reflecting on questions needing to be addressed in the future, “our message to anthropologists . . . is not to ignore the precise measurement of individuals that specifies variation of behavior among people who share a common culture” (Minturn and Lambert 1964:293).
The Whitings concluded from their work that “the question of the correspondence of . . . variables within and across cultures remains unanswered” (Whiting and Longabaugh 1975:135).

3.4

Marshall Sahlins and Elman Service’s *Evolution and Culture* (1960) helped initiate this trend. Their purpose, they stated in the book’s introduction, “is not to describe the actual evolution of culture, but rather to argue in favor of several general principles that we believe are fundamental to the theory of cultural evolution” (1960:1). Chapters in *Evolution and Culture* discuss “The Principle of Stabilization,” “The Law of Cultural Dominance,” and “The Law of Evolutionary Potential.” The most cited chapter involves Sahlins’s effort to bring White’s and Steward’s differing approaches into a common framework. Sahlins saw White’s work as involving general evolution and Steward’s as concerning specific evolution. Sahlins wrote, “general cultural evolution . . . [involved the] passage from less to greater energy transformation. . . . Specific evolution is the . . . ramifying, historic passage of culture along its many lines, the adaptive modification of particular cultures” (1960:38). Where specific evolution might be perceived as adaptation moving from more homogeneous structures toward more heterogeneous ones, general evolution involved a progressive movement toward “all-around adaptability” (see 1960:16, 37). Drawing on an idea from Steward, Sahlins suggested a proposition that was later elaborated by Service: Different societies had different levels of social integration (1960:21-22).

In *Primitive Social Organization: An Evolutionary Perspective* (1962), Service described three stages of cultural evolution – bands, tribes, and chiefdoms. One might note Service helped establish the popularity of these perceived stages - bands, tribes, chiefdoms, states - through his 1958 textbook (Service 1958) and *Primitive Social Organization* this present work, cf. Ortner (Ortner 1984:132). He suggested that each stage had its own distinct form of social integration. He wrote that hunters and gatherers formed cohesive bands as a result of “familistic bonds of kinship and marriage which by their nature can integrate only the relatively small and simple societies that we call bands.” Tribal social solidity derives from certain “pan-tribal solidarities which can integrate several bandlike societies” into one unit. Chiefdoms, by contrast, involve “specialization, redistribution, and the related centralization of authority” (1962:181). (Service contrasted chiefdoms with states that have “a bureaucracy employing legal force.”)

What did their colleagues make of these ideas? The *American Anthropologist* reviews of *Evolution and Culture* and *Primitive Social Organization* focused on a perennial problem with the trend. The review of *Evolution and Culture* claimed that “the authors haven’t seriously investigated any of [the] principles [discussed], nor do they give any
sign of an intention to do so, nor do they express any diffidence about the usefulness of their untested concepts or the validity of their untested hypotheses” (Naroll 1961:390). The review of *Primitive Social Organization* stated: “Chapters 3, 4, and 5 dealing with bands, tribes, and chiefdoms . . . [are mostly] devoted to defining the criteria of the specific levels [of integration]. This leaves little [room for] . . . more than a listing of . . . examples with discussion devoted primarily to problem cases. The reader, in effect, is left to accept the type societies on authority” (Lane 1964:152). Not that Service did not warn readers: “This book is rife with speculation and conjecture,” he writes in the first chapter. And in concluding the book, Service adds “what seem to be needed are more evolutionary studies designed to reveal those things that actually are related, for only in the course of evolutionary change are such functional connections fully revealed” (Service 1962:10, 184

Roy Rappaport’s *Pigs for the Ancestors* (1968) approached cultural adaptation differently, emphasizing the regulatory role of ritual in maintaining a community’s ecological viability (Rappaport 1984 [1968]:224, 374, 380 ff.). He perceived the 200 Tsembaga he studied in highland New Guinea as more than simply a cultural collectivity. He viewed them as a biotic community that sought to stay in balance with its environment.

Addressing the flaw in the two works just cited, Rappaport focused on the specific ethnographic “processes by which systems maintain their structure.” He summarized his thesis this way: “The regulatory function of ritual among the Tsembaga . . . helps to maintain an undegraded environment, limits fighting to frequencies that do not endanger the existence of the regional population, adjusts man-land ratios, facilitates trade, distributes local surpluses of pig . . . and assures people of high-quality protein when they most need it” (Rappaport 1984 [1968]:224).

[Clarifying these remarks, in a subsequent commentary written 16 years later, he adds: “the aim of the book was not to account for either the presence or the origin of the ritual cycle, and it was not asserted that the regulatory functions ascribed to the cycle could not be fulfilled by other mechanisms. The ritual cycle was taken as a given and the aim of the book was simply to elucidate its place in the operation a particular system during a particular period in its history” (Rappaport 1984 [1968]:354). Rappaport emphasized he was more concerned with “the processes by which systems maintain their structure” rather than with adaptive “change in response to environmental pressures” (Rappaport 1984 [1968]:241, cf. 414).

Criticisms of *Pigs for the Ancestors* centered on the degree to which Rappaport moved beyond simply suggesting that rituals had certain ecological functions to actually demonstrating it. Even today, the book remains a descriptive tour de force. But many critics feel that Rappaport never proved the postulated cause-and-effect relationships between ritual and environment. They perceive them as mostly conjecture. He accepted Friedman’s criticism that “while it is valid to *describe* . . . the ritual as operating to keep the pig population below a certain level, it is incorrect to claim that it is a homeostat [or regulator]. . . when no relation has been shown to exist
between the limit and the triggering of the cycle.” He continued, “I showed no intrinsic relation between women’s labor [which was asserted to be a key element in precipitating the kaiko ritual] and carrying capacity [the reputed triggering mechanism for the ritual to rebalance human-pig populations], and there may not be any” (Rappaport 1984 [1968]:334,406) – see Friedman 1974; Strathern 1985; Watson 1969.

Rappaport himself came to realize that the explanatory power of his ecological formulation was, as he phrased it, “exaggerated” (Rappaport 1984:334).

Marvin Harris, in The Rise of Anthropological Theory (1968), wrote a history of the discipline emphasizing ecological/evolutionary theory (which he termed “cultural materialism”). One need only read Lowie’s 1937 History of Ethnological Theory to see how it had been marginalized by the Boasians in their tellings and re-tellings of the discipline’s history. “The reader. . . [is] forewarned that, while this book is a history of anthropological theories, it is intended to prove a point rather than to provide an encyclopedia inventory of all the figures who have in any degree ever made suggestions concerning the causes of sociocultural phenomena,” Harris writes (Harris 1968:5). “The essence of cultural materialism” is that it directs attention to the interaction between behavior and environment [and emphasizes] . . . that group structure and ideology are responsive to . . . material conditions” (Harris 1968:659, see also 4, 520). Harris continues: “The vindication of the strategy of cultural materialism . . . lies in the capacity of the approach to generate major explanatory hypotheses which can be subjected to the tests of ethnographic and archaeological research” (Harris 1968:687). Harris, Cows, Pigs, Wars, and Witches (1974), Cannibals and Kings (1977). Harris published his book in 1968, at a critical time in anthropology. With the discipline expanding in the late 1960s, a new recounting of its past was needed to convey its new sense of respectability. Harris’s account filled this need in a way that thrilled supporters of the evolutionary approach.

Rather, Harris seeks to turn Rappaport’s analysis on its head by making ecological concerns – particularly population pressure and land carrying capacity – the reasons for certain rituals, even though, as Harris admits, the ritual occurs well before “the onset of actual nutritional deficiencies or the actual beginning of irreversible damage to the environment.” (1974:66).

“There is not great mystery,” he asserts, regarding how this kaiko became part of Tsembaga life: “As in the case of other adaptive evolutionary novelties, groups that invented or adopted growth cutoff institutions survived more consistently than those that blundered forth across the limit of carrying capacity” (Harris 1974:66). Extensive diachronic data - that showed how the kaiko ritual varied with ecological conditions through time - would buttress Harris’s suggestion. But he offers none.
Rappaport briefly cites Sahlins and Service in a critical footnote (Rappaport 1984 [1968]:xi) and adds a citation to Harris in the revised 1984 edition of *Pigs for the Ancestors* (Rappaport 1984 [1968]:350). But he ignores Harris’s explanation in *Cows, Pigs, Wars, and Witches*, and doesn’t even list the book in his 1984 bibliography.

Harris briefly discusses Sahlins’s analysis in *Evolution and Culture* of general and specific evolution, but he is critical of the analysis, asserting that a different approach would be better (Harris 1968:651-653).

<table>
<thead>
<tr>
<th>TO WHAT DEGREE DID ANTHROPOLOGISTS FOLLOWING THESE THREE AUTHORS STRIVE TO SERIOUSLY BUILD ON THEIR WORK? (The dates in parentheses represent the years the books were published or republished as new editions.)</th>
<th>Service (1962/71)</th>
<th>Rappaport (1968/84)</th>
<th>Harris (1968)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Number of citations examined in five leading journals 5/10/15 years following dates of publication.</strong></td>
<td>24</td>
<td>21</td>
<td>16</td>
</tr>
<tr>
<td><strong>A sustained attempt to DEVELOP one of the three authors’ work in a specific way (through reinterpreting, building on, or criticizing their respective work). Involves at least three sentences of discussion of the specified author’s work.</strong></td>
<td>4 percent (Snow 1969)</td>
<td>5 percent (Hallpike 1973)</td>
<td>0 percent</td>
</tr>
<tr>
<td><strong>A sustained attempt to DISCUSS one of the three authors’ work – in a review of the literature – before presenting the anthropologist’s own perspective. Involves at least three sentences of discussion of the specified author’s work.</strong></td>
<td>8 percent (Gibbon 1972; Hines 1977)</td>
<td>5 percent (Kim 1994)</td>
<td>0 percent</td>
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</table>
Relevant author’s work CITED ONLY IN PASSING as a way of demonstrating the anthropologist’s own competence and credibility – by indicating her or his familiarity with the relevant literature. Often embedded in a list of citations, but may be referred to separately in one or, on a rare occasion, two sentences in passing as the anthropologist is developing a certain point.

<table>
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<tr>
<th>88 percent</th>
<th>90 percent</th>
<th>100 percent</th>
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</table>

Rappaport Festschrift: A sustained attempt to DEVELOP one of the three authors’ work in a specific way (through reinterpreting, building on, or criticizing their respective work). Involves at least three sentences of discussion of the specified author’s work.: (Gezon 1999; Kottak 1999; Wolf 1999). A sustained attempt to DISCUSS one of the three authors’ work – in a review of the literature – before presenting the anthropologist’s own perspective. Involves at least three sentences of discussion of the specified author’s work: (Biersack 1999a; Ernst 1999). Relevant author’s work CITED ONLY IN PASSING as a way of demonstrating the anthropologist’s own competence and credibility – by indicating her or his familiarity with the relevant literature: (Biersack 1999b). I left out from the categorization percentages Watanabe and Smuts (1999) because while I viewed it as belonging with the “sustained attempt to DEVELOP” category, it did not focus on Pigs for the Ancestor. I also left out Brosius (1999b) in fairness because it was dealing, again, with a different focus than Pigs for the Ancestor. It seemed reasonable not to obscure the categorization percentages by casually including articles that did not precisely fit the categorization criteria.

In inventing structuralism, a prominent American anthropologist suggested, Lévi-
Strauss created “the only genuinely original social science paradigm . . . in the
twentieth century” (Ortner 1984:135).

In respect to myths, the topic dealt with here, he argues that “the true constituent
units of a myth are not isolated relations but bundles of such relations and it is only as
bundles that these relations can be put to use . . . to produce a meaning” (Lévi-
Strauss 1963:211).

Lévi-Strauss sets out his approach in Structural Anthropology (Lévi-Strauss 1963)
and then elaborates on it first in “the Myth of Asdiwal” (Lévi-Strauss 1967) and later
far more extensively in a four volume study of Amerindian myths. The first volume,
The Raw and the Cooked (Lévi-Strauss 1969), has achieved particular renown -
partly because it clearly illuminated his approach in all its complexity and partly
because of a provocative “overture” outlining the relation of his approach to musical
analysis: “My intention,” he writes, is “to treat the sequences of each myth, and the
myths themselves in respect of their reciprocal interrelations, like the instrumental
parts of a musical work and to study them as one studies a symphony” (Lévi-Strauss
1969:26).

Lévi-Strauss viewed myth in terms of a musical composition in which, as in music,
different variants of the myth represent variations on an underlying theme –
frequently addressing a particular social contradiction. He suggested that myths don’t
usually resolve such contradictions. Rather, a myth and its variants tend to soften a
contradiction’s polarizing tensions in a way that allows people to better cope with
them.

[In the “Story of Asdiwal,” Lévi-Strauss writes, “all the paradoxes conceived by the
[Tsimshian] . . . mind, on the most diverse planes: geographic, economic, sociological
. . . are, when all is said and done, assimilated to that less obvious yet so real
paradox which marriage with the matrilateral cousin attempts but fails to resolve. But
the failure is admitted in our myths, and there precisely lies its function” (Lévi-Strauss
1967:27-28). What Lévi-Strauss does with a special brilliance is “to reduce apparently
arbitrary data [in, I would add, overwhelming quantities] to some kind of order”
through such structural analyses (Lévi-Strauss 1969:10).]

In the “Overture” to The Raw and the Cooked (1969) Lévi-Strauss addressed a
question frequently raised by his critics: Whose “order” was being represented in his
analyses – his own, or that of the people he was studying? Lévi-Strauss offered an
intriguing answer. “If the final aim of anthropology is to contribute to a better
knowledge of objectified [human] thought . . . it is in the last resort immaterial whether
in this book the thought processes of the South American Indians take shape through
the medium of my thought, or whether mine take place through the medium of theirs.
What matters is that the human mind, regardless of the identity of those who happen to be giving it expression, should display an increasingly intelligible structure as a result of the doubly reflexive forward movement of two thought processes acting one upon the other” (Lévi-Strauss 1969:13).

In the early 1900s, the Belgian anthropologist Arnold Van Gennep had postulated three stages to rituals of passage (such as those concerned with the change from a child to an adult). These were separation (i.e., exclusion from the society), margin (i.e., liminality), and aggregation (i.e., reintegration into the society). Turner built on Van Gennep’s work focusing on the middle or liminal state, which he felt embodied an anti-structural quality – a way of relating distinct from the demarcations and separations of normal social structures. “The liminal group,” he wrote, “is a community . . . of comrades and not a structure of hierarchically arrayed positions. This comradeship transcends distinctions of rank, age, [and] kinship position” (1967:100). (see also The Ritual Process (Turner 1969). Turner perceived “two alternative ‘models’ for human interrelatedness, juxtaposed to one another. The first involved society as a structured, differentiated, and often hierarchical system of politico-legal-economic positions. . . . The second, which emerges recognizably in the liminal period, is of society as an unstructured or rudimentarily structured and relatively undifferentiated communitas, community, or even communion of equal individuals” (1969:96). He suggested that for both religious and secular groups, “a fairly regular connection is maintained between liminality, structural inferiority, lowermost status, and structural outsiderhood . . . [involving] . . . such universal human values as peace and harmony . . . fertility, health of mind and body, universal justice . . . and brotherhood” (1969:134).

In concluding The Ritual Process, Turner suggested that society “seems to be a . . . dialectical process with successive phases of structure and communitas. There would seem to be – if one can use such a controversial term – a human ‘need’ to participate in both modalities” (1969:203). It is a powerful vision that gave new impetus to the study of rituals, especially when Turner extended his approach, as he did in later work, to include historical events in Western societies. (He analyzed Saint Francis of Assisi, Thomas Becket, and the Hell’s Angels (Turner 1969; Turner 1974).

But Turner fell prey to the same criticisms as Lévi-Strauss did regarding why we should trust his interpretations. Turner couldn’t demonstrate that his analyses were anything more than symbolic guessing. (Regarding symbolic guessing, see Turner 1967:26.) Gluckman, his former teacher, suggested that structure/anti-structure distinction was too rigid, for example. The two were more entwined, one with the other, than Turner suggested (Gluckman and Gluckman 1977:242) (see also Deflem 1991:19). Schwartz observed in his American Anthropologist review, that “Ritual - the situation from which Turner derives communitas via liminality - is usually not spontaneous or unstructured, nor is it ungoverned by norms” (Schwartz 1972:906). Morris suggests that Turner often missed “the ideological nature of religious
symbolism” - meaning the political dynamics enacted through and embedded in rituals (Morris 1987:259).

== An Interesting Note:

In a quirk of history, Turner first discovered Van Gennep’s work in the Hastings, England Public Library while waiting for an American visa to take up a position at Cornell (Deflem 1991:7).

Believing that humans are caught up in webs of meaning that they themselves shape as well as are shaped by, Clifford Geertz sought to interpret the webs of meaning that bind people together in groups. Geertz, who conducted fieldwork in sites ranging from Bali to Morocco, has been described as “one of the foremost figures in the reconfiguration of the boundary between the social sciences and the humanities in the second half of the twentieth century” (Ortner 1999:1).

Lindholm asserts, “Clifford Geertz is undoubtedly modern America’s best-known, most quoted, and most intellectually influential cultural anthropologist. . . . he has found an admiring audience in disciplines as diverse as history, literary theory, and philosophy” (Lindholm 1997:214). We might also take note of another distinguishing feature: No other cultural anthropologist, to my knowledge, has ever won the National Book Critics Circle award.

Let me focus here on his most famous volume, The Interpretation of Cultures and, specifically, on its two most famous chapters, “Thick Description” and “Deep Play” (which covers Balinese cock fighting). “When scholars from the humanities . . . cite an anthropologist,” one anthropologist noted, “it is more often than not Geertz . . . and usually it is Geertz on thick description or Geertz on the cockfight” (Spencer 1996:538). (Intriguingly, the American Anthropologist never reviewed The Interpretation of Cultures.)

Geertz writes in a famous passage, “Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs and the analysis of it to be . . . not an experimental science in search of law but an interpretive one in search of meaning” (1973:5).

In discussing “thick” description, Geertz suggests, “it is not against a body of uninterpreted data, radically thinned descriptions, that we must measure the cogency of our explications, but against the power of the scientific imagination to bring us into touch with the lives of strangers” (1973:16).

Geertz interprets the Balinese cockfight as a dramatization of status concerns and fears enacted on a public stage. He writes: “Attending cockfights and participating in them is, for the Balinese, a kind of sentimental education. What he [the Balinese]
learns there is what his culture’s ethos and his [private] sensibility . . . look like when spelled out externally in a collective text” (1973:449). Building on this point, Geertz writes, “the culture of a people is an ensemble of texts . . . which the anthropologist strains to read over the shoulders of those to whom they properly belong” (1973:452).

“Cultural analysis is (or should be) guessing at meanings,” Geertz suggests. But there remains the question of how we should judge his guesses. In respect to his Balinese material, other anthropologists have challenged Geertz’s interpretation on a number of points. One writes: “When Geertz discusses the aims and nature of interpretive theory, he seems more interested in possibility than in tangibility” (Shankman 1984:264). Readers are left – as with Lévi-Strauss and Turner – wondering why one particular interpretation is more valid than another.

Ortner suggests: “Attacked by the positivists for being too interpretive, by the critical studies scholars as being too politically and ethically neutral [cf. Keesing 1987], and finally by the interpretivists . . . as being to invested in a certain concept fo culture, Geertz has frequently gotten it from all sides” (Ortner 1999:1).

“The inability of interpretive theory to offer criteria for criteria for evaluating . . . different interpretations . . . poses a formidable barrier to claims of theoretical superiority” over more positivist orientations (Shankman1984:265).

In a critique of Lévi-Strauss, Geertz writes: “That Lévi-Strauss should have been able to transmute the romantic passion of Tristes Tropiques into the hypermodern intellectualism of La Pensée Savaoge is surely a startling achievement. But there remain questions one cannot help but ask. Is this transmutation science or alchemy? . . . Is Lévi-Strauss writing . . . a prolegomenon to all future anthropology? Or is he, like some uprooted neolithic intelligence cast away on a reservation, shuffling the debris of old traditions in a vain attempt to revivify a primitive faith whose moral beauty is still apparent but from which both relevance and credibility have long since departed” (Geertz 1973:359). Geertz remarks that Turner “can expose some of the profoundest features of social process, but at the expense of making vividly disparate matters look drably homogeneous” (Geertz 1983:28). Lévi-Strauss criticizes Turner, noting “ritual is not a reaction to life; it is a reaction to what thought has made of life. It is not a direct response to the world . . . it is a response to the way man thinks of the world” (Lévi-Strauss 1981:681). From the literature I have examined, Lévi-Strauss appears not to have engaged with Geertz - cf. (Shankman 1984:86).

Turner is the most positive of the three. He affirms his agreement with Geertz on certain points (Turner 1975:147). and acknowledges Lévi-Strauss’s criticism of his work (Turner 1975:147-148).
TO WHAT DEGREE DID ANTHROPOLOGISTS FOLLOWING THESE THREE AUTHORS STRIVE TO SERIOUSLY BUILD ON THEIR WORK? (The dates in parentheses represent the years the books were published or republished as new editions.)

<table>
<thead>
<tr>
<th>Number of citations examined in five leading journals 5/10/15 years following dates of publication.</th>
<th>Lévi-Strauss (1969)</th>
<th>Turner (1969)</th>
<th>Geertz (1973)</th>
</tr>
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<tbody>
<tr>
<td>17</td>
<td>18</td>
<td>38</td>
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</table>

A sustained attempt to DEVELOP one of the three authors' work in a specific way (through reinterpreting, building on, or criticizing their respective work). Involves at least three sentences of discussion of the specified author's work.

| 18 percent (Gould 1978; Hage 1979; Spiro 1979) | 6 percent (Holloman 1974) | 5 percent (Asad 1983; Keller 1983) |

A sustained attempt to DISCUSS one of the three authors' work – in a review of the literature – before presenting the anthropologist's own perspective. Involves at least three sentences of discussion of the specified author's work.

| 13 percent (Guindi and Read 1979; Utley 1974) | 6 percent (Hazan 1984) | 5 percent (Chaney 1978b; Kennedy 1978) |
Relevant author’s work CITED ONLY IN PASSING as a way of demonstrating the anthropologist’s own competence and credibility – by indicating her or his familiarity with the relevant literature. Often embedded in a list of citations, but may be referred to separately in one or, on a rare occasion, two sentences in passing as the anthropologist is developing a certain point.

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<th>69 percent</th>
<th>89 percent</th>
<th>89 percent</th>
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3.6

Among the early works were: Wissler The American Indian (1917), Kroeber Cultural and Natural Areas of North America (1939), and Kroeber and Driver Quantitative
Expression of Cultural Relationships (1932) as well as the Kulturkreis analyses of Graebner’s Methode der Ethnologie (1911). There were also important acculturation studies such as Mooney's The Ghost-Dance Religion and the Sioux Outbreak of 1890 (1965 [1896], Linton's Acculturation in Seven American Indian Tribes (1940), and Wilson and Wilson's The Analysis of Social Change, Based on Observations in Central Africa (1945). ). Redfield's The Primitive World and Its Transformations (Redfield 1957), and Spindler's Sociocultural and Psychological Processes of Menomini Acculturation (Spindler 1955b). We should also acknowledge the ethnohistorians such as Fenton’s studies of the Iroquois (Fenton 1941; Fenton 1957; Fenton 1987; Fenton 1998) and Vansina’s studies of Central Africa (Vansina 1961; Vansina 1966; Vansina 1968).

We might highlight two works that few cite today but in their time thrilled the field: Wissler’s 1914 “The Influence of the Horse in the Development of Plains Culture” and Richardson and Kroeber’s 1940 Three Centuries of Women’s Dress Fashions. Finally, readers of Wolf (1982) might appreciate knowing that as far back as 1908 Bernheim asserted: “primitive peoples . . . should be a subject of history. There are no peoples without history” in (Thomas 1956:169 -- quoted by Koppers).

Michel Foucault:

The French scholar Michel Foucault used history to discover how submerged structures of power shaped important Western (especially French) institutions – from the treatment of the insane to the development of the medical profession to patterns of imprisonment. While Foucault wasn’t technically an anthropologist, he had an air “of the anthropologist about him, [involving] a greater attention to the fit of institutions in a society than to the dynamics of change” (Rothman 1978:1).

“At the beginning of the nineteenth century,” Foucault writes, “the great spectacle of physical punishment disappeared” (Foucault 1979 [1977]:14). Torture stopped. It was replaced “by a punishment that acts in the depth on the heart, the thoughts, the will, the inclinations” – the “soul rather than the body” he states quoting Maltby (1979:16). Foucault framed what he perceived as changes in the “political economy of the body”—how the body was controlled and regulated by others – in terms of changes in the political economy of society (1979:25, 77, 87). “Is it surprising,” he asks in respect to the rise of capitalist orientations in industry, “that prisons resemble factories, schools, barracks, [and] hospitals” in respect to the rise of new disciplined regimes such as organizing behavior according to regimented time schedules? (1979:228) Such “discipline produces subject and practised bodies, ‘docile’ bodies,” Foucault writes. “Discipline increases the forces of the body (in economic terms of utility) and diminishes these same forces (in political terms of obedience)” (1979:138). These broader and more continuous control mechanisms of discipline can be perceived as the ‘dark side’ of the bourgeoisie rise to power (1979:22). Control is more subtle and, at the same time, more all pervading. The politics of such discipline regimens comes clear in the fact that “prisons do not diminish the crime rate” (1979:265). Rather prisons - or more precisely those who move in and out of them in cyclic patterns - differentiates off a set of acts and actors, from the larger mass of workers, who it then
marginalizes and, at the same time, makes more observable. “The [in]carceral texture
of society assures both the real capture of the body and its perpetual observations; it
is, by its very nature, the apparatus of punishment that conforms most completely to
the new economy of power and the instrument for the formation of knowledge that
this very economy needs” (1979:304, see also 224).

Reviewers repeatedly highlight two problems regarding Discipline and Punish. First,
Foucault tended to work by analogy, showing how similar processes operated in
different areas of French society rather than demonstrating historical or causal
connections between the two. To quote Rothman again, “he constructs his argument
not by mapping precise lines of influence from one institution to another but by
defining broad similarities of approach” (1978:26).

Goldstein writes “the usual criteria of historical scholarship cannot be used to assess
[Foucault’s work] . . . it is more like metahistory. The analysis is highly abstract, as
befits its vast scope, and Foucault never marshals the concrete and specific evidence
that historians crave” (Goldstein 1979:117).

Second, Foucault asserted, “where there is power there is resistance” (Smart
1985:77). While readers can find suggestions of resistance - in the public disruptions
of royal hangings, for example - the resistance he describes tends to be at a vague,
abstract level. We don’t see real people taking real actions. A reviewer observed,
“Foucault’s is a history without significant actors, a history filled with disembodied . . .
forces” (Goldstein 1979:117).

[Related to this, Foucault does not examine how the controls of the prison work out in
practice (or the larger discipline in the larger society). Shelley notes, citing Sykes’s
Society of Captives, “the prison fails to consolidate its control because the population
is unwilling and the rulers are defective” (Shelley 1979:1510). Control in prisons -
then and now - was (and is) far more open, far more negotiated, than Foucault
portrays it to be. In offering a clear, indeed brilliant, vision of a key transformation in
Western history, Foucault then touches only lightly on the actual processes and
causes involved and the forms of resistance engendered by it. Perhaps it is for this
reason that his English translation (but not the original French version) ends with the
assertion: his book “serve[s] as a historical background to . . . studies of . . . power . . .
and the formation of knowledge in modern society” (1979:308).

In Europe and the People Without History (1982), Eric Wolf demonstrated that non-
Western groups did not lack history in our sense of the term. In fact, for over 500
years their history has been part of our history – they have been entwined with
Western systems of trade and exchange. As Worsley noted in his review of Europe
never be quite the same” (Worsley 1984:170). Asad provides a fair summary of
Wolf’s project: “to demonstrate that societies typically studied by anthropologists have

been continuously changed over the past five centuries by global political-economic forces. Two explicit assumptions are made: first, that no society is completely self-contained or unchanging, and, second, that a proper understanding of societal linkages and transformations must start from an analysis of the material processes in which all social groups are involved - the production, circulation, and consumption of wealth" (Asad 1987:594; cf. Wolf 1982:390).

Take, for example, the early American fur trade. Wolf writes: “wherever it went, the fur trade brought with it contagious illness and increased warfare. Many native groups were destroyed, and disappeared entirely; others were decimated, broken up, or driven from their original habitats. Remnant populations sought refuge with allies or grouped together with other populations, often under new names and ethnic identities” (1982:193). In respect to the African slave trade, Wolf observes, “the Tallensi [a group described in a famous ethnography] . . . were formed from a fusion of original inhabitants of the country with immigrants headed by slave-taking chiefs” (1982:230). Wolf’s point is that the ethnographic groups anthropologists study are frequently the “outcome of a unitary historical [economic] process” (1982:230) or, phrased another way, “the global processes set in motion by European expansion constitute their history” as well as our history (1982:385).

In contrast to such world systems theorists as Gunter Frank (Frank 1970; Frank 1998) and Wallerstein (Wallerstein 1974; Wallerstein 1980; Wallerstein 1989), Wolf is interested not only in how forms of production in the European periphery effected the development of European capitalism at the European center but in the variable reactions that took shape across the breadth of the periphery from center-periphery interactions: how, in his words, “a general dynamic, capitalism . . . gave rise to a variability of its own” (1982:266). To give his wide-ranging chronological and ethnographic analysis intellectual coherence, Wolf focuses on the interactions, through time, of three “modes of production” that allow him to “deal with the spread of the capitalistic mode and its impact on world areas where social labor was allocated differently” (1982:76). The capitalist mode, he perceives as controlling the “means of production, buys labor power, and puts it to work continuously expanding surpluses by intensifying productivity through an ever-rising curve of technological inputs” (1982:78). The tributary mode entails a primary producer, such as a cultivator or herdsmen, who “is allowed access to the means of production, while tribute is exacted from him by political or military means” (1982:80). And a kinship mode involves “shareholders in social labor . . . through marriage and filiation” or, claims to resources through a “kinship license” (1982:92, 91).

The book’s problem is that, in a manner not dissimilar to Foucault, Wolf tends to discuss the abstract dynamics of capitalism rather than how real people in specific locations had their lives shaped and re-shaped by capitalistic forces. A reviewer of Europe and the People without History phrased it this way: “the work . . . ends with the movement of commodities and with the movement of people as just another commodity. There is little about the self-movements of people.” (Worlsey 1984:173).
[As Asad observes, “few anthropologists would have had the courage to write it” (Asad 1987:607). Yet it suffers from a noticeable failing: It talks far more about the localization of capitalism - in diverse locales around the world - than about the locals, the actual people, who shaped and re-shaped capitalism in the process of succumbing to it. Asad suggests “another book remains to be written . . . the story of transformations . . . within which . . . people . . . make their history” (1987:607). It is one thing to suggest that Tallensi are a cultural construction of the slave trade, for example. But where is this substantiated? (see 1982:230 vs. 412-413).

Through two key books, *Historical Metaphors and Mythical Realities* (1981) and *Islands of History* (1985), Marshall Sahlins demonstrated how bringing cultural structures together with actual historical events allows us to better understand historical events – especially in terms of indigenous responses to Western contact. Sahlins focused on Captain James Cook’s 1778-79 visit to Hawaii.

In the interactions of Europeans with Hawaiians, Sahlins suggested, was “a possible theory of history, of the relation between structure [i.e. culture] and event [i.e. history] beginning with the proposition that the transformation of a culture is a mode of its reproduction” (1985:138).

“Culture may set the conditions to historical process, but it is dissolved and reformulated in material practice . . . . The great challenge of an historical anthropology is not merely to know how events are ordered by culture,” Sahlins writes, “but how, in that process the culture is reordered. How does the reproduction of a structure becomes [sic] its transformation?” (1981:7-8).

Cook was drawn into the Makahiki, a Hawaiian ritual celebrating the New Year, during his visit to Kealakekua Bay. He was perceived, according to both British and Hawaiian accounts, as a manifestation (though not an actualization) of the Hawaiian atua Lono. (Though atua is often translated into English as “god,” it might be better phrased as conveying a sense of the divine.)

Sahlins suggested that Cook’s sudden murder, at Kealakekua Bay on February 14, 1779, “was not premeditated . . . but neither was it an accident, structurally speaking. It was the Makahiki in a historical form” in which Lono was praised with offerings and then symbolically dismissed (or killed) to return again the following year (1981:24).

Wolf wrote that an “older anthropology had little to say . . . about the major forces driving the interaction of cultures since 1493” (1982.ix). In fact a host of works within American anthropology directly addressed this issue. Wolf simply did not refer to them. One might note Mooney (1965[1896]), Barber (Barber 1941), and Hill (Hill 1944) on the ghost dance religion, or, more broadly, Service (Service 1955) and Wolf
(Wolf 1957) himself on how Europeans shaped post-conquest social organization in the Americas. Sahlins’s perspective on Cook, without the theory or details, was basically enunciated by Gavan Daws in 1968 (Daws 1968). Sahlins dismisses Daws in a footnote without giving him credit for the analysis (1981:74).

Sahlins, in *Islands of History* (which came out three years after Wolf’s *Europe and the People without History*), offers a brief one-sentence nod to Wolf’s perspective. Sahlins is concerned with Cook’s role as “Adam Smith’s global agent” – as a spreader of capitalism – but he never seriously takes up Wolf’s analysis of capitalism (1985:131). And while concerned with how Cook used “tolerance for the pursuit of domination,” Sahlins never refers to Foucault (though other writings demonstrate that he is clearly familiar with him).

<table>
<thead>
<tr>
<th>TO WHAT DEGREE DID ANTHROPOLOGISTS FOLLOWING THESE THREE AUTHORS STRIVE TO SERIOUSLY BUILD ON THEIR WORK? (The dates in parentheses represent the years the books were published or republished and in Sahlins’ case when his two separate books were published.)</th>
<th>Foucault (1977/79)</th>
<th>Wolf (1982)</th>
<th>Sahlins (1981/1985)</th>
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<tr>
<td>Number of citations examined in five leading journals 5/10/15 years following dates of publication.</td>
<td>18</td>
<td>37</td>
<td>47</td>
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<tr>
<td>A sustained attempt to DEVELOP one of the three authors’ work in a specific way (through reinterpreting, building on, or criticizing their respective work). INVOLVES at least three sentences of discussion of the specified author’s work.</td>
<td>0 percent</td>
<td>0 percent</td>
<td>2 percent</td>
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(Ohnuki-Tierney 1995)
Still, the unfolding of history isn’t always neatly ordered. In the (Re)turn to history, an exception to the pattern arose. A Princeton-based Sri Lankan anthropologist, Gananath Obeyesekere, challenged Sahlins’s interpretation of Cook’s “apotheosis” as the atua Lono (Obeyesekere 1992).

At first, few challenged Obeyesekere (to Sahlins’ frustration). Sahlins finally decided that he himself had to reply to Obeyesekere and he did so in a strongly worded book, How "Natives" Think: About Captain Cook, for Example (1995). The ensuing conflict and the literature it engendered need not concern us here. (Interested readers might
turn to Borofsky 1997.) (Because of University of Chicago Press legal fears regarding *How “Natives” Think*, I myself was paid to verify the validity of the conclusions and citations in Sahlins’s 17 appendixes.)

== On Reviews of Sahlins’ Work And the Dynamics at Play at Kealakekua Bay

Reading reviews of *Historical Metaphors* by Marcus (Marcus 1982) and of *Islands* by Friedman (Friedman 1988) and Toren (Toren 1988), I am puzzled, at times, by their critiques. Throughout his writings, Sahlins asserts the importance of historical contingencies: he emphasizes, for example, that Hawaiians were entwined with performative structures - structures that “tend to assimilate themselves to contingent circumstances . . .[and] are . . . open to negotiation.” “The cultural order reproduces itself” in Hawaii, he asserts, “in and as change” (1985:xii). Yet these reviews criticized his overly static and structural portrayals of Hawaiian ritual. They perceived, at the ethnographic level, little of the fluid, agency-led sense of ritual Sahlins affirms in the abstract. The resolution of the problem returns us to Sahlins’s need to grasp Hawaiian understandings through the formalistic accounts of later writers. Hawaiians’ agency to shape and reshape their rituals - which both Sahlins and his critics concur on - tends to get lost in the shuffle. One is trying to infer process while giving due credence to fairly static, depictions of the contact era. Still, if one looks carefully, it remains possible to see agency clearly. One of the unanimously agreed facts - if I dare use that word - surrounding the events at Kealakekua Bay is that the priests of Lono (specifically Kanekoa, Kuakahela, Ka‘ō‘ō, Keli‘ikea, and Omeah) supported the British well after Cook’s murder. In doing so, they were challenging - as an expression of their own agency - the very ritual cycle that gave them power. They were continuing their alliance with Cook, that is to say, after he was ritually out of season, out of sequence. The Makahiki might have been over, but the priests of Lono continued to demonstrate respect for Cook up to and well after his death. We see people resisting the accepted ritual sequence and thereby undermining the cultural basis for their own ritual power. (It eventually cost them; see Sahlins 1995:256.)


3.7

For a trend that rebelled against broad master narratives and valued pastiche in ethnographic writing (drawing bits and pieces from here and there together into an anthropological account) -- see e.g. Rabinow in (Clifford and George E. Marcus 1986:249-250), (Knauff 1996:68) -- the tenets of Postmodernism appear relatively coherent. One repeatedly reads about “a crisis of representation” and “uncertainty about adequate means of describing social reality” (Marcus and Fischer 1986:7, 8 ff.)

“No longer is it credible,” Fischer asserts, “for a single author to pose as an omniscient source on complex cultural settings, nor to pose those settings as distanced exotic forms without direct interaction with the author’s own society, time, and place” (Fischer 1997:370).

There is an emphasis on reflexivity - on "working into ethnographic texts a self-conscious account regarding the conditions of knowledge production as it is being produced . . . [replacing] the observational objective ‘eye’ of the ethnographer with his or her personal . . . ‘I’" (Marcus 1994:45). There is a call for intellectual experimentation in ethnographic writing. (Marcus and Fischer’s 1986 book is subtitled, An Experimental Moment in the Human Sciences.)

“What motivates [these] experiments,” Marcus suggests, “is the recognition of a much more complex world, which challenges the traditional modes of representing cultural difference in ethnographic writing” (Marcus and Fischer 1986:168).

== On Using the Term Postmodernism

The label that is repeatedly attached to the trend by others - post-modernism - turns out not necessarily to be embraced by the trend’s key figures in their key works. It is relatively easy to find comprehensive discussions of postmodernism in recent accounts of anthropological theory (e.g., Barnard and Spencer 1996; Harris 1999; Knauff 1996; Layton 1997) as well as in broad synthetic works (e.g., Barfield 1997; Barnard and Spencer 1996). But the term - if we judge by the pages cited in each book’s index - is downplayed in both Anthropology as Cultural Critique and The Predicament of Culture. In Writing Culture, Clifford intriguingly suggests “most of us [involved in the book] were not yet thoroughly ‘post-modern’” (1986:21) – cf. Pool 1991:318-319; Sangren 1988:425.

Postmodernism portrayed itself as superseding modernism, but it would be more accurate to suggest that it amplified selected modernist tendencies. Gellner notes
post-modernism’s parallels with the Romantic Counter-Enlightenment’s opposition to positivistic ideals (Gellner 1992).

One anthropologist shrewdly noted that “fragmentation, pastiche, and the juxtaposition of images and genres had been used [to question certain established Western frames of reference since] . . . at least as early as Nietzsche’s writings. In art, this trend was foreshadowed by French impressionism and then made explicit in cubist and surreal art” (Knauff 1996:16, cf. Marcus and Fischer 1986:122-125).

A prominent literary critic noted, “incompletion is the password to modernism . . . in modernism, form is not perfect act but process and incessant revision” (Steiner 1999:3). As with the (re)turn to history, the new and old very much overlap (cf. Pool 1991:310 ff., Marcus and Fischer 1986:67-8).

*James Clifford and George Marcus:*

“Anthropology’s premier postmodernist text,” Barnard suggests, was *Writing Culture*, (2000:169). James, Hockey and Dawson refer to it as “a watershed in anthropological thought” (James, et al. 1997:1). Or as Nugent noted, it involved “a benchmark publication indicating . . . possible trajectories for a postmodernist . . . anthropology” (Nugent 1996:443). The volume grew out of a 1984 School of American Research seminar.

In *Writing Culture*, editors James Clifford and George Marcus state that “by looking critically at one of the principal things ethnographers *do* - that is, write . . . [the book seeks] to come to terms with the politics and poetics of cultural representation” (1986:vii-viii).

Clifford: “Their focus on text making and rhetoric serves to highlight the constructed, artificial nature of cultural accounts (2000:2) . . . Ethnographic truths are . . . inherently partial - committed and incomplete” (Clifford 2000:7)

[May not the vision of a complex, problematic, partial ethnography lead, not to its abandonment, but to more subtle, concrete ways of writing and reading, to new conceptions of culture as interactive and historical?] (Clifford 2000:25)

Rabinow: “When corridor talk about fieldwork becomes discourse we learn a good deal. Moving the conditions of production of anthropological knowledge out of the domain of gossip - where it remains the property of those around to hear it - into that of knowledge would be a step in the right direction” (p.253).

Marcus: “more is at stake than the mere demystification of past dominant conventions of representation. Rather, such a critique legitimates experimentation and a search for options in research and writing activity” (p. 263).

[Rosaldo: The pastoral mode of writing - in the work of Evans-Pritchard and Le Roy Ladurie - “permits a polite tenderness that more direct ways of acknowledging inequality could inhibit. Its courtesy becomes respect . . . Yet the pastoral also licences patronizing attitudes of condescension, such as reverence for a simplicity ‘we’ have lost . . . the pastoral mode becomes self-serving because the shepherd
symbolizes that point beyond domination where neutral ethnographic truth can collect itself” (p. 97).

Asad: “the ethnographer’s translation/representation of a particular culture is inevitably a textual construct, that as representation it cannot normally be contested by the people to whom it is attributed . . . the process of ‘cultural translation’ is [thus] inevitably enmeshed in conditions of power” (p. 163].

“a benchmark publication indicating . . . possible trajectories for a postmodernist . . . anthropology” (Nugent 1996:443).

What is intriguing about Marcus and Fisher’s *Anthropology as Cultural Critique* (1986), the second key text, is that, while it is strongly criticized by those not embracing Postmodernism, it seems fairly conventional in both content and structure.

While it has been described as “an important contribution to the postmodernist discussion” (Birth 1990:549), as a “milestone” (Whitten 1988:733) and as a companion volume to *Writing Culture* that “instigated a wider debate” (James, Hockey and Dawson 1997:1), one has a hard time discovering - beyond such brief snippets - extended positive commentary from reviews in major anthropological journals. It appears almost universally recognized as an important book. At the same time, it is strenuously criticized (more so even than *Writing Culture*). This holds despite the fact that several writers - including many critics - acknowledge that *Anthropology as Cultural Critique* is, in many ways, a fairly conventional book. Pool writes, “the critics usually single out *Writing Culture* . . . and *Anthropology as Cultural Critique* . . . for criticism as the programmatic statements or the examples of postmodern ethnography. In fact, it is the latter book, which is perhaps the most ‘conventional’ of all the so-called postmodern anthropological texts, which receives the most attention” (1991:319).

One might divide the book into three parts.

(1) The first two chapters frame the book. Seeking to place the present “crisis of representation” in historical perspective, Marcus and Fisher write, “the current period, like the 1920s and the 1930s, . . . [is] one of acute awareness of the limits of our conceptual systems as systems” (1986:12).

[“An experimental ethnography works,” Marcus and Fischer suggest, “if it locates itself recognizably in the tradition of ethnographic writing and if it achieves an effect of innovation” (1986:40). What is sought “is not experimentation for its own sake, but the theoretical insight that the play with writing technique brings to consciousness” (1986:42).

(2) The next two chapters offer a range of illustrative experimental texts: Chapter 3 examines ways to represent “the authentic differences of other cultural subjects . . . focusing on the person, the self, and the emotions” (1986:44-46). Chapter 4 takes
“account of power relations and history within the context of their subjects’ lives” (1986:77). What is most striking, in both chapters, is the range of ethnographies included. Chapter three, for example, includes not only Geertz’s “Person, Time and Conduct in Bali”, Obeyesekere’s “Medusa’s Hair,” and Crapanzano’s “Tuhami,” but also Levy’s Tahitians, Shostak’s Nisa, Rosaldo’s “Knowledge and Passion,” and Shore’s “Sala'ilua.” Chapter four includes not only Willis’s “Learning to Labour,” Taussig’s “The Devil and Community Fetishism in South America,” and Price’s “First Time,” but also Wolf’s “Europe and the People Without History” and Sahlins’s “Historical Metaphors and Mythical Realities.”

Chapters 5 and 6 focus on anthropology as a form of cultural critique: “The juxtaposing of alien customs to familiar ones, or the relativizing of taken-for-granted concepts . . . that lend certainty to our everyday life, has the effect of disorienting the reader and altering perception . . . [but] the promise of anthropology as a compelling form of cultural critique has remained largely unfilled” (Marcus and Fischer 1986:111).

Marcus and Fischer write: “the idea of the ethnographer’s function as uncovering, reading, and making visible to others the critical perspectives and possibilities for alternatives that exist in the lives of his subjects is an attractive one. It is a function that anthropology has been performing abroad, and it should be a style of cultural criticism it could perform at home” (1986:133). Offering examples, in chapter six, of “epistemological critique,” Marcus and Fischer note Sahlins’s Culture and Practical Reason as well as Geertz’s Negara. Examples of “defamiliarization by cross-cultural juxtaposition” include Mauss’s The Gift and Mead’s Coming of Age in Samoa. The chapter ends with a call for recognizing that ethnographers, especially if they write cultural critiques, are writing for “diverse and critical readerships at home and abroad” that extend beyond the discipline itself (1986:164).

Critical comments regarding Anthropology as Cultural Critique focus on the same problems highlighted in respect to Writing Culture.

First, critics question how to evaluate the experimental ethnographies, if not in traditional ways. Marcus and Fischer list several standards for determining a “good ethnography” – a sense of fieldwork conditions, effective “translation” across cultural boundaries, and holism (1986:24-25). They even include “the value and quality of verifiable data” (1986:180). But these standards don’t appease critics. One critic writes: “Like Marcus and Fischer, I believe that anthropology is and ought to constitute a kind of reflexive cultural critique; unlike them, I believe that such a critique must emanate from a holistic and explicit allegiance to scientific values. Otherwise . . . it will succeed only in reproducing the hierarchically asymmetrical contrast between humanities and sciences that is one of the mystifying foundations of Western individualistic ideology” (Sangren 988:421).
Critics charge that Marcus and Fischer frequently downplay the politics of their own ploys while emphasizing the political ploys of others. As the above critic phrased it, "postmodernists feel free to mythologize, criticize, and demystify ‘realist’ arguments as hopelessly limited by the historical and cultural contingencies of their production while at the same time refusing to allow criticism of their own arguments on similar grounds" (Sangren 1988:421).

Second, many critics view Postmodernism and several of the examples referred to in _Anthropology as Cultural Critique_ as forms of self-absorbed “navel-gazing” focused less on their informants than on the anthropologists themselves. Jarvie writes: “Postmodernism offers navel-gazing (study of texts)” (in Sangren 1988:428). It is relevant to note that what Marcus and Fischer actually assert is this: In so far as texts “slide into simple confessionals . . . or into atomistic nihilism where it becomes impossible to generalize from a single ethnographer’s experience . . . they are of no particular ethnographic interest” (1986:68). Pool suggests, “no one in their right mind can read Marcus and Fischer’s remarks . . . and then claim they are appealing for nihilism and solipism” (1991:323). Still, the criticism persists.

== A Puzzle Regarding Anthropology as a Cultural Critique

We are left with a puzzle regarding Anthropology as a Cultural Critique. Why did a book that seems so inclusive in its praise, makes its arguments in a comparatively straight-forward fashion, and affirms the need for ethnographic experimentation without demanding the dismantling of the discipline’s traditions seem to arouse so many anthropologists’ ire? It relates to context. Like Lowie (1937) and Harris (1968) before them, Marcus and Fischer reframe the discipline’s history in terms that highlight their own orientations (cf. Hobsbawm and Ranger 1983 for this process on a nationalistic scale). Goldschmidt describes it as an “effort to recreate anthropology in their own image, to make what they have to say loom larger and more significant than it is” (Goldschmidt 1987:472). Like Lowie and Harris, Marcus and Fischer are fairly inclusive regarding who they depict as “the good guys.” Still, it was a political move that most anthropologists understood. Polier and Roseberry write: “ethnographic ‘textualism’ has become a formidable, and in some ways hegemonic, movement within American anthropology. . . . Institutionally, contributors to the movement are in a position to exercise extraordinary influence on the dissemination of ideas” (Polier and Roseberry 1989:245-246). Spencer adds, “there is every chance that it has set the agenda for self-criticism in American (if not British) anthropology for the next few years” (Spencer 1989:161). What one sees in the negative reviews is resistance to a wave that swept over the discipline in the late 1980s and into the 1990s (cf. Pool 1991:327-28).

Since today most anthropologists accept the need for critically analyzing the construction of their texts, one might say that the trend won out. But what is intriguing is how it won out. Essentially, it was done by a small, cohesive cadre -
Pool refers to Marcus, Fischer, Clifford and Tyler as the “central gang of four” (1991:319) with, we might say, Crapanzano, Rabinow, Taussig, and Geertz as fellow-travelers. One might well argue that the Boasians and the cultural ecologists carried out similar coups. But in the former cases it was more through “genealogical” ties - teachers drawing in graduate students who, in turn, drew in their students -- than through publications. What Marcus and Fischer and Clifford did, given that they were all at relatively small departments, was establish ties with a generation of younger anthropologists as well as their mentors (when possible). This makes their affirmation of such a wide range of ethnographies less puzzling. In contrast to earlier trends, Postmodernism gained momentum through a generational (rather than a genealogical) politics. The result, for senior anthropologists, was considerable disempowerment and, with that, a reflective questioning of where the decades-old treadmill of trends was taking the discipline. Pushed to the side, they starting questioning both the direction and dynamics of the way one trend repeatedly replaced another.

A reviewer of The Gender of the Gift called the book “a brilliant, subversive, anticompative analysis of what gender in Melanesia is not: it . . . reveals the fictions and hegemonic ethnocentrisms in our anthropological representations of the ‘other,’ and questions our capacity to view the world from a perspective different than our own. anthropological representations of the ‘other,’ and questions our capacity to view the world from a perspective different than our own. Although implying that gender in Melanesia will continue to elude our Western understanding, Strathern shows that we can comprehend with increasing subtlety the ways we may misunderstand it . . . Strathern’s book is a remarkable and provocative effort to think through and around the constraints on Western thought” (Gewertz 1990:797-798).

First, Strathern asks, how do Melanesian senses of gender clarify our perceptions and misperceptions of gender? (Here she is emphasizing anthropology as cultural critique.) In Melanesia, she writes, “being ‘male’ or being ‘female’ emerges as a holistic unitary state under particular circumstances. In the one-is-many mode, each male or female form may be regarded as containing within it a suppressed composite identity” (1988:14). We might assume that breasts belong to women and phalluses to men, she continues, but “if people say that phalluses were stolen from women or if a phallus is treated like a fetus, it is not at all clear that we can be so certain in our evaluation on the Melanesians behalf” (1988:127). Strathern writes: “As I have construed Melanesian ideas, the person is revealed in the context of relationships . . . the relations that compose him or her [constitute] . . . an inherently multiple construct” (1988:274).

Building on this theme, the second question asks how do Melanesian gift-oriented economies shape notions of gender domination in ways that diverge from those created in more commodity–oriented economies like our own? “In a commodity-oriented economy,” Strathern writes, people “experience their interest . . . as a desire
to appropriate goods; in a gift-oriented economy, the desire is to expand social relations” (1988:143, see also 1988:134). She suggests, despite assertions to the contrary, that no permanent domination exists between men and women in Melanesian contexts. “Being active and passive are relative and momentary positions; in so far as the relevant categories of actors are ‘male’ and ‘female’ then either sex may be held to be a cause of the other’s acts; and . . . vulnerable to the exploits of the other . . . The conclusion must be that these constructions do not entail relations of permanent domination” (1988:333-4).

The third question asks how do we describe the dynamics of Melanesian sociality when they diverge from concepts, such as society, which Western anthropologists employ? “The argument of this book,” Strathern writes, “is that however useful the concept of society may be to analysis, we are not going to justify its use by appealing to indigenous [Melanesian] counterparts” (1988:3). “As I understand [the] Melanesian concept of sociality,” she continues, “there is no indigenous supposition of a society that lies over or above or is inclusive of individual acts and unique events” (1988:102).

First, it isn’t clear exactly how to evaluate Strathern’s assertions. She doesn’t provide a detailed cognitive analysis of a few or even one Melanesian group, so we are left pondering how to interpret her interpretations. She admits, “I have not presented Melanesian ideas but an analysis from the point of view of Western anthropological and feminist preoccupations of what Melanesian ideas might look like if they were to appear in the form of those preoccupations” (1988:309, see also 1988:244).

Brown observes, “just when the reader begins to formulate an objection to her position or assertions, there is a new section, disavowing, subsuming the field of objection to minor status in the scheme. These disclaimers, combined with the difficulties of the language and argument, can impede both critical debate and acceptance” (Strathern 1992:127).

Second, while Strathern criticizes how Westerners essentialize the concepts of gender and society, it is clear she has her own set of essentialisms. For example, drawing from various Melanesian groups, she suggests a general Melanesian form of sociality and aesthetic (see e.g. 1988:341). But Melanesia, especially Papua New Guinea, is perhaps the most culturally diverse region in the world. What about the hundreds of Melanesian groups she does not discuss? She replies: “Within Melanesia we are dealing with societies [her term] that cannot be counted separately because, despite the contrasts between them, as versions of one another they are implicated in one another’s histories” (1988:342). The distinction between gift and consumption economies also seems somewhat overstated - see (Keesing 1992:130), Gewertz 1990:798).
Third, in reply to a set of reviews, Strathern admitted that “the book falls down. . . in its failure to be explicit about its interpretive methods” (1992:150). Again, we are left uncertain as to how we should assess her claims.

Keesing observes, “the New Guinea with which Strathern presents us is a world filled with complex, ambiguous, multivalent and contextually shifting images . . . But it is a world without terror, without violence . . . a timeless world where killing is symbolized and talked about but not practiced” (1992:133-4). “The real question,” Strathern offers in response to such criticisms, “is whether the exercise has enlarged on previous understandings” (1988:153).

== On Patterns of Citation

While some of the contributors listed in Writing Culture are cited in Anthropology as Cultural Critique, - e.g. Rosaldo, Crapanzano, Marcus, and Fischer - the ethnographic examples Marcus and Fischer cite in the latter tend to be different than those used by the contributors in the former. That is not to say there is no ethnographic overlap between the two books. Marcus consider’s Willis’s fieldwork (Willis 1981) in both books, and brief remarks regarding Rosaldo’s and Crapanzano’s ethnographies by Marcus in Writing Culture (1986:165, 192) are elaborated upon in Marcus and Fischer (1986). Fischer refers to novels by Arlen and by Kingston in both. And Pratt considers Shostak’s “Nisa” (Shostak 1983) in Writing Culture, which Marcus and Fischer discuss as well in Anthropology as Cultural Critique. But that is essentially the degree to which the same ethnographic texts are seriously engaged with in both texts. Given the range of texts referred to in these two books, it is a fairly low percentage. Even when there is ethnographic overlap, the discussions in Anthropology as Cultural Critique downplay reference to the analyses in Writing Culture or vice-versa.
To what degree did anthropologists following these three authors strive to seriously build on their work? (The dates in parentheses represent the years the books were published or republished as new editions.)

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<td>A sustained attempt to develop one of the three authors' work in a specific way (through reinterpreting, building on, or criticizing their respective work). Involves at least three sentences of discussion of the specified author's work.</td>
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Relevant author’s work CITED ONLY IN PASSING as a way of demonstrating the anthropologist’s own competence and credibility – by indicating her or his familiarity with the relevant literature. Often embedded in a list of citations, but may be referred to separately in one or, on a rare occasion, two sentences in passing as the anthropologist is developing a certain point.

96 percent

88 percent
(Brown 1996; Bruner 2001; Escobar 1991; Lewis 2001; Linnekin 1991; Shore 1991; Werbner 2001)

80 percent

Readers interested in further exploring topics covered in 3.7 may wish to peruse:

3.9

According to Deborah Rhode’s *In Pursuit of Knowledge: Scholars, Status, and Academic Culture*: “Because academic reputation and rewards are increasingly
dependent on publication, faculty have incentives to churn out tomes that will advance their careers regardless of whether they will also advance knowledge" (Rhode 2006:11).

I noted the National Science Foundation considers “how important is the proposed activity to advancing knowledge and understanding” (NSF 2004). The Social Sciences and Humanities Research Council of Canada asks what is the “expected contribution to the advancement of knowledge” (2009).

But as the noted British anthropologist Adam Kuper asserts: “The [grant] review process rewards people who can write good proposals even if they failed to deliver on earlier grants. Few foundations evaluate the research they fund. . . . The best credential for a fellowship is a previous fellowship. And landing a grant usually wins you more kudos than getting out the results of your research” (Kuper 2009).

Quoting Philip Salzman again, “The credibility of our [anthropological] field reports rests mainly on their uniqueness, that is, on the absence of any other reports that might present contrary ‘findings . . . that test their reliability” (Salzman 1994:35).

Robert Redfield, a noted anthropologist at the University of Chicago, studied a rural Mexican village named Tepoztlan in 1926-27. He wrote an ethnographic description of the village that focused on normative rules (Redfield 1930). Redfield portrayed neighbors as living in relative harmony. In 1943, another anthropologist, Oscar Lewis, with a different affiliation (the Interamerican Indian Institute), conducted research in the village. Focusing on observed behavior, rather than ideal norms, Lewis painted a picture of conflict and factionalism (Lewis 1951; Lewis 1960, also note: Butterworth 1972).


I also examined thirteen disciplinary histories: Barnard 2000; Carneiro 2003; Darnell 2001; Erickson and Murphy 1998; Eriksen and Nielsen 2001; Harris 1968; Hatch 1973; Langness 1987; Layton 1997; Leaf 1979; McGee and Warms 2000; Moore 2004; Patternson 2001. Two, written by European anthropologists, refer to it (one being Eriksen and Nielsen’s A History of Anthropology -- (Eriksen and Nielsen 2001), the other being (Barnard 2000). No American anthropologist did despite the fact that Redfield and Lewis were both American.
Still, Redfield drew the right conclusion from the episode: “The principal conclusion that I draw from this experience is that we are all better off with two descriptions of Tepoztlan than we would be with only one of them. More understanding results from the contrast and complementarity that the two together provide. In the cases of most primitive and exotic communities we have a one-eyed view. We can now look at Tepoztlan with somewhat stereoscopic vision” (Redfield 1956:136).

The striving for originality isn’t new. Historian William Clark dates it to the German Romantic Era of the late eighteenth and early nineteenth centuries (Clark 2006:442).

Michèle Lamont highlights its importance in How Professors Think (2009: 167, 171-172). Originality is listed as one of the most important criteria in assessing grant applications. On the grant assessing panels studied by Lamont, 89 percent of the panelists viewed originality as a significant factor in weighing a grant application. The panelists perceived developing a new perspective, a new approach, or a new method as affirming originality.

As Thomas Kuhn notes in his classic work The Structure of Scientific Revolutions, ideas that too openly challenge the existing consensus may be rejected or ignored.

Andrew Abbot observed: “The young build their careers on forgetting and rediscovery, while the middle-aged are doomed to see the common sense of their graduate-school years refurbished and republished as brilliant new insights” (Abbott 2001:147-148). We saw this in respect to the (Re)Turn to History. Various anthropologists played up their originality by downplaying their predecessors’ work. Anthony Wallace provides a telling image: “Theory in cultural . . . anthropology is like slash-and-burn agriculture: after cultivating a field for a while, the natives move on to a new one and let the bush take over; then they return, slash and burn, and raise crops in the old field again” (Wallace 1966:1254). Also note (Levi-Strauss 1991:91-92).

James Rule suggests in Theory and Progress in Social Science that, what seems original and innovative one day may seem less so another: A “manifestation of our troubled theoretical life is the . . . contested, and transitory quality of what are promoted as ‘state-of-the-art’ lines of inquiry. Apparently unsure of where the disciplines are headed, we are subject to a steady stream of false starts. . . . Exotic specialties arise . . . to dazzle certain sectors of the theoretical public, then abruptly lose both their novelty and their appeal” (Rule 1997:23).

My thanks to Les Sponsel for providing this information on books relating to the Yanomami:

1841, Sir Robert H. Schomburgk, Reisen im Guiana und am Orinoko während der Jahre 1835-1839, Leipzig, Germany.
1953, Alain Gheerbrandt, L’expedition Orenoque-Amazone, 1948-1950,
Paris, France: Gallimard.  
1976, Jacques Lizot, *La Cercle des Feux: Faits et dits des Indiens*
1977, Margaret Jank, Culture Shock, Chicago, IL: Moody.
1979, Alcida Rita Ramos and Kenneth I. Taylor, 1979, The Yanoama in Brazil 1979, Copenhagen, Denmark: IWGIA.
1983, Comite para la Creacion de la Reserve Indigena Yanomami, Los Yanomami Venezolanos, Caracas, Venezuela: Vollmer Fundation, Inc.
1985, Marcus Colchester, ed., The Health and Survival of the Venezuelan Yanomama, Copenhagen, Denmark: IWGIA.
1986, Jean Chaffanjon, El Orinoco y el Caura, Caracas, Venezuela: Editorial Croquis.
1995, R. Brian Ferguson, Yanomami Warfare: A Political History, Santa Fe, NM: School for American research.
1997, Jean Chiappino and Catherine Ales, eds., Del Microscopio a la Maraca, Caracas, Venezuela, Editorial Ex Libris.
2000, Claudia Andujar, Yanomami, Brazil: Curitiba.

As Rhode notes, “there is no guarantee that authors have actually read the sources cited. Indeed, with technological advances, they need not even trouble to type them; entire string citations can be electronically lifted from other publications. Nor does it follow that the sources listed establish the proposition for which they are cited. Even when someone checks the notes, it is generally to determine only whether particular authorities support the text, not whether they are reliable or respected among experts” (Rhode 2006:38). One might add that using software programs such as Endnotes, scholars can pull into their bibliographies scores (if not hundreds) of citations from the web -- based on various search criteria – without ever having reading them.
In looking at the anthropological drive for status, I am reminded of Max Weber’s *The Protestant Ethic and the Spirit of Capitalism* (1958). In Calvinism, he suggests, “the question, Am I one of the elect? . . . sooner or later [arises] . . . for every believer () . . . Favor in the sight of God is measured primarily . . . in terms of the importance of the goods produced” (110,162). “Continuous, systematic work . . . . [is] the surest and most evident proof of rebirth and genuine faith” (1958:172).

Deborah Rhode writes: “The arms race for relative status has almost no winners and many losers. . . . Few academics will achieve true eminence as scholars, and even those who do typically find that there is always someone more distinguished” (Rhode 2006:13).

“Since professionals draw their self-esteem more from their own world than from the public’s . . . The front-line service that is both their fundamental task and their basis for legitimacy becomes the province of low-status colleagues and para-professionals” (Abbott 1988:119).

Drawing on the work of Mary Douglas a prominent British anthropologist, we might frame the effort to keep the broader public at bay – while accepting its funding – in terms of purity and pollution. Moving beyond the academic pale, makes one impure. It pollutes. The pure remain comfortably ensconced within cultural anthropology producing work that few read. (Douglas 1966),

In a way not unlike what the anthropologist James Frazer articulated in his famous classic, *The Golden Bough*, association with senior faculty allows junior faculty to claim some of their status for themselves (Frazer 1963 [1922]).

Critiquing Boas’ approach to history, Kroeber wrote: “the uniqueness of all historic phenomena is . . . taken for granted . . . No laws or near-laws are discovered . . . there are no historical findings” (Kroeber 1935:542). In a biography of Julian Steward, Kerns writes that Steward “privately denied that his teachers – with the slight exception of Robert Lowie – had affected his thought. His unpublished writings suggest that he saw practically no connection between his graduate training and his ideas; he considered himself a maverick and freethinker, not anyone’s disciple. (Lowie declared in print that no one in his own field, including his graduate teacher, Franz Boas, had been a ‘source of inspiration’ for him)” (Kerns 2003:5). Eric Wolf begins *Europe and the People without History* with a critique of the work of his former teacher, Julian Steward (Wolf 1982:14-15).
Deborah Rhode notes an interesting study in this regard. She discusses a Carnegie Foundation report in which more than a third of the faculty believed their publications were mostly assessed in terms of quantity rather than quality. (At schools with doctoral programs, the figure was over 50 percent) (Rhode 2006:46).

Deborah Rhode writes: “Researchers took twelve articles that had been published in the preceding two-and-a-half years and resubmitted them with minor changes to the same journals in which they had appeared; the authors’ names were changed, they were given less prestigious institutional affiliations, and the opening paragraphs were slightly revised. Only three of the articles were recognized as resubmissions. The other nine previously published manuscripts went through the full review process, and eight were rejected, most of them for ‘serious methodological flaws” (Rhode 2006:58-59).

4.1 = On Coining the Term “Public Anthropology”

In chapter three, I referred to junior faculty using James Frazer’s associative principle to gain status for an idea by associating it with a high-status individual. I did this in to the course of developing the term public anthropology. In an earlier account I referred to having coined the term with Renato Rosaldo. Let me provide the full account of what transpired and readers can decide for themselves how credit for developing the term should be distributed. In seeking a name for the University of California Press Series Naomi Schneider and I were creating, I asked a range of people for suggestions. Renato suggested calling the series the “anthropology of public events.” I then reordered and shortened the phrase to “public anthropology.” I would also note that, after I rephrased the term to “public anthropology,” I pushed to popularize it – using the term in publications, University of California Press publicity, and, more generally, publicizing it throughout the discipline. Beyond his thoughtful suggestion, Renato did not contribute further to the popularizing process. I in no way want to diminish Renato’s suggestion. And I think it might be argued back and forth what coining a term specifically refers to. I am simply offering an explanation for why in an early article in the Anthropology Newsletter, I played up my association with Renato Rosaldo and now have emphasized the role I played in developing the term. It involved the politics of seeking to credentialize the term.

The term has certainly caught on. A Google search using the phrase “public anthropology” yields more than 40,000 hits. Here is a sampling: there is an Institute of Public Anthropology at California State University, Fresno (Department of Anthropology n.d. - accessed 8/9/2009b), a Public Anthropology Lecture Series at the University of Waterloo (Communications & Public Affairs 2006 (02-23), a Public Anthropology Post-Doctoral Fellowship at the Field Museum (in Chicago) (Museum 1999), a Masters Program in Public Anthropology at American University (Department of Anthropology n.d. - accessed 8/9/2009a), a faculty focus in Public Anthropology at Tufts University (Department of Anthropology n.d. - accessed 8/9/2009c), a forthcoming Public Anthropology Reviews section as well as a Public Anthropology Editor in the American Anthropologist (Anthropologist 2009; Checker, et al. 2009), a joint University of Guelph/University of Waterloo Master’s Degree in Public Issues Anthropology (Department of Anthropology n.d. - accessed 8/9/2009d), a doctoral program in Antropología de Orientación Publica at the Universidad Autonoma de Madrid in Spain (Personal Communication, Liliana Suarez-Nayaz), a Public Anthropology Initiative at Duke University (Department of Cultural Anthropology n.d. - accessed 8/9/2009), a Public Anthropology Graduate Student Working Group at the University of New Mexico (Public Anthropology 2007), and a
public anthropology category for posts at Savage Minds (Minds n.d. - accessed 8/9/2009). There are classes in Public Anthropology (with that title or a related phrasing) at various North American schools – ranging from York University, to the University of Pennsylvania, to the University of California, Berkeley.

The term is used in slightly different ways by different groups.

In the M.A. program in Public Anthropology at American University “students explore the workings of culture, power and history in everyday life and acquire skills in critical inquiry, problem solving and public communication” (Department of Anthropology n.d. - accessed 8/9/2009a).

A Tufts University web page states that “Public anthropology includes both civic engagement and public scholarship . . . in which we address audiences beyond academia. It is a publicly engaged anthropology at the intersection of theory and practice, of intellectual and ethical concerns, of the global and the local” (Department of Anthropology n.d. - accessed 8/9/2009c).

A web page discussing the Public Anthropology Initiative at Duke University highlights three areas of concern: “(1) training in public communication skills and community-based research; (2) collaborations . . . to address social problems; and (3) forums for critically reflecting upon lessons learned from public engagement” (Department of Cultural Anthropology n.d. - accessed 8/9/2009).

The Public Issues Anthropology joint program at the University of Guelph and the University of Waterloo, “explores the interface between anthropological knowledge and issues crucial to governance, public discourse and civil society” (Department of Anthropology n.d. - accessed 8/9/2009d).

And the American Anthropologist review section “will highlight anthropological work principally aimed at non-academic audiences” (Anthropologist, American 2009).

To a certain degree the discipline feels isolated from the broader society. James Frazer’s The Golden Bough (Frazer 1963 [1922]), Margaret Mead’s Coming of Age in Samoa (Mead 1928), and Ruth Benedict’s Patterns of Culture (Benedict 1934) engaged a wide range of readers beyond the academy in stimulating, important ways during the first half of the twentieth century.

In 2000, writing in the discipline’s monthly periodical, I commented: “I am not sure if I should laugh or cry in describing American anthropology’s present public status. On the one hand, anthropology is wildly popular with the wider public. One reads about anthropologists in novels, sees them in movies. . . . [But] the citations of anthropologists in literature and the popular press [aren’t] always positive. They
appear . . . [as one anthropologist noted] to often ‘reinforce negative and derogatory stereotypes’" (Borofsky 2000). A New York Times report on the 1994 American Anthropological Association’s annual meeting asked: "Who else has been studying colic and spiritualism, sex and field work, and redneck angst?"

Jim Peacock observed that the "anthropological ideas that are currently significant . . . [among the public] remain those that were developed prior to the [second world] war" (Peacock 1997:12).

What we have today, in Micaela di Leonardo's phrasing, is "anthropology without anthropologists" (Di Leonardo 1998:23).

One of the present ironies in the field is that today the best-selling anthropologically oriented books are all written by authors with little or no formal anthropological training: Anne Fadiman’s The Spirit Catches You and You Fall Down (Fadiman 1997), Barbara Ehrenreich’s Nickel and Dimed (Ehrenreich 2001), Jared Diamond’s Guns, Germs, and Steel (Diamond 1997).

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== On A Precise Definition Of Public Anthropology:

Various authors have discussed (and argued) over the difference between applied anthropology and public anthropology. I would prefer to avoid delimiting their intellectual boundaries in any precise way. Public anthropology, applied anthropology, whatever. Who, beyond the discipline, really cares? The important issue, whatever we call ourselves, is doing what we can to solve the social problems at hand. The goal is helping those in need over the long-term, not delineating you from me or me from you. From my perspective, the distinction between public and applied is not something that should take up a lot of energy. The California Series in Public Anthropology was named for certain specific reasons (see http://www.publicanthropology.org/public-anthropology/). Might we leave it at that and move on?

Readers interested in further exploring topics covered in 4.1 may wish to peruse: Rylko-Bauer, et al. 2006.

4.2
I would note that philosophers of knowledge often perceive different “theories” of truth. For elaboration and further details, see Borofsky 1987:16-17.

Remember the New York Times review of The Bottom Billion? “Collier’s is a better book than either Sachs’s or Easterly’s for two reasons. First, its analysis of the causes of poverty is more convincing. Second, its remedies are more plausible” (Ferguson 2007:July 1, 2007, internet version).

Many policy makers have praised the book and gained inspiration from it, including Lawrence Summers (Director of President Obama’s National Economic Council) and Ernesto Zedillo (President of Mexico from 1994 to 2000) – see back of Collier’s The Bottom Billion book jacket.

John Nagl’s Learning to Eat Soup with a Knife became a key reference in rewriting the United States’ Counterinsurgency Field Manual.

But as Sarah Sewall wrote in her introduction to the University of Chicago Press’s printing of the manual, the “counterinsurgency field manual challenges much of what is holy about the American way of war. It demands significant change and sacrifice to fight today’s enemies honorably . . . Those who fail to see the manual as radical don’t understand it” (Sewall 2007:xxi).

The Report of the Commission on the Future of Higher Education produced for U. S. Secretary of Education Margaret Spellings called “on higher education to shed some of its mystery and fundamentally prove the value it delivers” (see Basken 2007).

It is a principle of social relations articulated in an anthropological classic by Marcel Mauss, The Gift (Mauss 1954).

(I noted the New York Times reported that in 1970 not one university-based scholar focused on North Vietnam and fewer than thirty students studied Vietnamese – see Butterfield 1983.)

== Some Further Thoughts On the “Do No Harm” Standard:

From my perspective, the “do no harm” standard holds anthropologists to a rather low standard. Few people hold solely to the “do no harm” standard in their personal relations. Not assisting those close to us in their times of need, not reciprocating, not helping those who help us, means cutting ourselves off from important relationships. Caring for others and being cared for by them involves more than simply not harming. We seek to help, assist, and love those to whom we are tied to in deep emotional, personal ways.

In addition, the “do no harm” standard ignores the fact that the freedom which allows us to affirm personal ethical standards – such as “do no harm” -- is contingent upon certain political contexts. Freedom, the authors of the 1980s
classic *Habits of the Heart: Individualism and Commitment in American Life* write, for Americans often means “being left alone by others, not having other people’s values, ideas, or styles of life forced upon one, being free of arbitrary authority in work, family, and political life. . . . [But] if the entire social world is made up of individuals, each endowed with the right to be free of others’ demands, it becomes hard to forge bonds of attachment to, or cooperation with, other people since such bonds would imply obligations that necessarily impinge on one’s freedom” (Bellah, et al. 1985:23). Freedom comes with an obligation, the famous French writer Alexis de Tocqueville noted in his nineteenth century classic *Democracy in America* (Tocqueville 1945 [1840]). To assume that you can ensure your freedom by doing no harm rather than actively participating in the society, is to live off the goodwill of others, whom you are asking to assume your civic responsibilities to preserve key political structures for you.

In a changing world such as ours, one cannot assume that our freedom to decide how to lead our lives will endure simply because it is something we hold dear. The problem is that the smaller, local worlds in which we tend to live are often shaped by powerful large-scale forces and institutions beyond our small, lived-in worlds. We do not have control over these forces. Struggling against them can be time-consuming and burdensome. Nevertheless, if we accept the burden, we have the ability – through democratic action, through working with others – to resist these larger forces running roughshod over us. We have a method for restricting despotic demands, injustice, chaos, and violence. Focusing on doing no harm and doing no harm alone encourages people to opt out of this process.

*Readers interested in further exploring topics covered in 4.2 may wish to peruse:* American 1998 (Approved); American n.d.; Beard 1939; Bender 1993; Brandeis 1914; Florence 1940; Foley 1999; Goldenweiser 1940; Lasswell 1940; Lundberg 1939; Lynd 1939; Mowry 1939; Weintraub 1941; Westbrook 1994; Westhoff 1995.

4.3

In Chapter 1, I noted Boas’ activist stands during the early 1900s. I also referred to the journal of *Applied Anthropology* (later renamed *Human Organization*) founded in 1941 with a commitment to “putting plans into operation . . . to take action in problems of human relations” (Applied 1941).

Cora DuBois served with the Office of Strategic (or Intelligence) Services. She was awarded the Army’s Exceptional Civilian Award as well as the Order of the Crown by Thailand (Fowler 1991).

Anthropologists coming out of the war years, wrote Margaret Mead, “had learned that their skills could be applied fruitfully to problems affecting modern societies and the deliberations of national governments and nation states” (Mead 1973:1-2).

In the late 1980s, applied anthropology rose again to disciplinary prominence. Where in 1972, 88 percent of the new Ph.D.s were employed in academic settings and 12 percent in non-academic settings, in 1988, 54 percent were employed in non-academic settings and only 46 percent in academic settings (Givens, et al. 1997).

Turner highlighted “two alternative ‘models’ for human interrelatedness. The first involved society as a structured, differentiated, and often hierarchical system of politico-legal-economic positions.” The second, termed anti-structure, opposed society’s formal structures, emphasizing instead alternative, less conforming orientations. He wrote that “there would seem to be – if one can use such a controversial term – a human ‘need’ to participate in both modalities” (Turner 1969:96, 203).

Readers interested in further exploring topics covered in 4.3 may wish to peruse: D'Andrade, et al. 1975; Veblen 1918.

4.4

Why Moving to a more publicly oriented anthropology could personally benefit anthropologists.

Short Answer: Over the past five decades, professional autonomy has been seriously eroded in academia. Faculty today are often subordinate to (and subordinated by) bureaucratic administrators within their universities.

The solution is for faculty to build alliances with public groups beyond the university. An earlier sense of professionalism involved a clear trade -- for professional service to one’s community, service that served the common good -- the community allowed professions to control their own affairs. When the bargain breaks down, as happened with the medical profession in the 1970s and 1980s when doctors dramatically raised their income levels without providing additional care to patients, the community steps in and imposes new rules on a profession. In a sense, this is what has happened with anthropologists. Not serving the common good in readily perceivable ways, university bureaucrats have stepped in and inserted a range of bureaucratic controls over them.

To regain their professional autonomy, anthropologists can’t rely on university bureaucrats. They need to renew their sense of service to the broader community so they can draw on the wider community’s support in their drive for autonomy. In addressing the concerns of others, anthropologists can also address their own.

Elaboration: Some readers might ask: Has there really been a decline in faculty autonomy and authority in recent years? Here is a sample of what various experts on higher education report. Philip Altbach, Director of the Center for International Higher Education at Boston College, notes “the balance of power in many academic institutions has shifted from the faculty to administrators” (Altbach
George Keller, a Professor of Higher-Education Studies at the University of Pennsylvania, refers to the “increasing sway of the marketplace . . . over professional dominion . . . this shift [like various bureaucratic pressures] . . . tends to pull professors toward a . . . serving role and away from a commanding governing role where they design what the students should learn and how and when they can learn it” (Keller 2001:318-319). The internationally recognized Berkeley educator, Martin Trow, writes: “There are important and disquieting changes in the culture of the university . . . a serious decline in morale among academics arising out of their increasing workloads and a general deprofessionalization of the university teacher . . . a loss of authority by the academic community and its committees. Control is shifting to increasingly powerful university administrators and state authorities and to the market through the commercialization of research and teaching” (Trow 2001:113-114)– see also (Riesman 1980:1, 3).

Data from two reports recently described in The Chronicle of Higher Education emphasize this trend. The first, referring to a soon to be published report comparing the perceptions of college faculty from around the world, observes (quoting the Chronicle), “Faculty members in the United States feel exceptionally powerless over their institutions’ affairs, being less likely than their peers in nearly every nation to report having a big say over the selection of key administrators, the budget priorities of their institutions, or their own teaching loads” (Schmidt 2009). The American Academic: The State of the Higher Education Workforce 1997-2007 observes more adjunct faculty and graduate students are taking over teaching responsibilities at schools and being paid at sub-standard wages for the jobs performed. The report indicates (again quoting the Chronicle) “one of the most striking gains was among full-time administrators, whose numbers grew at almost twice the rate of growth for full-time tenured or tenure-track professors over the decade” (Schmidt 2009).

Part-time employees – adjuncts – now teach 40-60 percent of the courses offered at universities. Course enrollments, not curricular needs, often determine which courses are, and are not, taught (June 2009). Without curricular justification, administrators may drop departments that fail to bring in sufficient funding. A review of the report American Professors: A National Resource Imperiled indicates that “the visible signs of peril are a deterioration in faculty compensation in recent years, a slowing of faculty hiring, and increased reliance on nontenure-track faculty” (Kuh 1988:480).

To explain the source of their power, I need to place the problem within an historical context. The notion that certain groups had the authority to control the teaching and performance of particular services as well as regulate members’ behavior – as academics traditionally have affirmed for themselves – stretches back to the guilds of medieval times. In various European towns from the twelfth to fifteenth centuries, guilds constituted associations that controlled the teaching and production of such crafts as the making of earthenware pots, the baking of bread, or the selling of particular goods. To gain control over a particular line of work, a guild needed to pay a tax to the prince, king, or town council that controlled trade in the town (Krause 1997), see also .
In nineteenth century England, service to others (rather than payment to an authority) became central to establishing professional authority. To quote the historian Daniel Duman (Duman 1979:131-132):

The ideal of service not only became a central creed of the professions in the nineteenth century, but it also permeated public opinion. . . . The new ideology based on service as a moral imperative provided an intellectual framework in which the professions could become the essential service occupations without their members becoming a class of servants. The members of the learned professions used the ideal of service to buttress their claims to gentility. . . . The formula was clear: the professionalization process required that an occupation develop a body of technical knowledge, a system of training and testing of candidates, an ethical code, a professional association and an orientation, at least in theory, toward public service. The ideal of service also fulfilled a function for society. With the rise of industry and the apparent moral insufficiency of the old order, England was threatened by the creation of a society in which laissez-faire [or unregulated capitalism] would become a moral yardstick. For many of the social critics and novelists of the nineteenth century. . . it was apparent that some alternative to entrepreneurial society and values must be found.

Nowhere, in any of the material on professions, does it state that professionals have the power to define their authority and service solely for their own benefit, that is to say, without giving something back to the larger society. The larger society doesn’t grant professionals autonomy and authority for “doing no harm.” Professions gain their authority by delivering certain expertise to the larger society in the form of needed services.

To place the trend of declining faculty autonomy in perspective, we might turn to Eliot Freidson’s analysis of medicine. (Freidson is a noted New York University medical sociologist who writes on the professions.) There is a lesson for the learning in Freidson’s analysis.

“At its prime in the 1950’s,” Eliot Friedson (Freidson 2001) writes, “medicine was the most highly respected occupation in the United States, excepting only that of Supreme Court Justice. The federal government was essentially passive in financing and organizing health care, restricting itself primarily to enforcing the recommendations of the organized profession” (2001:182). Physicians “were free to charge all that the pockets of their patients could yield and to decide how much charity or free care to provide to whom. . . . They were free to follow their own clinical judgment, for neither state licensing boards, state medical societies, nor local community colleagues examined their work” (2001:184). This changed in the 1970's. “Once federal health insurance had been established for the elderly . . . both the legislative and executive branches began to take active steps to control the increasing cost to taxpayers and to take some responsibility for the quality of the care paid for . . . Having large sums of money at stake, large insurance companies . . . were not inclined to protect physicians’ freedom to order whatever services they thought best and to bill whatever they had established as the going rate” (Friedson 2001:187-188).
With the expansion of medical coverage and clear proof that some doctors were making oversized profits, physicians began losing their carte blanche control over the profession. Increasingly, consumer activists and governmental bureaucracies are placing limits on the medical profession’s ability to control its practice. The medical profession’s concern for the Hippocratic oath – for “doing no harm” – remains as strong as ever. But with a perceived decline in public service has come a real decline in professional autonomy.

To avoid further erosion of faculty authority and autonomy, anthropologists can learn from this example. It’s true that college and university faculty never had the same sense of service as doctors. But that doesn’t take away from the basic point: An alliance with the public, built on a renewed sense of service to those beyond the academy, would strengthen a faculty’s ability to resist the bureaucratic forces aligned against it within a university. As noted, the public today is often puzzled by what transpires within universities. Many are angry at professors’ seeming isolation and arrogance. Opening a dialogue with those beyond the academy – addressing their concerns in ways they appreciate and value – would help the broader public better understand the value of anthropology and the importance of allowing anthropologists their professional autonomy.

I’m not sure there are many other options to reverse the decline in faculty autonomy and authority. University administrators aren’t eager to give up their newly gained power. And the pressures of the marketplace, which bureaucrats use to disempower faculty and empower themselves, aren’t going away soon. The financial pressures could be reduced through an major influx of funding, as occurred during the 1950s and 1960s. But that isn’t likely to happen anytime soon. Presently, enrollments are rising without corresponding increases in faculty. Instead, student-faculty ratios are rising along with the hiring of non-permanent adjunct teachers and the faculty’s inability to control important academic matters.

Opening a real dialogue with the public could be a win-win situation. The public would protect anthropology because it needs the insights the discipline offers, insights that bureaucrats don’t provide. And in addressing public problems in insightful ways, anthropology would flourish as a profession with autonomy and authority.


The Yanomami controversy, touched on in Chapter 1, became public in 2000 with the publication of Darkness in El Dorado: How Scientists and Journalists Devastated the Amazon by Patrick Tierney, a journalist (Tierney 2000). Here is a sampling of what the media said. ABC News.com reported (Regush 2000): “Another red-hot scientific
scandal. This time anthropologists and geneticists are getting a noisy wake-up call.” *USA Today* (Vergano 2000) noted that the “face of anthropology stands riddled with charges that its practitioners engaged in genocide, criminality and scientific misconduct.” *Business Week* (Smith 2000) noted: “Tierney makes a persuasive argument that anthropologists for several decades engaged in unethical practices.”

In 1996 the Association decided to “no longer adjudicate claims of unethical behavior and focus its efforts [instead] . . . on an ethics education program.”

http://www.aaanet.org/committees/ethics/ethrpt.htm

In respect to Chagnon, the task force felt it could not resolve some of the accusations made against him. But the report affirmed that Chagnon had “violated Venezuelan laws . . . and involved him[self] in activities that endangered the health and well-being of the Yanomami.” (American, Anthropological Association 2002:I:44).

In 2004, Thomas Gregor and Daniel Gross, two supporters of Chagnon, wrote an article in the *American Anthropologist* criticizing the task force’s report (Gregor and Gross 2004).

Readers might take note of how the Association’s Executive Board’s phrased its acceptance of the “Resolution to Rescind Acceptance of the Report of the El Dorado Task Force”: “Notwithstanding that the board erred in permitting the [resolution] to be placed on the spring 2005 ballot as written . . . [as well as the fact that] the resolution contravened the law of the District of Columbia . . . and the AAA’s Articles of Incorporation . . . [and] contravened the AAA’s bylaws, in recognition of the vote . . . the executive board declares that the acceptance of the Report of the El Dorado Task Force of May 2002 is rescinded.” (AAA Executive Board Action. August 25, 2005. Anthropology News, Association Business, October 2005: 10)

Kuhn suggests that scientific revolutions come about when a particular paradigm – or framework for addressing an important set of problems – is realized to be inadequate. A new paradigm arises that proves better at solving these problems (as well as the old ones) and gradually replaces the outmoded paradigm. There is no sudden change. Instead, writes Kuhn (1970:145):

> Paradigm testing [or challenging of the existing paradigm] occurs only after persistent failure to solve a noteworthy puzzle has given rise to crisis. And even then it occurs only after the sense of crisis has evoked an alternative candidate for paradigm. . . . Testing occurs as part of the competition between two rival paradigms for the allegiance of the scientific community. . . . Though a generation is sometimes required to effect the change, scientific communities have again and again been converted to new paradigms.
Readers interested in further exploring topics covered in 4.4 may wish to peruse: Dietze 2001; Judis 2000; Mead 2007; Mills 2000; Smith 2008; Wikipedia n.d.f; Wikipedia n.d.g.

**4.5**

The third fundamental principle of Partners in Health (as specified on its website) reads:

Community partnerships: Health programs should involve community members at all levels of assessment, design, implementation, and evaluation. Community health workers may be family members, friends, or even patients who provide health education, refer people who are ill to a clinic, or deliver medicines and social support to patients in their homes. Community health workers do not supplant the work of doctors or nurses; rather, they are a vital interface between the clinic and the community. In recognition of the critical role they play, they should be compensated for their work. PIH doesn't tell the communities we serve what they need—they tell us ([http://www.pih.org/pages/what-we-do/](http://www.pih.org/pages/what-we-do/)).

All NSF applications for funding are evaluated in terms of two main standards that researchers must address in their applications: What is the intellectual merit of the proposed activity? “How important is the proposed activity to advancing knowledge and understanding within its own field or across different fields?” What are the broader impacts of the proposed activity? Among the impacts emphasized is: “What may be the benefits of the proposed activity to society?” (For the full description of these, see [http://www.nsf.gov/pubs/2007/nsf07544/nsf07544.htm](http://www.nsf.gov/pubs/2007/nsf07544/nsf07544.htm) - VI. A. Merit Review Criteria.)

Grantees were only required to submit project reports that provided “NSF program officers . . . with information on the progress of supported projects and the way these funds are used.” See E. Technical Reporting Requirements ([http://www.nsf.gov/pubs/policydocs/pappguide/nsf10_1/aag_2.jsp](http://www.nsf.gov/pubs/policydocs/pappguide/nsf10_1/aag_2.jsp))

A new section to the Reporting Requirements now reads (see [http://www.nsf.gov/pubs/policydocs/pappguide/nsf10_1/aag_2.jsp](http://www.nsf.gov/pubs/policydocs/pappguide/nsf10_1/aag_2.jsp))

E. Technical Reporting Requirements . . .

3. Project Outcomes Report for the General Public

Within 90 days following expiration of the grant, a project outcomes report for the general public must be submitted electronically via Research.gov. This report serves as a brief summary, prepared specifically for the public, of the nature and
outcomes of the project. This report will be posted on the NSF website exactly as it is submitted and will be accompanied by the following disclaimer:

“This Project Outcomes Report for the General Public is displayed verbatim as submitted by the Principal Investigator (PI) for this award. Any opinions, findings, and conclusions or recommendations expressed in this Report are those of the PI and do not necessarily reflect the views of the National Science Foundation; NSF has not approved or endorsed its content.”

Grantees are to ensure that the report does not contain any confidential, proprietary business information; unpublished conclusions or data that might compromise the ability to publish results in the research literature; or invention disclosures that might adversely affect the patent rights or those of the organization, in a subject invention under the award. Responses are not to contain any private personally identifiable information such as home contact information, individual demographic data or individually identifiable information collected from human research participants.

Section Contents:

1. Describe the project outcomes or findings that address the intellectual merit and broader impacts of the work as defined in the NSF merit review criteria. This description should be a brief (generally, two to three paragraphs) summary of the project’s results that is written for the lay reader. Principal Investigators are strongly encouraged to avoid use of jargon, terms of art, or acronyms.

2. NSF will automatically include all publications that are provided regarding the award in the FastLane project reporting system. Other products that have resulted from the award also are to be listed. Examples of other products include collections, data sets, software, as well as educational materials.
3. Additional information.

Information regarding anticipated publication of project results, as well as any other information that would be of interest to the public also may be included in this section.

The preliminary Outreach Assessment proved moderately successful. For further details on Public Anthropology’s Outreach Assessment Project, see http://www.publicanthropology.org/public-outreach/overview/ and http://www.publicanthropology.org/public-outreach/2006-results/

Readers interested in further exploring topics covered in 4.5 may wish to peruse: Anonymous 2007; Birchard 2006d; Holmes 2006 (?); Howard 2008a; Kelderman 2009; Labi 2008a; Labi 2008b; Macleans.ca 2007; Mangan 2007; Mangan 2009; Mytelka 2009; N/A 2009a; N/A 2009b; Schmidt 2008d; Yunus 2007.
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American, Anthropological Association


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Anastas, Benjamin


Anderson, Perry


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