REVITALIZING ANTHROPOLOGY . . . WITH YOUR HELP!

A Study in Public Anthropology

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Anthropologists have now spent a generation reflecting on power dynamics "in the field"—that is, where there are unlikely to be any real-world consequences because we are the ones with all the power—but written almost nothing about conditions of work, patronage, funding, institutional hierarchy in the academy—that is, the power relations under which anthropological writing is actually produced. Borofsky is one of the few who's had the requisite courage to do so.[1]

—DAVID GRAEBER, *London School of Economics*

ABSTRACT: This essay considers key barriers to anthropology living up to its intellectual and moral potential. It explores how the academy's infrastructure—of funding agencies, universities, and academic publishers—which, in principle, claims to be focused on benefiting the broader society, in practice reframes benefits to involving publications of uncertain value. Instead of advancing knowledge and benefiting others, these publications often appear oriented toward advancing academic careers. How to move anthropology beyond claiming to benefit others to actually doing so in demonstrable, meaningful ways to them, remains an open question. That is the topic the three $1,000 Revitalizing Anthropology Graduate Student Awards address.

PLEASE, DO NOT UNDERESTIMATE the potential of anthropology to facilitate meaningful change that improves people's lives. With its in-depth research techniques and broad comparative understandings, it can make a difference—a real difference—in the lives of many people around the world. At its best, anthropology represents an antidote to hate, provincialism, and despair. In stressing

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1   *Epigraph*: Graeber's quote refers to Borofsky (2019: frontmatter).

the fluid nature of group identities through time and space, it helps soften ethnic violence. In valuing cultural diversity for how it enriches our world, anthropology fosters tolerance of difference. In emphasizing how context shapes behavior, it encourages people to reshape the contexts needed to reshape their lives—medically, economically, socially—so as to find new meaning, opportunity, and hope.

Unfortunately, anthropology frequently falls short of this potential. The question is why. What are the structural impediments that, despite the best intentions, limit the field's development—fragmenting its intellectual focus and limiting its public significance and support? The Center for a Public Anthropology (publicanthropology.org) has spent more than a decade exploring this question—most recently in *An Anthropology of Anthropology*—with uncertain results.[2] On the positive side, *An Anthropology of Anthropology* has been downloaded and/or purchased over six thousand times in the past twenty-one months

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2   Borofsky (2019). This open-access book can be downloaded at https://books.publicanthropology.org/an-anthropology-of-anthropology.html.

by people from more than one hundred countries. Less clear is whether it has convinced many anthropologists to seriously address the problem at the book's core. Despite having blurbs by some of the world's leading cultural and social anthropologists, for example, the book has not yet been reviewed by any of the field's leading journals.[3] Few seem seriously concerned with exploring the book's central concern: Why is anthropology not living up to its potential?

This question has particular relevance given the financial pressures bearing down on academic departments. Since governments are spending trillions of dollars to cope with the medical and economic devastation wrought by the Covid-19 pandemic, it is unlikely there will be enough funding to support existing programs, research projects, and faculty salaries across a host of academic fields, including anthropology. A recent headline in the *New York Times* reports: "Colleges Slash Budgets in the Pandemic, with 'Nothing Off-Limits': Liberal arts departments, graduate student aid and even tenured teaching positions are targets as the coronavirus causes shortfalls."[4]

This essay seeks to do two things: (1) It explores key problems limiting anthropology from living up to its potential; and (2) it sets the foundation for the three $1,000 Revitalizing Anthropology Graduate Student Awards for the best essays on how to address the problems highlighted here.[5]

1.   Dynamics That Shape Anthropology as a Profession

As David Graeber's quotation at the beginning of this essay suggests, anthropologists need to be more reflective about the dynamics that shape their work. Anthropologists are skilled at writing incisive analyses about the dynamics of life in foreign lands. However, many anthropologists shy away from writing incisive analyses about the dynamics that shape their own intellectual productions. We need to better understand these dynamics, if we are going to address the problems highlighted in this essay. Let me provide three brief illustrations.

First, universities often portray themselves as centers of meritocracy. Competency trumps status. But frequently, universities do not operate this way. Disguised behind the rhetoric of equality, some suggest, is a self-serving elite pa-

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3   The one exception is *Anthropologica,* which, I am told, intends to publish a review sometime in 2021.

4   Hubler (2020).

5   Details regarding the graduate essay awards are available at: http://revitalizing. publicanthropology.net.

tronage system that parallels broader inequalities in American society.[6] We can see this in faculty hiring. In principle, any PhD can apply for a position at a university and expect to be taken seriously. In actual fact, however, those who are hired tend to come from a few elite universities. As Chad Wellmon and Andrew Piper have reported: "Several recent studies have shown a high degree of concentration of academic hires from a small number of PhD-granting institutions. . . . Only 25 percent of institutions produced 71 to 86 percent of all tenure-track faculty. And the top ten institutions produced 1.6 to 3.0 times more faculty than the second ten."[7]

Similarly, as Nicholas Kawa, José A. Clavijo Michelangeli, Jessica Clark, Daniel Ginsberg, and Christopher McCarty have noted: "In US academic anthropology, a small cluster of programs is responsible for producing the majority of tenured and tenure-track faculty in PhD-granting programs, with a very select few dominating the network." They write that "the top ten programs produced 2.5 times more faculty than the second ten programs, and programs ranked 11–20 produced 1.5 times more than those ranked 21–30."[8] More than forty years ago, Beverly McElligott Hurlbert reported a similar pattern in anthropological hiring.[9]

The same pattern holds true in respect to publishing. Once again, the general image is that quality trumps status. Yet as Wellmon and Piper have observed, faculty at high-status universities have more papers accepted for publication in prominent journals than faculty at less prestigious universities. They write: "When, as our data show, Harvard University and Yale University exercise such a disproportionate influence on . . . publishing patterns, academic publishing seems less a democratic marketplace of ideas and more a tightly controlled network of patronage and cultural capital."[10] (Rhode discusses a study in which twelve recently published papers were resubmitted to the same journals with different authors' names, less prestigious academic affiliations, and slightly changed opening paragraphs. Three articles were recognized as resubmissions, one was accepted again, and eight were rejected for "serious methodological flaws."[11])

A second illustration of the dynamics shaping our intellectual productions involves the downplaying of intracultural diversity. With the rise of nation

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6   Cf. Leighton (2020).
7   Wellmon and Piper (2017).
8   Kawa et al. (2018:23, 18).
9   Hurlbert (1976).
10   Wellmon and Piper (2017).
11   Rhode (2006:58–59).

states, especially during the 1800s, there was a move away from defining political units as dynastic centers with a diverse assortment of subjects affirming loyalty to a head of state. In Anderson's phrasing, many became imagined communities. "Members of even the smallest nation will never know most of their fellow-members . . . yet in the minds of each lives the image of their communion."[12] Hobsbawm writes, "we should not be misled by a curious, but understandable paradox: modern nations . . . claim to be the opposite of novel, namely rooted in the remotest antiquity, and the opposite of constructed, namely human communities so 'natural' as to require no definition other than self-assertion."[13] When anthropologists studied non-Western groups, they tended to carry this embracing of imagined communities with ancient traditions over to the people they studied. They perceived these group as bound together by their shared cultural understandings through time.

The way anthropologists studied these groups reinforced this perspective. They would often work with a select number of "key" informants assuming these informants were representative of their whole group.

Yet for decades some anthropologists have been aware that intracultural diversity exists within the social units they study—for more than a century if you include J. Owen Dorsey's Omaha Sociology.[14] More than eighty years ago, Franz Boas noted that "most attempts to characterize the social life of peoples are hampered by the lack of uniform behavior of all individuals."[15] And Edward Sapir, following Dorsey, wrote: "Two Crows, a perfectly good and authoritative [informant] . . . could presume to rule out of court the very existence of a custom or attitude or belief vouched for by some other [informant] . . . equally good and authoritative."[16] Anthropologists have studied diversity in family values (Swartz),[17] religious conceptions (Brunton),[18] bird classifications (Gardner),[19] plant knowledge (Hays),[20] hot-cold categorizations (Foster),[21] and views of the

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12   Anderson (1991:6).
13   Hobsbawm (1983:14).
14   Dorsey (1884).
15   Boas (1938:683).
16   Sapir (1949:570).
17   Swartz (1982).
18   Brunton (1980).
19   Gardner (1976).
20   Hays (1976).
21   Foster (1979).

past (Borofsky).[22] Anthony F. C. Wallace has suggested that ethnographic units should be conceived less in terms of shared uniformity than in terms of what he calls the "organization of diversity"—that is, how "individually diverse organisms work to maintain, increase, or restore [a] quantity of organization . . . within [heterogenous] sociocultural . . . systems."[23]

If we turn to genetic diversity to gain a perspective on the problem, the Pulitzer Prize–winning author Siddhartha Mukherjee has written that "the most recent estimates suggest that the vast proportion of genetic diversity (85–90 percent) occurs within so-called races . . . and only a minor proportion (7 percent) between racial groups. (The geneticist Richard Lewontin had estimated a similar distribution as early as 1972)."[24] This is certainly intriguing, but we lack enough data to draw cultural parallels with Mukherjee's work. We know intracultural diversity certainly exists. But for many societies, we lack a clear sense of its breadth, depth, and dynamics. Still, hopefully, my point is clear. Asserting shared cultural understandings within social units, especially when participants do not regularly interact with one another, needs to be based on more than a 19th century political ideology that emphasized long enduring communities of shared values and knowledge.

Allowing more space for divergent perspectives, I would note, could soften the colonial image of a single anthropologist presenting an authoritative ethnographic account of a group. This, in turn, might lessen Indigenous resistance to ethnographies of their societies by outside anthropologists.[25]

A third illustration concerns how the discipline's early organization has shaped current disciplinary perspectives. When American anthropology departments were created, they drew together scholars from an array of backgrounds to facilitate the examination of a set of intellectual concerns centered on the "cultural roots" of non-Western groups without recorded history. It paralleled, as just noted, how scholars framed their studies of nation-building in nineteenth-century Europe.[26] That is the reason researchers from cultural anthropology, archaeology, physical anthropology, and linguistics were originally included within anthropology departments. The difficulty American anthropology faces today is that many cultural anthropologists have gone on to deal

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22   Borofsky (1987).
23   Wallace (1970:129).
24   Mukherjee (2016:341–342).
25   Note, in this respect, Trask (1991, 1993); Hereniko (2000[2020]); Borofsky (2020).
26   See Hobsbawm and Ranger (1983); Colley (1992).

with other questions, other concerns. What does an anthropology department do when a large percentage of its members move off in new intellectual directions that separate them from others in their department? Bureaucratically, anthropology departments are set up to defend themselves—their funding, their faculty positions, their status in wider settings—against competitors. Splitting up into smaller departments does not resolve the problem since smaller units would be at a bureaucratic disadvantage in defending their funding and faculty positions from competing departments.[27]

How have anthropologists handled this bureaucratic problem? One way has been to embrace a myth of disciplinary integration in times past. Eric Wolf expressed this myth in a well-received 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."[28] However, if we examine 3,264 articles published from 1899 to 1998 in the *American Anthropologist*, the discipline's flagship journal, perhaps only 311 substantially draw on more than one anthropological subfield in the analysis of their data. That is to say, over a hundred-year period, perhaps only 9.5 percent of the articles published in the *American Anthropologist* bring the discipline's subfields together in a significant way. Most of the articles focus on narrow subjects and use the perspectives and tools of one subfield. They offer relatively little synthesis across subfields.[29]

So why would anthropologists affirm something about the past—that the subfields previously collaborated in significant ways—that is clearly at variance with established fact? The myth of an earlier "golden age" of disciplinary integration constitutes a "social charter" (in Bronislaw Malinowski's terms) for today's departmental structure. Disciplinary integration is held up as an ideal—an "invention of tradition," to quote Eric Hobsbawm and Terence Ranger.[30] But it also does more. It implicitly calls for more disciplinary integration today to soften departmental fragmentation. The myth allows anthropologists to address a problem of social structure—intellectual fragmentation within a department—without the pain of anyone actually having to change. It allows them to pretend that they all once worked together as an intellectual team without having to do so today.

27 Stocking (1976).
28 E. Wolf (1974:x).
29 See Borofsky (2002) for further details.
30 Hobsbawm and Ranger (1983).

In brief, exploring the social construction of knowledge and practice within anthropology allows us to better understand what lies behind how the field presents itself to itself and to others. It provides much food for thought.

2. Why Do Publications Play Such an Important Role in Anthropology?

Digging deeper, we can perceive a key dynamic that frames the field's production of social value for the broader society. It relates to Antonio Gramsci's sense of hegemony. Especially as the term is espoused in *Prison Notebooks*, it involves two key elements useful for understanding why many academics focus on publications in addressing social problems.

Hegemony can be seen as "the 'spontaneous' consent given by the great masses of the population to the general direction imposed on social life by the dominant fundamental group." When this "spontaneous" consent fails, Gramsci continues, there is "the apparatus of state['s] . . . coercive power which 'legally' enforces discipline on those groups who do not 'consent' either actively or passively."[31] I refer to this "spontaneous consent" as hegemonic-like because, while it provides broad constraints on behavior, it lacks the general sense of *hegemon* as used in reference to larger political units. The hegemonic-like framing can be seen in the common perception among many academics that publishing serves the common good. The three structural supports of the academic order—funding agencies, universities, and university publishers—in overlapping ways underscore this.

All three structural supports emphasize the public benefits their institutions provide the broader public. The National Science Foundation, for example, requires proposals to specify the "broader impacts" of their research defined as encompassing "the potential to benefit society and contribute to the achievement of specific, desired, societal outcomes."[32] The mission statement of a university, such as the University of Illinois at Urbana-Champaign, asserts "the University . . . is charged by our state to enhance the lives of citizens in Illinois, across the nation and around the world through our leadership in learning, discovery, engagement and economic development."[33] The University of California Press portrays itself as "one of the most forward-thinking scholarly publishers, committed to influencing public discourse and challenging the status quo."[34]

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31   Gramsci (1971:12).
32   See Public Law (2007).
33   University of Illinois (n.d.).
34   University of California Press (n.d.).

A critical question that is often left unstated, however, is whether these social concerns are best approached through publications or through other means that strive to directly address the noted concerns. From the academy's hegemonic-like perspective, "broader impacts," "engagement and economic development," and "challenging the status quo" are most often addressed by studying and writing about them. The implication is that hopefully, with time, such publications will lead to demonstratable, meaningful public benefits. Sometimes they certainly do. Unfortunately, at least in anthropology, often they do not.

In assessing whether faculty members deserve promotions, universities tend to focus on their publication records. The implication is that by fostering faculty publications, universities are providing a platform for improving society. For those who object to the push to publish, in accord with Gramsci, university administrators can ensure it is taken seriously. Those who are slow writers or wander off into other activities during their university employment can be turned down for promotion or not rehired when their contracts come up for renewal.

The case of university presses is interesting. Obviously, they are focused on publishing books. What is less emphasized is that most of the books these presses print are aimed for use in academic courses. Their inspiring goals, in practice, tend to refer to academic audiences. University presses mostly publish books by academics who tend to write in an academic style so as to convey intellectual competence to their colleagues. Some of the books produced by university presses sell well beyond the academy. But most academic presses make much of their profit by selling books to students who are required to read them as part of a university course.

I would add, as an aside, because these institutions' mission statements tend toward abstract ideals, publications offer concrete reference points that convey an appearance of accountability. They can be counted. They affirm, in a vague, almost magical way, the realization of these institution's mission statements without looking at what the publications actually achieve in practice.

While the various mission statements emphasize improving the human condition, what is significant, but frequently left unsaid, is that these institutions frequently do not want too much change. They are cautious about destabilizing the structures that support them. One might perceive many academic books as a form of passive political activity bordering on Kabuki theater in which the appearance of public benefits counts as actual public benefits. Or as Corey Miles asserts in *The Chronicle of Higher Education*, "Harvard's denial of tenure consideration for Cornel West shows universities embrace activist rhetoric but

not activists."[35] Robin D.G. Kelley, discussing Cornel West's situation in *Boston Review,* observes, "Harvard has a problem with outspoken, principled faculty who take public positions that question university policy, challenge authority, or might ruffle the feathers of big donors."[36]

Junior faculty are wise to approach certain topics gingerly. An exposé is fine if it involves an unrelated institution. But, in dealing with one's own university and its supporters, caution is in order. It could prove disadvantageous to one's professional career. My point is this: Funding agencies, universities and university publishers are entwined in a hegemonic-like structure, in which certain behaviors surrounding publishing are portrayed as serving the broader social good, as what an academic career should involve. Faculty benefit the wider society through their publications.

It would be appropriate, however, to clarify this assertion in four ways. First, as suggested above and detailed below, it is far from clear that most publications in anthropology significantly benefit the broader society in demonstrable, meaningful ways. Certainly, some do. But for many publications, their demonstrable, meaningful benefits beyond the academy—to the people the anthropologists worked with and to the broader society that supported their research in a variety of ways—often remains in doubt. Paraphrasing Claude Lévi-Strauss, from another context, the gap between the hopeful rhetoric of positive benefits and the actual benefits that demonstrably improve other people's lives in meaningful ways to them is often large enough that you could drive a carriage through it.[37]

Second, there are coercive elements to ensure proper behavior. Faculty should publish so many academic articles or books in a review period if they want to be promoted. They should also be somewhat strategic about what they study—especially if, as many anthropologists do, they conduct research in a foreign country that may revoke their visas if they stir up what might be perceived as "trouble." Behind the idealism of various mission statements, I am suggesting, is a pragmatic concern for not rocking the political boat too much (as Cornel West's case demonstrates).

Third, for more than two centuries, publications have played a role in assessing the intellectual competence of faculty members.[38] Recently, as funding

35   Miles (2021).
36   Kelley (2021)
37   Lévi-Strauss (1963:55)
38   Clark (2006:259).

for faculty research has notably increased, there has been a growing concern for assessing faculty productivity in metric terms—especially in respect to the number of publications they produce and the degree to which colleagues cite them. The push for assessing faculty in these terms often draws anthropologists toward producing a number of relatively narrowly framed articles. Rather than spending years synthesizing a range of comparative materials and writing a single publication of broad relevance to the discipline and/or the public, the emphasis is on producing a number of more focused articles. In dealing with social inequality and the broader patterns behind it, for example, the emphasis is on single case studies. Caught up with describing individual trees, we rarely analyze the wider forest. The broader comparative perspective that gives context and meaning to individual cases is only briefly touched on.

The focus on metrics, I would add, also downplays the role of teaching in faculty assessments. Even though teaching is frequently deemed one of the academy's primary responsibilities, it is harder to measure and hence not stressed to the same degree as publications in the assessment of faculty.[39]

As Cris Shore and Susan Wright write: "A key aspect of this process has been its effect in changing the identity of professionals and the way they conceptualize themselves. The audited subject is recast as a depersonalized unit of economic resource whose productivity and performance must constantly be measured and enhanced."[40] Faculty productivity need not be primarily measured in metric terms. But many administrators find these metrics convenient to use, especially across disciplines. It means that administrators need not depend as much on faculty versed in a particular discipline's intricacies in making assessments for tenure and promotion. Using these metrics, they can gain greater control over the process.

Fourth, many anthropologists harbor a secret hope that their publications will break through the hegemonic-like structures of the academy and bring change. They may. There are important examples of this by those outside the academy— such as Rachel Carson's *The Silent Spring* or John Hershey's *Hiroshima*.[41] But if

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39   For assessments questioning what students learn in college, readers might peruse Arum and Roksa (2010); Association of American Colleges and Universities (2005); Miller and Malandra (n.d.); and Pascarella and Terenzini (2011).

40   Shore and Wright (2000:62).

41   Carson (1962); Hershey (1946); see also Blume (2020). Both Carson's and Hershey's works quickly drew the attention of other media, including the *New York Times*, the BBC, and in Carson's case, committees of the US Congress.

readers look carefully, they will see that many examples of seminal publications leading to change by less prominent authors (at the time of publication), involve their work being taken up by one or more powerful groups *beyond* the academy. These groups embrace a publication because its message fits with their own agenda. In the case of John Hershey, *The New Yorker* played a prominent role in bringing his work to the broader public. This, in turn, brought positive attention to the magazine, thereby helping to enlarge its readership. (*An Anthropology of Anthropology* provides additional examples of this phenomenon.)[42]

The dampening down of political activity—and redirecting it toward publications—reverberates outward in interesting ways. It affects, for example, how many academics perceive objectivity. Objectivity arises when different people independently confirm a research project's results. Since at least the early 1900s, many universities have sought to limit faculty activism. As Mary Furner has pointed out in *Advocacy and Objectivity*, becoming a credible professional in a university often meant establishing a disinterested attitude toward the subject studied.[43] To gain academic security and respectability, she suggested, many activists were seduced away from social activism by the comforts and financial stability of university positions. "Objectivity" came to mean avoiding politically charged topics that might threaten the "powers that be."

Yet as stressed in *An Anthropology of Anthropology*, objectivity doesn't lie in avoiding certain topics, in appearing respectable. The issue isn't whether one does (or doesn't) have a political agenda. To some degree, everyone has biases of one sort or another. Being a "disinterested" professional doesn't mean being uninterested in the world outside one's laboratory. It means putting the larger society's interests ahead of one's own interests or the interests of those one works for. Objectivity derives from the open, public analysis of differing accounts. We know an account is more objective, more credible, after other researchers—whatever their personal biases—independently and publicly confirm certain claims being made.[44]

Another example of dampening political activity can be perceived in the American Anthropological Associations code of ethics for 1998, 2009, and

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42   See Borofsky (2019:173–193).

43   Furner (1975).

44   This topic is elaborated upon in greater detail in Borofsky (2019:11–15, 53–57), Latour and Woolgar (1986: 176, 202-3, 213,240, 243), Galison (1997:xx, 6, 48, 53, 733, 781-844) and Shapin (1994:xxx-xxxi, 132, 338, 352–54).

2012 that emphasized "do no harm."[45] The focus appears to be on protecting a researcher's informants. But as practiced today, the phrase seems to connote maintaining the general status quo. It allows anthropologists to skirt a set of moral dilemmas and obligations. Since much of the suffering in the communities that anthropologists study derives from outside forces beyond their field sites—involving national governments, international corporations, competing groups, and/or acts of nature—doing "do no harm" implies anthropologist need not address these concerns.

As Philippe Bourgois has written, in anthropology, "the power relations that create the worlds of the people they study and cause them to suffer disproportionately . . . are usually glaringly absent from . . . ethnographies." He adds: by "focusing our discussion of ethnography onto fascinating, hypertextual topics we do not threaten significant power structures."[46] It would be better if the American Anthropological Association's code of ethics, instead of focusing on "do no harm," focused on providing positive benefits to those who helped in one's research. Thomas McIlwraith has emphasized that, as anthropologists, we should be sure our work benefits "the groups of people who have shared with us their lives and entrusted us with their stories."[47] The question is not whether you, the researcher, feel you have benefited the people you worked with. The question is whether those you worked with perceive concrete benefits from you and your research. In this regard, it is often best to work with socially constituted groups, especially those respected by many of your informants. Consulting with a group, rather than a few select individuals, means there will be broader support for your research. Working with such groups also means your efforts to provide something in return for various people's assistance will usually have a broader impact than if you did it yourself. There may also be more collective appreciation. Even if your efforts are less successful than hoped, working with such groups acknowledges and empowers them—far more than a promise to "do no harm."

3.   Exploring How to Assess the Value of the Publications Produced

As the previous section notes, the hegemonic-like structure of the academy emphasizes that publications matter, that they benefit the broader society. A single

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45   American Anthropological Association (1998, 2009, 2012).
46   Bourgois (2002:217, 219); see also Scheper-Hughes (1993).
47   McIlwraith (2018).

publication may not advance knowledge. But as one publication builds on an-other, many suggest we develop a deeper, richer, more textured understanding of a topic. That does not always hold true in anthropology, however. Many stud-ies in the field involve unsubstantiated assertions of uncertain, ambiguous value. Such publications may claim to be objective in the academic sense noted earlier. But it is often unclear if their knowledge claims can be readily confirmed, built on, and/or refined by others.

There is a continuing conversation in anthropology—not in the limelight but off to the side—regarding whether the field is doing more than simply appear-ing to advance knowledge. As Eric Wolf has asserted: "In anthropology, we are continuously slaying paradigms, only to see them return to life, as if discovered for the first time. As each successive approach carries the axe to its predecessors, anthropology comes to resemble a project of intellectual deforestation."[48] The noted Berkeley anthropologist Elizabeth Colson put it this way:

> Rapid population growth and geographical dispersal [within 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.[49]

And the Canadian anthropologist Philip Carl Salzman has observed that:

> A well-known and occasionally discussed problem is the fact that the vast multi-tude of anthropological conferences, congresses, articles, monographs, and col-lections, while adding up to mountains of paper . . . 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 fash-

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48   E. Wolf (1990:588). Kroeber (1948:391) phrases it more gently. He states that sciences such as anthropology "are subject to waves of fashion."

49   Colson (1992:51).

ion. 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.[50]

The degree to which one perceives significant intellectual advances in the piles of publications produced by anthropologists depends on how one measures intellectual advances. Most anthropologists perceive the field as having two coexisting and, at times, overlapping traditions in this regard. One focuses on interpretation and understanding; the other on the scientific accumulation of knowledge. The first standard considers to what degree a set of publications refines a particular framework's approach by addressing problems within it. The second standard involves establishing objective, cumulative accounts that allows one anthropologist to systematically build on the work of others.[51]

These traditions often overlap. Take, for example, Annette Weiner's research among the Trobrianders of Papua New Guinea. By the time of her fieldwork in the early 1970s, the Trobrianders had become well known, thanks to the writings of Bronislaw Malinowski. His *Argonauts of the Western Pacific*, *The Sexual Life of Savages in North-Western Melanesia*, and *Coral Gardens and Their Magic* are seen as anthropological classics.[52] But that didn't mean Weiner could not add to them. As Weiner described it: "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. . . . My most significant . . . departure from [Malinowski] . . . was the attention I gave to women's productive work." She continues: "My taking seriously the importance of women's wealth not only brought women . . . clearly into the ethnographic picture [which was not the case

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50   Salzman in Borofsky (1994:34).
51   See Borofsky (2019:49–53) for an elaboration of these standards.
52   Malinowski (1922, 1929, 1935).

in Malinowski's accounts] but also forced me to revise many of Malinowski's assumptions about Trobriand men."[53] In short, building on Malinowski's classic writings, Weiner refined and extended the analysis of Trobriand society.

The key to advancing knowledge on a group, such as the Trobrianders, seems clear. There needs to be more than one study of the people in the same general locale that in some manner adds additional material to the initial study. The value of a single study, by itself, tends to be uncertain. It needs supporting data to affirm its validity. The accounts need not necessarily agree. But they need, in one way or another, add to our understanding of the group.

The Redfield-Lewis controversy offers an example. It involves the rural Mexican village of Tepoztlán.[54] Robert Redfield studied Tepoztlán in 1926–1927. He wrote an ethnographic description of the village that focused on normative rules. Redfield portrayed neighbors as living in relative harmony. In 1943 another anthropologist, Oscar Lewis, conducted research in the village. Focusing on observed behavior rather than ideal norms, Lewis painted a picture of conflict and factionalism. As Redfield emphasized, the differing accounts led to a better understanding of the village: "The principle conclusion that I draw from this experience is that we are all better off with two descriptions of Tepoztlán 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 Tepoztlán with somewhat stereoscopic vision."[55] Having two accounts—even if they disagree—is better than having one account because they provide a richer, fuller, more credible account of a group.

Because anthropologists rarely visit each other's field sites and do restudies of them, they often have difficulty assessing the credibility of another anthropologists' field work. They tend to rely on certain contextual factors: How does the author's work fit with other work in the area? Does the author convey familiarity with the Indigenous language? Does the author cite important references that, in one way or another, seem relevant to the research? Citing such references makes the author appear knowledgeable about the topic she or he is writing about.

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53   Weiner (1988:5); see also Weiner (1976).
54   Redfield (1930, 1956); O. Lewis (1951, 1960); also note Butterworth (1972).
55   Redfield (1956:136).

In 1968, Carlos Castaneda, drawing on his doctoral dissertation at UCLA, wrote *The Teachings of Don Juan.*[56] The book has sold more than twenty-five million copies—making it one of the most popular ethnographies of all time. Today, many anthropologists suspect the book is a work of fiction. But is it? No one knows for sure. Looking at both sides of the controversy, a March 5, 1973, *Time* article by Sandra Burton, states:

> [The credibility of Castaneda's work] hinges on the credibility of Don Juan as a being and Carlos Castaneda as a witness. Yet there is no corroboration beyond Castaneda's writings that Don Juan did what he is said to have done, and very little that he exists at all. A strong case can be made that the Don Juan books are of a different order of truthfulness. Where, for example, was the motive for an elaborate scholarly put-on? *The Teachings* were submitted to a university press [the University of California Press], an unlikely prospect for best-sellerdom. Besides getting an anthropology degree from UCLA is not so difficult that a candidate would employ so vast a confabulation just to avoid research. A little fudging perhaps, but not a whole system in the manner of *The Teachings*, written by an unknown student with, at the outset, no hope of commercial success.[57]

Given how rarely anthropologists confirm one another's work, it is not unreasonable to wonder how many publications in the field involve unsubstantiated assertions of uncertain, ambiguous value. It remains an open question?

4. Two Case Studies That Should Make Us Ponder

This section focuses on two main points related to the advancement of knowledge in anthropology. First, when two prominent anthropologists utilize the same intellectual framework in their research, they tend not to systematically refine or build on one another's work with new material. They often to go off in different directions from one another—the point Salzman made earlier. Second, other anthropologists using this same framework may extensively cite these prominent figures. The citations suggest they are engaging with these figures' work and building on it. But a closer analysis at the citations indicates that they rarely discuss these figures' work in any depth. They mainly refer to them in passing, usually only for a sentence or two. It is mostly to show readers that they

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56   Castaneda (1968).
57   Wikipedia, s.v. "Carlos Castaneda."

are familiar with these figures as a way of making their own work seem credible. In *An Anthropology of Anthropology*, I examine five frameworks that dominated cultural anthropology from the 1930s through the 1990s: Culture and Personality; Cultural Ecology; Interpreting Myths, Symbols, and Rituals; a (Re)Turn to History; and Postmodernism. Let me take two of these frameworks—Cultural Ecology and Postmodernism—to illustrate my points. (Readers can refer to the full study in *An Anthropology of Anthropology* to track all five frameworks.)[58]

Three of the leading figures in Cultural Ecology were Elman Service, Roy Rappaport, and Marvin Harris. Marshall Sahlins and Service's book played a key role in initiating this trend by reinvigorating the evolutionary approach in anthropology.[59] Service (in *Primitive Social Organization*, 1962) offered a set of evolutionary stages of social organization.[60] Rappaport (in *Pigs for the Ancestors*, 1968, revised in 1984) emphasized that ritual often possesses important adaptive, ecological functions.[61] And Harris (in *The Rise of Anthropological Theory*, 1968) reframed the discipline's history in evolutionary terms.[62] We might perceive in a vague way a certain exchange of ideas among these three books.

But I question whether these works significantly built on one another. They all seemed to go off in different directions and, moreover, deal with different ethnographic locales. If Rappaport had worked among one of the tribal groups discussed by Service, or if Harris, following in Rappaport's heels, had offered a detailed cultural materialistic reinterpretation of Rappaport's work among the Tsembaga that brought new data to light, then we could perceive some sense of intellectual advancement. But that is not what occurs.

Harris, in *Cows, Pigs, Wars, and Witches*, for example, criticizes Rappaport's analysis.[63] But he doesn't really present new data (as Lewis did regarding Redfield's work in Tepoztlán). Rather, he 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, a key ritual occurs well before "the onset of actual nutritional deficien-

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58   Borofsky (2019:41–121).
59   Sahlins and Service (1960).
60   Service (1962).
61   Rappaport (1968[1984]).
62   Harris (1968).
63   Harris (1974).

cies or the actual beginning of irreversible damage to the environment."[64] Harris is simply postulating an alternative perspective to that offered by Rappaport without presenting any new data that migh support his position. He is not refining or building on Rappaport's work in any substantive way.

Aside from Harris's reinterpretation of Rappaport's analysis and Service building on his earlier edited book with Sahlins, these authors don't seriously engage with each other's work. Rappaport briefly cites Sahlins and Service in a critical footnote and adds a citation to Harris in the revised 1984 edition of *Pigs for the Ancestors*.[65] 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' analysis of general and specific evolution (in Sahlins and Service's 1960 work), but he is critical of the analysis, asserting that a different approach would be better.[66] The authors all grapple with the same problem: assuming adaptive value without offering corroborating diachronic data. Ultimately, they all fall back on unsubstantiated conjectures. We are left with much to ponder but little confirmed. New possibilities keep piling up, but we never really see how, in specific cases, ecological and social dynamics are entwined through time in generating adaptive and evolutionary advances.

Another way to examine whether these authors' works constitute a significant intellectual advance by the standards cited above is to consider to what degree other anthropologists intellectually engaged with these key figures. To make the task manageable, I limited myself to citations from articles in five journals: the *American Anthropologist*, *American Ethnologist*, *Current Anthropology*, *Man*, and (because these authors are frequently cited in archaeology) *American Antiquity*. I collected citations for Service (1962), Rappaport (1968[1984]), and Harris (1968) for five, ten, and fifteen years out from their respective publication dates. An examination of the citations made by other anthropologists suggests their publications rarely built on the above cited works. Rather, they mainly cited these figures in passing as a way of showing readers that they were familiar with

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64   Harris (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.

65   Rappaport (1968[1984]:xi, 350).

66   Harris (1968:651–653).

them. If we look at to what degree (of the total collected articles) these other authors made concerted attempts to develop these figures' work—defined as involving more than two sentences of discussion regarding the cited work—we get the following ratios and brackets (explained in the footnote).[67]

> Service (1962) 1/24, [2/24]
> Rappaport (1968[1984]) 1/21, [1/21]
> Harris (1968) 0/16, [0/16]

The other framework I use here to illustrate my points is Postmodernism, which explores the role of the knower in the known, especially the ways ethnographers represent the people they study. To better understand this perspective, we looked at two general works closely associated with Postmodernism—one edited by James Clifford and George Marcus (*Writing Culture*, 1986) and the other written by George Marcus and Michael Fischer (*Anthropology as a Cultural Critique*, 1986).[68] A third book by Marilyn Strathern (*The Gender of the Gift*, 1988) offers an ethnographic example of the approach.[69] The authors of *Writing Culture* consider the rhetorical devices anthropologists apply in presenting their ethnographic materials. Marcus and Fischer call for experimentation in reframing ethnographies, offering a range of possibilities that anthropologists might consider. Strathern examines how we represent others in our writing, especially the way we describe gender, exchange, and social units in Melanesian societies.

Their works offer much food for thought. But I am cautious in believing that the authors' collective efforts embody significant intellectual advancement as defined above. In a vague sort of way, we might perceive these books as entwined with one another. *Writing Culture* (1986) sets the stage, so to speak, for the other books by suggesting that ethnographies can be analyzed as literary constructions. *Anthropology as Cultural Critique* (1986) offers a more systematic account of Postmodernism. It places Postmodernism in historical context and presents a range of "experimental" texts. *The Gender of the Gift* (1988) fleshes

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67   Rather than rely on percentages, as I do with Borofsky (2019), I have used ratios here so readers can get a sense of the number of cases involved. For Service, for example, 1/24 means that out of twenty-four citations to his 1962 book, only one tried to develop an idea in it for more than two sentences. The ratio in brackets, such as [2/24] for Service, means that out of the twenty-four references, only two cited Service for more than two sentences in a review of the literature before presenting their own perspective. The specific citations to Service, Rappaport, and Harris are listed in Borofsky (2019:78–79) as footnote 96.

68   Clifford and Marcus (1986); Marcus and Fischer (1986).

69   Strathern (1988).

out the postmodern agenda ethnographically. It offers an in-depth analysis of Melanesian gender relations and how we, as Westerners, tend to distort these relations in describing them.

However, as with the above framework of Cultural Ecology, the works discussed here do not directly build on or refine one another, either ethnographically or analytically. The contributors to *Writing Culture*, for example, do not collectively deal with a specific ethnographic area in-depth. They discuss a range of locales not necessarily connected to one another. Nor do the authors/editors involved with these works take particular note of each other's publications. Strathern's *The Gender of the Gift*, published two years after the others, doesn't cite either *Writing Culture* or *Anthropology as Cultural Critique*. While Clifford and Marcus list *Anthropology as Cultural Critique*, and Marcus and Fischer list *Writing Culture* in their bibliographies, neither lists the other work in their indexes (allowing readers to see how they were cited). The absence of discussion (or even reference) to Clifford's well-known introductory chapter for *Writing Culture* in Marcus and Fisher is puzzling.[70]

The three books address similar concerns. But again, the authors emphasize their own perspectives rather than engaging with each other's. Critical problems regarding the framework are not adequately addressed. We are left to guess how to assess the experiments and positions the authors embrace if not through traditional intellectual standards. Postmodernism strives to create an impression that it is above the rhetorical politics it analyzes, while in fact being very much a part of them. One senses the framework tries to appear new, innovative, and unburdened by the field's old baggage, while at the same time striving for the traditional disciplinary status rewards and validation.

To determine to what degree other anthropologists, citing the works of Clifford and Marcus, Marcus and Fischer, and Strathern, intellectually engaged with their work, I again examined citations five, ten, and fifteen years out from the

70   While some of the contributors listed in *Writing Culture* are cited in *Anthropology as Cultural Critique*—e.g., Renato Rosaldo, Vincent Crapanzano, Marcus, and Fischer—the ethnographic examples Marcus and Fischer cite in the latter tend to be different from those used by the contributors in the former. That is not to say there is no ethnographic overlap between the two books. Marcus considers 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 Michael Arlen and by Maxine Hong Kingston in both. And Mary Louise Pratt considers Shostak's Nisa (Shostak 1983) in *Writing Culture;* Marcus and Fischer discuss it 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 books.

publication of each of the above books in some of the field's leading journals: the *American Anthropologist*, *American Ethnologist*, *Current Anthropology*, and *Man*. For the fifth journal, I chose one of Postmodernism's leading forums, *Cultural Anthropology* (of which George Marcus was the founding editor). If we look at to what degree, the authors of these articles make a sustained attempt to develop one of the above figures' ideas—involving more than two sentences of discussion—we get the following ratios and brackets (explained in footnote 64).[71]

> Clifford and Marcus (1986) 0/28 [1/28]
> Marcus and Fischer (1986) 0/8 [1/8]
> Strathern (1988) 2/30 [4/30]

In brief, we see in these two examples that anthropologists rarely systematically build on or refine one another's work. Instead, there is a tendency to go off other directions that personally interest them.

5.   Is the Focus More on Advancing Knowledge or on Advancing Careers?

Why have many anthropologists persisted in their claim to be building and refining anthropological knowledge despite, as we saw in the last two sections, this claim diverges noticeably from actual practice? The answer, I suggest, derives from how accountability is framed within the field and, more broadly, within academia. While refining perspectives and building cumulative knowledge are widely affirmed as important professional standards—they fit with what the larger public expects of the field and what scholars need say in order to obtain funding—in practice they are not strictly adhered to. The focus is less on producing publications that advance knowledge than on producing publications needed to advance academic careers.

Publicly embracing these standards conveys to university administrators and the wider public that anthropologists are not self-serving individuals primarily bent on their own aggrandizement. Rather, they are scholars dedicated to advancing knowledge and serving the common good. Such affirmations encourage foundations and governmental organizations to fund their research. Most anthropologists resist the suggestion that they compose their ethnographies out

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71   Please refer to footnote 64 for an explanation of the ratios and the brackets surrounding some of them. The specific references, which readers can check for themselves, are in Borofsky (2019:105–106), footnotes 165, 166, 167.

of thin air. An ethnography often only sells around eight hundred copies today if it is not picked up for use in various undergraduate courses—a small number compared to the millions of books Castaneda's *The Teachings of Don Juan* has sold. While anthropologists claim they are producing professional works of scholarship, few check to ensure their colleagues' writings are not—at least in part—works of creative fiction.

Although it is critical to anthropologists, for funding and for upholding their professional image, to assert they are advancing knowledge, it is not critical that anthropologists actually do so for career advancement. To succeed at this, many lower and reframe the previously discussed standards. Quoting 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."[72] Lowering the standards—from the actual production of reliable knowledge to the appearance of producing such knowledge—serves two important ends. It provides considerable freedom to anthropologists regarding where and what they study. They are not tied to "building on" or "refining" earlier work in specific ways. It also increases the chances for career advancement. Lowering the standards allows anthropologists to produce more publications in a shorter time—just what they need if they are on a publishing treadmill seeking promotion.

Many anthropologists quietly embrace these weaker standards. They are less effective than many might wish for in advancing knowledge. But they work well for those caught up in the push to publish. As previously noted, anthropologists tend to assess a manuscript's credibility by whether the author is familiar with certain references and generally fits with other ethnographic accounts of a region. The author's publications should also include detailed ethnographic and linguistic data that suggest the author conducted extensive research with real people in a real locale.

Another standard for assessing ethnographic work—besides those just cited—emphasizes originality. The striving for originality is not new. The historian William Clark dates the focus on producing new, innovative work to the German Romantic Era of the late eighteenth and early nineteenth centuries.

72   Rhode (2006:11).

Michèle Lamont in *How Professors Think* lists originality as one of the most important criteria in assessing grant applications.[73]

But there is a problem. As Thomas Kuhn has noted in *The Structure of Scientific Revolutions*, ideas that are too original may be rejected or ignored because they challenge the existing consensus.[74] Many anthropologists strive to be original in appearance but not so original as to challenge the currently accepted norms. As a result, many frameworks in anthropology that initially appear new, in fact, may be variations on older ones. This is the point Eric Wolf and Elizabeth Colson made earlier in this essay. 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."[75] "The young build their careers on forgetting and rediscovery," Andrew Abbott observed, "while the middle-aged are doomed to see the common sense of their graduate-school years refurbished and republished as brilliant new insights."[76]

In *Theory and Progress in Social Science*, James Rule writes: 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."[77] But this may not matter, if the focus is on producing publications, rather than on advancing knowledge. How much faculty publish, who cites them, and how "original" their publications appear to be are regularly stressed in faculty assessments. Certainly, some anthropologists publish solid credible work that addresses a framework's major problems and/or builds cumulative knowledge regarding a particular group or topic. But many find this approach unnecessary for moving up the status ladder. The ephemeral, dazzling possibilities that Rule mentions—that arise and then abruptly lose appeal—often work just as well. Sometimes they work better.

The system, as it currently operates, advances key interests within the academic community—not only those of individual academics but also those of their departments and universities as they too search for status. A host of

73   Clark (2006:442); Lamont (2009:167, 171–172).

74   Kuhn (1970).

75   Wallace (1966:1254); also note Lévi-Strauss (1991:91–92).

76   Abbott (2001:147–148).

77   Rule (1997:23).

publications on an exciting new topic raises the status of all three.[78] To what degree funding agencies and universities prefer to focus on the appearance of benefitting others, over actually doing so, is an open question. As Adam Kuper has written, "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."[79] Some might perceive a parallel to what David Keen suggests happens with development agencies. Quoting him: "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 [foreign aid] work better."[80]

In this respect, the lack of transparency is important. It means that people outside of anthropology often puzzle over what is going on within it. They cannot readily evaluate anthropologists' publications. The lack of transparency allows anthropologists, if they choose, to pursue their personal interests without being that beholden to those funding their work.[81]

Appearing to advance knowledge and seeming to serve the broader social good, without significantly disrupting the existing status quo, doesn't threaten the financial and political powers that be. It *supports* them. It portrays everyone as well intentioned.

You might think that many anthropologists would offer something significant in return for the public's financial support. They clearly offer a rhetoric of support. Only some offer more. As long as many anthropologists publicly embrace the strong standards noted in this essay, they can continue to be funded without—as we have seen—actually advancing knowledge within their field in a significant way.[82]

6. Unfortunately, We Are Not Alone

It is important that readers understand that the doubts cast on whether a host of publications produce more than unsubstantiated assertions of uncertain, ambiguous value is not limited to anthropology. It occurs across a gamut of disciplines.

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78   For an elaboration of this theme, see Borofsky (2019:120).

79   Kuper (2009).

80   Keen (1999:28), see also Deaton (2013:274).

81   In an essay titled *Other People's Money and How Bankers Use It*, Justice Louis Brandeis famously states: "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" (1914:92).

82   See June (2018b); Rabesandratana (2018).

I offer a few examples, although readers can certainly find others as well.[83] *The Lancet* reports that perhaps $200 billion—which constitutes about 85 percent of all global medical research spending—is likely wasted on poorly designed and reported research studies.[84] Since this is a fairly shocking figure, let me provide the actual quote from Malcolm Macleod et al.:

> Global biomedical and public health research involves billions of dollars and millions of people. Although this vast enterprise has led to substantial health improvements, many more gains are possible if the waste and inefficiency in the ways that biomedical research is chosen, designed, done, analysed, regulated, managed, disseminated, and reported can be addressed. In 2009, Chalmers and Glasziou . . . estimated that the cumulative effect was that about 85% of research investment—equating to $200 billion of the investment in 2010—is wasted.[85]

In a related article, Paul Glasziou states: "Research publication can both communicate and miscommunicate. Unless research is adequately reported, the time and resources invested in the conduct of research is wasted. . . . Adequate reports of research should clearly describe which questions were addressed and why, what was done, what was shown, and what the findings mean. However, substantial failures occur in each of these elements."[86]

Ben Goldacre, in *Bad Pharma: How Drug Companies Mislead Doctors and Harm Patients*, discusses transparency in relation to the pharmaceutical industry. He reports:

> Missing data is key to the whole story . . . because it poisons the well for everybody. If proper trials are never done, if trials with negative results are withheld, then we simply cannot know the true effects of the treatments that we use. Nobody can work around this, and there is no expert doctor with special access to a secret stash of evidence. With missing data, we are all in it together, and we are all misled . . . evidence in medicine is not an abstract academic preoccupation. Evidence is used to make real-world decisions and when we are fed bad data, we make the wrong decision, inflicting unnecessary pain and suffering, and death, on people just like us.[87]

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83   See, for example, Borofsky (2019:173–193). Also note Yeagle (2021); Al-Khalili (2020).

84   *The Lancet*, for those unfamiliar with the life sciences, is one of the world's leading medical journals.

85   Macleod et al. (2014:101). See also Chalmers and Glasziou (2009:88); Belluz (2017).

86   Glasziou et al. (2014:267).

87   Goldacre (2012:341–342).

How do the social sciences measure up? The *New York Times* reported in 2015:

The past several years have been bruising ones for the credibility of the social sciences. A star social psychologist was caught fabricating data, leading to more than 50 retracted papers. A top journal published a study supporting the existence of ESP that was widely criticized. The journal *Science* [one of the world's leading journals] pulled a political science paper on the effect of gay canvassers on voters' behavior because of concerns about faked data. Now, a painstaking years long effort to reproduce 100 studies published in three leading psychology journals has found that more than half of the findings did not hold up when retested.[88]

In an article titled "Psychology's Replication Crisis Inspires Ecologists to Push for More Reliable Research," Cathleen O'Grady reports that the problems of research integrity and reliability plague ecological research as well.

In a 2018 study published in *PLOS ONE*, Parker, Fidler, and colleagues reported on a survey of more than 800 ecologists and evolutionary biologists. About half of the respondents said they sometimes presented unexpected findings as if they confirmed a hypothesis, they'd had all along, and about two-thirds said they sometimes reported only significant results, leaving out negative ones. Together, these forces mean a literature overflowing with potentially dubious results, Parker says. It's a "house of cards." But unlike psychology, in which researchers have tried to replicate famous studies and failed in about half the cases, ecology has no smoking gun. A 2019 *PeerJ* study found only 11 replication studies among nearly 40,000 ecology and evolution biology papers—and only four of these 11 studies managed to replicate the original finding.[89]

And finally, a recent article in *Science News* (published by the American Association for the Advancement of Science, AAAS) reports on the difficulties involved in having institutions of higher learning embrace higher ethical research standards:

The world's largest multidisciplinary survey on research integrity is in danger of falling short of its goals after two-thirds of invited institutions declined to collaborate, citing the sensitivity of the subject and fearing negative publicity. That left researchers leading the Dutch National Survey on Research Integrity on their own to scrape many email addresses and solicit responses. The survey will close

88   Carey (2015a).
89   O'Grady (2020).

on 7 December, but the team has gathered responses from less than 15% of 40,000 targeted participants . . .

Lex Bouter, who studies research methods and integrity at the Free University of Amsterdam (VU), began to plan the survey in 2016 to address a lack of data about questionable research practices and scientific misconduct. He wanted to ask all working academics in the Netherlands not just about how they conduct their research, but also about work habits, pressures, and other aspects of academic life. Bouter, a former VU president himself, assured the heads of other universities that the survey would not generate an institutional ranking of misbehavior . . .

But at a meeting in December 2019, some university presidents argued that a survey would just not be suitable for such a sensitive topic, Bouter recalls. Others found the survey too focused on bad behaviors, such as data falsification or cherry-picking of results. "I thought it was biased," says Henk Kummeling, president of Utrecht University, which declined to participate. "If you only ask for questionable research practices, you already know what you will get out of the survey." Eventually, five out of the Netherlands's 15 universities agreed to collaborate, on the condition that they could have a say on the survey's setup and content . . .

Jeroen de Ridder, a philosopher of science at VU who is not involved in the study, says he is disappointed that a unique opportunity to study research integrity across disciplines may be lost. He denies the survey has methodological flaws: "This has become the most careful and thorough survey one could wish for," de Ridder says.[90]

My point is that anthropology is not alone in focusing on appearance more than substance in claiming to advance knowledge. Without further information it is unclear how pervasive this practice is. But undoubtedly many disciplines share the same problem.[91]

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I have emphasized several points in this essay. (1) Anthropologists need become more self-reflexive. They need to study their own field in the same systematic and empirical way they use to carry out fieldwork in other settings. (2) In the intertwining of institutions central to the field— funding agencies, universities, and academic presses—there is a hegemonic-like infrastructure that replaces the focus on public benefits with a focus on publications. The implication is that a plethora of publications lead, over time, to concrete benefits for the society as

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90   de Vrieze (2020).
91   O'Grady (2021).

a whole. This certainly holds true for some publications in anthropology. But for many publications, probably majority, it may not hold true. Anthropological publications tend to go off in all sorts of exciting ways. But they rarely systematically build on each other. (3) We often have the appearance of advancing knowledge more than the actual reality. Off to the side, the problem has been noticed for some time in anthropology. Yet noticing the problem is not the same as effectively addressing it. Hence, the problem persists.

(4) This focus on the appearance of knowledge makes sense to many. They can publish more articles if they lower their intellectual standards from the actual production of knowledge to the appearance of producing such knowledge. It offers a way to advance their careers. (5) But the focus on appearances does not usually benefit others beyond the field in demonstratable, meaningful ways – neither the communities that anthropologists work with nor the wider public that, more often than not, funded the research that anthropologists draw on in producing their publications. It also limits, as we have seen, the field's intellectual development.

The key question we must answer, the question the three $1,000 Revitalizing Anthropology Graduate Student Awards asks, is: How do we effectively address this situation? Given the problems highlighted in this essay, how do we overcome them? How do realize—in actions (not just words)—the very real potential of anthropology to facilitate change that demonstrably improves other people's lives in meaningful ways to them?

Quoting Amanda Gorman's "The Hill We Climb"

> For there is always light,
> if only we're brave enough to see it
> If only we're brave enough to be it.

PLEASE NOTE: Further Information on the three $1000 Revitalizing Anthropology Graduate Student Awards is available at http://revitalizing.publicanthropology.net.

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