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Anthropology's Science Wars**Insights from a New Survey**

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In recent decades the field of anthropology has been characterized as sharply divided between proscience and anti-science factions. The aim of this study is to empirically evaluate that characterization. We survey anthropologists in graduate programs in the United States regarding their views of science and advocacy, moral and epistemic relativism, and the merits of evolutionary biological explanations. We examine anthropologists' views in concert with their varying appraisals of major controversies in the discipline (Chagnon/Tierney, Mead/Freeman, and Menchú/Stoll). We find that disciplinary specialization and especially gender and political orientation are significant predictors of anthropologists' views. We interpret our findings through the lens of an intuitionist social psychology that helps explain the dynamics of such controversies as well as ongoing ideological divisions in the field.

Controversy beckoned in 2010 when the leadership of the American Anthropological Association (AAA) proposed removing reference to "science" in its long-term plan. The "science wars" of the nineties were a not-so-distant echo, after all, and yet the scope of the response was unexpected. Writing in the *New York Times* shortly thereafter, science journalist Nicholas Wade (2010) suggested that the decision reopened a "long-simmering tension" among anthropologists, whose science-oriented and activist "factions" experienced a phase of "bitter tribal warfare." Alice Dreger (2010) echoed Wade's construal and did not hesitate to take sides. Decrying the AAA's view of science as a "four-letter word," Dreger faulted those she dubbed "fluff-head" cultural anthropologists prone to appear as "superheroes for oppressed peoples." The danger for the "real scientists" (including some "non-fluff-head" cultural anthropologists) is a climate within the discipline that "actively denigrates science" as just "another way of knowing" and "promotes activism over data collection and scientific theorizing" (Dreger 2010).

As the storm surged into the blogosphere, anthropologists raised the alarm. Peter Wood (2010) warned in the *Chronicle of*

Higher Education about a "self-appointed radicalizing faction" threatening the discipline. Shorn of its scientific basis, anthropology would become, Wood feared, "little more than colorful travel literature (travelogues) occasionally mixed up with political hucksterism and theoretical obscurantism." Peter Peregrine, president of the Society for Anthropological Sciences, emailed group members about the troubling development and shared their resolution condemning the abandonment of the field's core principles.

While sensationalized as an "implosion of anthropology" (Rosenberg 2010) or disciplinary "civil war" (Hartwell 2010), the AAA executive board sought to defuse matters and reached out to association leaders. In its formal response, the leadership stressed that changes to the long-term plan were blown out of proportion by media misrepresentations, which gave the mistaken impression of a field divided. Indeed, they underscored their endorsement of the scientific method as "crucial to much anthropological research," noting that the changes were never intended "to signal a break with the scientific foundations of anthropology." Executive board member Hugh Gusterson followed suit in a joint radio interview with Peregrine (Lehrer 2010). He rejected the characterization of an antiscientific current among anthropologists. "This is an unfortunate exhumation of a battle that ended a decade ago," Gusterson remarked, noting that there was a time in the nineties when postmodern scholars embraced the "social construction of knowledge"—but that "those days have ended" (Lehrer 2010). He added that "many activist-oriented anthropologists" who are drawn to advocacy today actually "embrace science."

Peregrine was unmoved. "There is a real division within the organization," he stressed, one that has marginalized evolutionary anthropologists in particular. Peregrine contrasted

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those anthropologists who seek to explain widespread cultural variations—generally through a biocultural lens—with those more inclined to description and understanding of cultural particularity. “There is a subgroup of descriptive interpretive anthropologists,” he asserted, “who do aggressively attack scientific approaches.” Indeed, it is precisely this “very deep rift” in the field that has made the battle over its mission “rage the way that it has” (Lehrer 2010).

One scholar who has fallen into perhaps the deepest depths of that rift is Napoleon Chagnon. If not the most maligned anthropologist in memory (Eakin 2013), Chagnon’s research among the Yanomami has certainly polarized the anthropology community. We address the Chagnon controversy centrally in our survey. Suffice to note here that he has not shrunk from calling out what he sees as antiscientific attitudes among his colleagues. In the final pages of his provocative memoir, Chagnon (2013) chimes in regarding the AAA mission flap. He laments that in the past 20 or so years, cultural anthropology has come “precipitously close to abandoning the very notion of science.” In his view, there is indeed a schism between anthropologists who “do science” and those whose exclusive goal is to “advocate on behalf of native peoples.” The latter are not just wrong, Chagnon contends, but also “border on irresponsibility,” as they define their advocacy as “incompatible with science.” Moreover, Chagnon echoes Peregrine in linking anthropology’s emphasis on postmodernism and advocacy to a deep current of “biophobia”—a “chronic opposition in cultural anthropology to ideas from biology” that may “help account for what humans in all cultures did.”

Disciplinary harmony is hardly expected in academia and perhaps less so in four-field anthropology. Yet here we have a field allegedly fractured over the very foundations of scientific practice, objectivity, and ethical responsibility. Are such characterizations accurate? Is anthropology in fact divided into demonstrably proscience and antiscience factions? Do postmodern sentiments endure in the discipline, undermining scientists’ commitment to objectivity and capacity for dispassionate approximations to truth? We examine these questions and more with an eye to anthropologists’ political identities. We hope, in this regard, to bolster a moral intuitionist interpretation of knowledge production proposed in prior research.¹ We will turn to our survey in a moment, but first let us sketch the normative

1. We build here on former investigations of controversies in sociology, economics, and evolutionary anthropology (Horowitz and Hughes 2017; Horowitz, Yaworsky, and Kickham 2014; Yaworsky, Horowitz, and Kickham 2015). Our aim is to contribute to a social psychology of knowledge in the social sciences. We take a social-psychological rather than a traditional “sociology of knowledge” approach in that the latter tends to highlight wider historical and cultural forces that shape knowledge production from without. We suggest a complementary social-psychological approach that attends “bottom up” from scholars’ moral intuitions (and consequent political identities) to their often “tribal” constructions of knowledge. We unpack these ideas in further detail in our discussion below.

tenor of the field’s major debates to provide context for our chosen questions.

Construing the Postmodern Turn

Characterizing a movement or moment as postmodern is a dicey undertaking. Whether we attend to the boundaries of anthropology or beyond, the postmodern turn means many different things to observers of many different shades of sympathy, suspicion, or antipathy. Reflecting on his role in the meteoric *Writing Culture*, Clifford (2015:25–31) suggests that the text “imperfectly registered” anticolonial and feminist sensibilities. Set against the backdrop of a late 1960s to early 1990s “transitional moment,” Clifford describes a chastened insurgent outlook that replaced “frontal resistance to hegemony” with “subversion,” “critique,” and the clearing of space for “discrepant senses of the real” (25–31).

This decentering of hegemonic truth claims toward an “epistemological diversity,” as Clifford puts it, is dissected by Marcus and Fischer (1996) in their widely cited discussion of the “crisis of representation” in the human sciences. No longer confident in grand theoretical explanations—and ever attentive to power relations and discursive contexts—interpretive ethnographies would supplant generalizing explanatory frameworks in an avowedly contingent social world. Stocking captures the mood perhaps best in his depiction of an increasingly self-reflexive, fin de siècle anthropology:

Ideologically, it would move beyond the liberal posture of relativistic tolerance towards one of radical engagement in the struggles of the powerless against the holders of power. Methodologically and epistemologically, it would reject the positivistic assumption that cultures or cultural behavior could be observed as “objects” in the external world, and recognize the essential reflexivity of participant observation, and the inherently problematic character of the knowledge generated by the ethnographic process. (Stocking 1995:65–66)

Stocking refers to anthropology’s “relativizing critique” of science over three decades, as well its “debiologizing” and historicizing tendencies throughout the century. The many permutations in the debate over the politics of representation exceed our scope here. Yet we see in our brief sketch that the twin concerns of political responsibility in a stratified world and suspicion of authoritative representations of that world (especially by Western observers) have long preoccupied anthropologists. Indeed, the spirit of such concerns appears to animate some of the most vivid controversies in the field.

Moral Readings and Epistemological Angst

Machetes, Menchú, and Mead

Anthropological controversies over the years often occasion what Segerstrale (2000) calls “moral reading.” When a text or theory is read morally, it is considered in terms of the typically baneful political consequences it allegedly causes. Such

normative critique may call out an author's hidden ideological agenda or hold them accountable for how their findings are used in the wider political arena.

Chagnon's work is prototypical in this regard. Shortly after publishing his 1988 *Science* article linking male violence to marital and reproductive success among the Yanomami, the Brazilian Anthropological Association sent a letter to the AAA ethics committee alerting North American anthropologists to the danger (Carneiro da Cunha 1989). The letter condemned Chagnon's misrepresentations of Yanomami violence, which were appropriated in the press by political and mining interests bent on exploiting the region's land and resources. Similarly, Chagnon's behavior in the field has been subject to withering critique. Most famously, Marshall Sahlins condemned Chagnon's ethical breaches in obtaining genealogies from a people deeply reticent about revealing the names of the dead. Sahlins (2000) denounced Chagnon's use of "draconian devices" to solicit names, from "bribery" and trading of machetes and other goods to "exploiting animosities" in the villages (much of which Chagnon acknowledges in his writings). For Sahlins and other critics, Chagnon helped instigate the very violence he would later use in stereotyping the Yanomami as "fierce." All of this was done, Sahlins lamented, "for the sake of science"—despite the irrevocable "pain and hate" visited upon the Yanomami.²

Criticism of Chagnon's research has not been limited to its purported moral offenses or political misuses. Scientific and empirical challenges abound. Above all, critics contest his sociobiological account of the roots and extent of Yanomami violence. Ferguson (1995a, 2015) highlights broader historical factors in shaping patterns of violence—particularly Western colonial influence through proximity to trade in highly sought steel tools. He and others dispute Chagnon's statistics as well, especially his data on *unokai* "killers"—dismissed by Sahlins (2000) as having perhaps the "shortest half-life" of any study in *Science*—as well as the nature and rate of female abductions (e.g., Albert 1989; Lizot and Dart 1994). In the end, Chagnon's critics paint what they see as a more balanced picture of Yanomami aggression, attentive to historically specific causes in no way attributable to an allegedly primordial Hobbesian past.

Our report does not aim to adjudicate conflicting claims in this or other controversies. We submit, however, that disentangling the empirical and the normative in these debates is

2. Sahlins would later resign from the National Academy of Sciences, partly in protest over Chagnon's election. Chagnon's critics cheered the move. Perhaps the most caustic (and colorful) criticism was penned by David Graeber, who suggested that Chagnon "vilified a group of human beings so that enormous violence could be unleashed on them." Graeber continued by affirming that Sahlins is a "man of genuine principle," who "never had a lot of patience for shirtless macho Americans who descend into jungles declaring their inhabitants to be violent savages, and then use that as an excuse to start behaving like violent savages themselves—except with command over infinitely greater technological resources" (cited in Golden 2013).

tricky, not least because they implicate authors' moral intuitions and "emotive communities" (more on this below). We hope nonetheless that our sketch of the social psychological aspects of such controversies contributes to greater self-consciousness in the field, perhaps aiding deeper consensus on vital empirical matters down the road.

Although hardly as tenacious as the Chagnon saga, David Stoll's (1999) exposé of the *testimonio* of famed Guatemalan activist Rigoberta Menchú certainly piqued passions. Stoll discovered several claims in her narrative that did not square with the facts (e.g., that Menchú was illiterate and received no formal education, that wealthy ladinos sought belligerently for decades to steal her family's land, and that she witnessed the immolation of her brother by the Guatemalan army). Stoll contested many such claims, citing archival records and scores of interviews with neighbors and extended family. Perhaps most contentious was his broader challenge to Menchú's portrayal of widespread peasant provocation and support of the guerrilla movement.

Stoll's book and consequent *New York Times* reporting (Rohter 1998) were embraced by far-right observers as vindication of Menchú's mendacity (D'Souza 1998; Horowitz 1999). Progressive and academic responses were far from friendly to Stoll, however, as moral readings snowballed (Arias 2001). Pratt (2001) rebuked Stoll's "hubris," asking how, in light of the Guatemalan army's "monstrosities" and the US government "complicity," one could have "an ethical compass whose point of orientation is not these truths but the truth that Menchú was not present when they killed her brother or that she does not discuss her family's internal quarrels" (Pratt 2001:43–46). Sommer (2001) added that Stoll was "probably right to quibble over Rigoberta's facts," yet asked why he "wants to shift attention away from the genocidal war waged by the Guatemalan government against its indigenous population" (239).

While some challenged the validity of Stoll's findings—accusing him of cherry-picking his sources or questioning their veracity—a more common response was to raise larger epistemological questions about the nature and intent of *testimonio*. Attentive to the "geopolitics of truth" (Pratt 2001), the genre became framed as a collective political enterprise, representing plural and marginalized voices rather than a single "objective" truth privileged by Western academics (Fermin 2001; Smith 2001). The arguments in this vein tend to be subtle, if not opaque, prompting critics to dismiss them as "postmodern obfuscations" (Patai 2001:273). Hence Pratt (2001) writes that "marginalized groups are insisting on entering into dialogue with lettered knowledge, from alternative epistemological grounds;" *testimonio* has "undermined the hierarchy" of "lettered knowledge over narrated experience;" and "despite the metropolitan cult of the fact, it is common in everyday practice that verisimilitude counts as veracity" (41–43). Smith (2001) concurs by stressing "new ways" of seeing "truth and responsibility" that welcome diverse "positions and positionalities" (151–152). Yet "Stoll's very positionality," she cautions, represents "the illusory truth of 'objective' reportage" (Pratt 2001:151–152).

While calls for multivocality shorn of the pursuit of objectivity strike us as paradoxical—and we are wary of alternative epistemological grounds that are left unspecified—we will hold our reflections on questions of power and epistemology for our concluding discussion. Here we step back from the polemics to take stock of the field. Does distrust toward scientific objectivity characterize contemporary anthropology? Is such epistemic relativism more preponderant in cultural anthropology than in the other subfields? Are there demographic factors that may influence variation in anthropologists' views?

The third and no doubt most famous controversy addressed in this survey is Derek Freeman's (1983, 1999) challenge to Margaret Mead's classic research on adolescence in Samoa. Without belaboring the details of this familiar drama, suffice to note that the claims and counterclaims about Samoan life were politicized right out of the gate. As Shankman (2009:210) notes in his absorbing overview, Freeman's book found a receptive audience among conservatives in the context of the culture wars of the eighties. His purported debunking of Mead's allegedly ideological embrace of cultural relativism was welcome news to those who viewed moral laxity as the prime culprit of societal ills. Moreover, the fledgling field of sociobiology found the characterization of Mead as an antievolutionary cultural determinist an inviting foil, as they sought to deepen the scientific heft of their budding program. Popular evolutionary psychologists such as Steven Pinker, David Buss, and Matt Ridley would echo aspects of Freeman's critique, including his claim that Mead was duped by a couple of her Samoan informants (Shankman 2009:207–210). As Shankman shows through careful research, however, Freeman's portrayal of Mead entailed substantial cherry-picking from her writings, consistently omitting her references to biological determinants of behavior, an ironic finding given Freeman's critique of Mead's ideologically driven agenda.³

However one-sided Freeman's representation of Mead, it is evident that the Mead/Boas legacy is typically evoked in terms of its moral relativist critique of old Eurocentric claims of biological and cultural superiority. Indeed, Mead's career embodies a tension still visible in anthropology today—a passionate commitment to advocacy and human rights coupled with hesitancy, given the legacy of colonialism, to privilege Western society's norms or values over those of others. For critics of moral relativism today, however, what was once a corrective to colonial racism has become a logically self-refuting and politically irresponsible position (see, e.g., Bagish 1981; Harris 2010).⁴

To be sure, questions of relativism, advocacy, and ethical responsibility endure in the field (e.g., Abu-Lughod 2002;

3. In his new afterword to the 2016 edition of *The Blank Slate*, Pinker acknowledges (following his reading of Shankman) that his uncritical repetition of Freeman's claims needs to be "modified."

4. Harris (2010:221) refers to what he sees as widespread moral relativism among liberal intellectuals as a kind of "learned pathology." Hardly mincing words, he condemns the AAA's well-known statement to the United Nations in 1947—widely seen as an endorsement of relativism—as "the best the social sciences could do in the United States with the crematoria of Auschwitz still smoking" (221).

D'Andrade 1995; Scheper-Hughes 1995; Shweder 2000). We will hardly resolve such matters in this study. We are surprised, however, that our survey appears to be the first to attempt to gauge the field as a whole on these issues. We question anthropologists candidly and in straightforward language, querying them as well on controversial recent research revisiting the explosive category of race (Cochran, Hardy, and Harpending 2006).

Methods

Survey Sample and Questions

We compiled a list of 1,632 anthropologists in all doctoral anthropology programs in the United States using AAA's *AnthroGuide 2013–2014*. We included additional faculty on departmental websites and eliminated all invalid, repeating, or unverifiable emails, for a revised list of 1,553 professors. After an email and follow-up in October 2016, we received 301 usable surveys, for a 19% response rate.⁵ We hope that our focus on faculty in doctoral programs affords a glimpse into the likely direction of the field, given their role in training future anthropologists. Table 1 reports relevant characteristics of our sample, including demographics and respondents' self-identified political orientations.

Substantively, our survey addresses questions grouped into the following four thematic categories: (1) science and epistemology, (2) moral relativism and advocacy, (3) biology and culture, and (4) controversial legacies. Table 2 lists the questions.

As can be seen, we crafted the questionnaire while being attentive to the aforementioned splits over epistemology, activism, and the merits of evolutionary biology. Respondents were asked Likert-type items, that is, whether they "strongly agree," "agree," "neither agree nor disagree," "disagree," or "strongly disagree" with the 38 statements.⁶ We are cognizant that formulating workable survey questions often invites objections, especially with regard to controversy, so we aimed in our framing to use as familiar and unambiguous language as possible. We begin here with an overview of the findings of the field as a whole, then turn to interpretation of salient and indeed surprising patterns in the data.

Findings

A Bird's-Eye View of the Field

Table 3 provides a description of anthropologists' overall responses. The findings at this level of generality are simple yet offer a useful springboard to explore obvious points of consensus or division.

5. Note that we had 342 original entrants, with 41 electing not to answer a single substantive question (beyond demographics).

6. These include six speculative "Biology and culture" questions (BC5–BC10) that ask respondents to gauge the "plausibility" or "implausibility" of the respective items.

Table 1. Sample characteristics of anthropologists

Characteristics	Respondents (<i>n</i> = 301)	Percent
Gender:		
Female	126	42
Male	172	58
Age:		
30–45	81	27
46–61	120	40
62+	97	33
Subdiscipline:		
Archaeology	93	32
Biological	70	24
Cultural	118	41
Linguistic	9	3
Academic rank:		
Assistant professor	46	16
Associate professor	91	33
Professor	142	51
Political orientation:		
Radical	37	13
Liberal	213	73
Moderate	36	12
Conservative	2	1
Libertarian	3	1

Note. The table leaves out a modest number of missing cases across categories.

We see in items SE1 and SE2 that anthropologists in our sample overwhelmingly affirm the value of science to society, with strong majorities agreeing that science improves human well-being (89%), and that the voices of scientists should be heeded in government policy (86%). Moreover, only a small fraction (13%) affirms that the AAA's 2010 proposal to remove "science" from its long-term plan was a worthy aim. Such views should give pause to facile characterizations of the field as antiscience.

With regard to epistemology, however, the picture muddies. Here we see sizable minorities of anthropologists agree that traditional indigenous cosmologies are "no less 'true' than modern scientific explanations" (31%) and that science is "just one way of knowing, no more valid or accurate than other approaches to knowledge" (22%). Although a solid majority of respondents (67%) concur that the best scientific theories provide "truthful representations of reality," fewer than half (47%) reject the view that anthropologists' depictions of indigenous cultures "reflect more their own power and interests" than "'true' descriptions of those cultures." Although we cannot attend to every question here, the ambivalence of the field regarding epistemology is unmistakable. In fact, modest majorities affirm both that postmodern theories make an "important contribution to the field" (60%) and that "antiscientific attitudes" are undermining the discipline (54%).

Questions of moral relativism reveal greater consensus. Eighty-nine percent of respondents support the UN Declaration of Human Rights, and 72% personally view female genital mutilation as "brutal and unethical" wherever it is practiced. Only about one-sixth of the field (18%) believes that the

procedure should not be condemned by Western anthropologists. These findings render hyperbolic claims by critics such as Harris (2010) that the field is muddled in an allegedly obtuse moral relativism.

Considerations of advocacy divide the field, however. Fifty-four percent of anthropologists affirm their ethical responsibility to "stand in solidarity with the marginalized groups they study." Fifty-two percent reject the view that the AAA should avoid "politically oriented" resolutions or boycotts. And last, anthropologists are about evenly divided over whether advocacy and fieldwork should be kept separate to protect the "objectivity" of research (SE10).

The biology and culture questions appear most divisive. With the exception of queries on race—where large majorities deny the utility of the concept and the reality of "racial" differences (BC3, BC4, BC10)—the field's appraisal of evolutionary biology is contentious. Just under one-third of respondents (32%) view as "misguided" evolutionary attempts to uncover "universal" features of human cultures. Yet 57% of respondents attest to the "important contribution" that evolutionary perspectives make in explaining "key aspects" of such cultures. We see comparable divisions when we turn to more speculative questions regarding humans' evolutionary heritage. Anthropologists are closely split over the plausibility of whether human prehistory was more peaceful than industrial society, whether tribal conflict was a principal selective force in human nature, and whether men have evolved a desire for more sexual variety than women (BC5, BC6, BC8).

The legacies of Mead and Chagnon suggest a mixed bag.⁷ Two-thirds of respondents agree that Mead "romanticizes the sexual freedom of Samoan adolescents," while just under one-half affirm that she was "ideologically motivated" to do so. A modest 28% endorse Derek Freeman's charge that Mead was duped by a number of her Samoan informants, while only 36% find Freeman's 1983 critique "unconvincing." Regarding Chagnon, and given his notoriety, it is hardly surprising that anthropologists' responses would prove sharply polarized. Indeed, the field is closely divided over whether Chagnon "harmed the Yanomami by acting unethically in his fieldwork" and whether his provocative research on unokai reproductive success is "credible" (CL5, CL7). While close to half of the field (48%) finds his work "ideologically motivated," almost as many respondents (45%) view Patrick Tierney's ruinous allegations in *Darkness in El Dorado* as "thoroughly discredited."

We see, hence, a field divided. To probe more deeply the variegated results, we turn to three areas where significant

7. We will not address the David Stoll/Rigoberta Menchú controversy here due to the paucity of meaningful survey responses. (Although we return to it in our discussion below.) Despite the fervor of the controversy in the late nineties, relatively few anthropologists appear conversant with it today and fewer yet take decided positions. To wit: 55% of anthropologists reveal "no knowledge" of the controversy, and among the others, fewer than one in five (on average) mark agreement or disagreement with any of the corresponding survey items.

Table 2. Survey items

Survey item	Survey item description
Science and epistemology (SE):	
SE1	On balance, advances in scientific knowledge over the years have improved human well-being.
SE2	Society would be better off if the consensus views of scientists were better heeded in government and public policy.
SE3	On the whole, traditional indigenous knowledges regarding medicine or cosmology are no less “true” than modern scientific explanations.
SE4	Science is just one way of knowing, no more valid or accurate than other approaches to knowledge.
SE5	At their best, scientific theories provide truthful representations of reality beyond merely instrumentally useful predictions.
SE6	Anthropologists’ depictions of indigenous cultures reflect more their own power and interests as knowledge producers than “true” descriptions of the reality of those cultures.
SE7	Anthropology is undermined today by antiscientific attitudes in the discipline.
SE8	The American Anthropological Association’s (AAA) 2010 proposal to remove references to “science” in its long-term plan—affirming that the purpose of the association is to advance “public understanding of humankind”—was a worthy effort.
SE9	Postmodern theories have made an important contribution to anthropology.
SE10	Advocacy and fieldwork should be kept as separate as possible to help protect the objectivity of the research.
Moral relativism and advocacy (MR):	
MR1	I support the UN Universal Declaration of Human Rights.
MR2	No matter how “brutal” some cultural practices may appear (e.g., female genital mutilation, sati, etc.), we should not condemn such practices as anthropologists from Western societies.
MR3	I personally find female genital mutilation (type III—infiltration) to be a brutal and unethical procedure regardless of how it is viewed in the places where it is practiced.
MR4	I consider myself to be a moral relativist.
MR5	Anthropologists have an ethical responsibility within their research to stand in solidarity with the marginalized groups they study.
MR6	As an academic organization, the American Anthropological Association (AAA) should not be involved in proposing any politically oriented resolutions, academic boycotts, et cetera.
Biology and culture (BC):	
BC1	Attempts to use evolutionary biology to explain “universal” features of human cultures are misguided.
BC2	Evolutionary biological perspectives make a significant contribution to explaining key aspects of human cultures.
BC3	The social construct of “race” has no corresponding biological reality.
BC4	Attempts to discover a genetic component to behavioral differences between “racial” groups are bound to fail.
BC5	Whatever the variability, foraging societies in prehistory were on the whole more peaceful than later agricultural or industrial societies (i.e. they experienced less violence and homicide on average).
BC6	Tribal conflict was a principal selective force that shaped human nature.
BC7	People’s taste for foods containing fat and sugar is prewired into our brains by evolution. Such high-energy foods conferred a fitness advantage in humans’ ancestral environment—hence people evolved an impulse to gorge on such foods.
BC8	Men have a biologically evolved desire for more sexual variety than women.
BC9	Multiple sexual partners among women and men was common in foraging societies in prehistory, where paternity was not a major concern.
BC10	The comparatively high IQ scores and disproportionate scientific contributions of Ashkenazi Jews (e.g., Nobel and Fields Medal Prizes, etc.) reflect in part a genetic component of their intelligence.
Controversial legacies (CL):	
CL1	In <i>Coming of Age in Samoa</i> , Margaret Mead romanticizes the sexual freedom of Samoan adolescents.
CL2	Mead’s description of Samoan sexual norms was significantly influenced by her being duped by some of her Samoan informants.
CL3	Derek Freeman’s criticism of Mead in <i>Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth</i> is unconvincing.
CL4	Mead was ideologically motivated to present a permissive portrait of sexual norms in Samoa.
CL5	Napoleon Chagnon harmed the Yanomami by acting unethically in his fieldwork.
CL6	Chagnon was ideologically motivated to present a bellicose view of human nature in his research.
CL7	Chagnon’s research documenting a link between male violence and reproductive success among the Yanomami is credible.
CL8	The major claims in Patrick Tierney’s <i>Darkness in El Dorado</i> have been thoroughly discredited.
CL9	David Stoll’s <i>Rigoberta Menchú and the Story of All Poor Guatemalans</i> accurately documents a number of misrepresentations by Menchú about her life, such as her educational background and the nature of her family’s land dispute.
CL10	Stoll’s search for factual misrepresentations in Menchú’s story was reprehensible given Menchú’s background in the Guatemalan civil war (including the army’s murder of members of her family).
CL11	Stoll’s research was carried out poorly.
CL12	Stoll was politically motivated to discredit Menchú and Guatemala’s guerilla movement.

Table 3. Distribution of survey responses

Survey item	Question description	<i>n</i>	Percent agree ^a	Percent disagree ^a
Science and epistemology (SE):				
SE1	Scientific advances have improved well-being	289	89	2
SE2	Society better off if scientists heeded on policy	288	86	3
SE3	Traditional indigenous knowledges no less "true"	286	31	41
SE4	Science is just one way of knowing	289	22	61
SE5	Best theories represent reality truthfully	282	67	9
SE6	Depictions reflect power more than "truth"	281	17	47
SE7	Field is undermined by antiscientific attitudes	288	54	28
SE8	AAA removing reference to "science" a worthy effort	288	13	65
SE9	Postmodern theories made important contribution	288	60	23
SE10	Advocacy and fieldwork kept separate for objectivity	292	39	43
Moral relativism and advocacy (MR):				
MR1	Support UN Declaration of Human Rights	300	89	1
MR2	"Brutal" cultural practices should not be condemned	300	18	58
MR3	Female genital mutilation is brutal and unethical	300	72	9
MR4	I consider myself to be a moral relativist	301	29	38
MR5	Should stand in solidarity with marginalized groups	291	54	17
MR6	AAA should not propose resolutions, boycotts, etc.	293	33	52
Biology and culture (BC):				
BC1	Evolutionary "universal" explanations misguided	283	32	45
BC2	Evolutionary biological perspectives contribute	281	57	23
BC3	Social construct of "race" has no biological reality	281	76	15
BC4	No genetics to "racial" behavioral differences	280	78	8
BC5 ^b	Foraging societies in prehistory more peaceful	280	37	34
BC6 ^b	Tribal conflict was a principal selective force	278	36	33
BC7 ^b	Taste for fat and sugar prewired by evolution	276	65	17
BC8 ^b	Men have evolved desire for more sexual variety	274	31	38
BC9 ^b	Multiple sexual partners in prehistory common	268	41	20
BC10 ^b	Intelligence of Ashkenazi Jews genetic component	273	14	57
Controversial legacies (CL):				
CL1	Mead romanticizes sexual freedom of Samoans	260	66	9
CL2	Mead duped by Samoan informants	261	28	25
CL3	Freeman's criticism of Mead unconvincing	261	36	19
CL4	Mead ideologically motivated	264	49	10
CL5	Chagnon harmed the Yanomami	263	41	34
CL6	Chagnon ideologically motivated	263	48	24
CL7	Chagnon's research credible	262	38	31
CL8	Tierney's <i>Darkness in El Dorado</i> discredited	251	45	13
CL9	Stoll accurately documents Menchú	218	26	4
CL10	Stoll's search for misrepresentations reprehensible	217	12	18
CL11	Stoll's research was carried out poorly	211	8	5
CL12	Stoll politically motivated	215	18	5

Note. AAA = American Anthropological Association.

^a Percentages rounded and collapse "agree/strongly agree" and "disagree/strongly disagree."

^b BC5-BC10 percentages collapse "plausible/highly plausible" and "implausible/highly implausible."

patterns emerged in the data: anthropologists' subfield specialty, gender, and political identity.

Disciplinary Divisions

Tables 4, 5, and 6 report responses by anthropological specialty, gender, and political orientation. These descriptive tables are helpful in providing baselines with regard to where the various groupings stand on the survey items. Reading them in tandem with our regression models in table 7 nicely captures both the meaning and the statistical significance of the salient patterns.

Regarding subfield, the first anthropological camp appears to be biological anthropologists, who vary significantly from

their colleagues on 16 survey items across all four thematic categories (see table 7).⁸ Biological anthropologists are much less likely to reveal any hint of epistemological relativism than their colleagues. They are significantly less likely to view science as just "another way of knowing" (SE4) or to value the contributions of postmodern theories (SE9). They contrast sharply with their sociocultural colleagues by affirming that research "objectivity" should be protected by strict separation of advocacy and fieldwork (SE10), and they are significantly

8. Note that we use linguistic anthropologists as the reference group in the subfield models. This makes sense, as they make up the smallest branch of the discipline, reflected proportionally in our sample.

Table 4. Distribution of survey responses by anthropological specialty

Survey item	Question description	Archaeology (% agree/disagree) ^a	Biological (% agree/disagree) ^a	Cultural (% agree/disagree) ^a
Science and epistemology (SE):				
SE1	Scientific advances have improved well-being	93/2	93/0	85/3
SE2	Society better off if scientists heeded on policy	87/3	90/4	85/3
SE3	Traditional indigenous knowledges no less "true"	32/37	20/63	37/31
SE4	Science is just one way of knowing	22/63	8/85	29/46
SE5	Best theories represent reality truthfully	68/9	78/9	60/8
SE6	Depictions reflect power more than "truth"	20/43	19/36	14/56
SE7	Field is undermined by antiscientific attitudes	60/23	85/4	31/47
SE8	AAA removing reference to "science" a worthy effort	7/76	3/89	20/49
SE9	Postmodern theories made important contribution	63/23	39/41	69/12
SE10	Advocacy and fieldwork kept separate for objectivity	51/38	54/29	21/55
Moral relativism and advocacy (MR):				
MR1	Support UN Declaration of Human Rights	94/1	84/1	88/1
MR2	"Brutal" cultural practices should not be condemned	15/57	24/63	16/56
MR3	Female genital mutilation is brutal and unethical	84/2	80/4	56/16
MR4	I consider myself to be a moral relativist.	33/33	32/35	21/44
MR5	Should stand in solidarity with marginalized groups	51/19	49/18	60/13
MR6	AAA should not propose resolutions, boycotts, etc.	34/49	45/41	25/58
Biology and culture (BC):				
BC1	Evolutionary "universal" explanations misguided	30/46	16/67	44/31
BC2	Evolutionary biological perspectives contribute	61/17	77/8	41/38
BC3	Social construct of "race" has no biological reality	79/14	61/28	81/9
BC4	No genetics to "racial" behavioral differences	69/9	73/14	87/1
BC5 ^b	Foraging societies in prehistory more peaceful	31/39	30/44	48/23
BC6 ^b	Tribal conflict was a principal selective force	38/33	44/30	30/39
BC7 ^b	Taste for fat and sugar prewired by evolution	62/16	83/7	58/22
BC8 ^b	Men have evolved desire for more sexual variety	26/38	45/28	24/45
BC9 ^b	Multiple sexual partners in prehistory common	36/11	35/37	48/18
BC10 ^b	Intelligence of Ashkenazi Jews genetic component	14/57	21/51	9/64
Controversial legacies (CL):				
CL1	Mead romanticizes sexual freedom of Samoans	63/6	63/5	71/12
CL2	Mead duped by Samoan informants	33/15	30/16	24/37
CL3	Freeman's criticism of Mead unconvincing	28/21	24/19	49/18
CL4	Mead ideologically motivated in Samoa	46/14	37/8	59/8
CL5	Chagnon harmed the Yanomami	39/30	17/60	58/23
CL6	Chagnon ideologically motivated	51/21	20/38	66/19
CL7	Chagnon's research credible	38/23	64/11	26/47
CL8	Tierney's <i>Darkness in El Dorado</i> discredited	44/6	69/2	34/24
CL9	Stoll accurately documents Menchú	21/7	16/0	33/3
CL10	Stoll's search for misrepresentations reprehensible	8/15	4/18	19/20
CL11	Stoll's research was carried out poorly	10/3	6/4	7/7
CL12	Stoll politically motivated	11/3	10/2	24/9

Note. AAA = American Anthropological Association.

^a Percentages rounded and collapse "agree/strongly agree" and "disagree/strongly disagree."

^b BC5–BC10 percentages collapse "plausible/highly plausible" and "implausible/highly implausible."

more likely to believe that the AAA should not propose any politically oriented resolutions or boycotts (MR6).

Unsurprisingly, biological anthropologists are significantly more likely to endorse evolutionary approaches to human cultures. Yet they differ significantly on the more speculative questions as well. They are much more receptive to evolutionary explanations of people's "prewired" taste for fats and sweets (BC7),

while expressing greater skepticism of the view that paternity was no major concern in polyamorous prehistoric societies (BC9). Note that although there is strong consensus in the field denying the utility of the construct of "race," biological anthropologists are less likely to deny any corresponding biological reality to the construct (BC4). Moreover, although the difference does not reach statistical significance when controlling for other factors, a

Table 5. Distribution of survey responses by gender

Survey item	Question description	Women (% agree/disagree) ^a	Men (% agree/disagree) ^a
Science and epistemology (SE):			
SE1	Science and epistemology		
SE1	Scientific advances have improved well-being	82/1	92/2
SE2	Society better off if scientists heeded on policy	83/1	89/2
SE3	Traditional indigenous knowledges no less "true"	49/23	19/55
SE4	Science is just one way of knowing	36/42	12/73
SE5	Best theories represent reality truthfully	60/9	71/9
SE6	Depictions reflect power more than "truth"	28/35	11/55
SE7	Field is undermined by antiscientific attitudes	45/33	61/23
SE8	AAA removing reference to "science" a worthy effort	18/62	9/68
SE9	Postmodern theories made important contribution	67/18	55/27
SE10	Advocacy and fieldwork kept separate for objectivity	26/56	49/34
Moral relativism and advocacy (MR):			
MR1	Support UN Declaration of Human Rights	88/1	89/2
MR2	"Brutal" cultural practices should not be condemned	20/56	17/60
MR3	Female genital mutilation is brutal and unethical	69/12	74/7
MR4	I consider myself to be a moral relativist	30/38	27/38
MR5	Should stand in solidarity with marginalized groups	61/12	50/20
MR6	AAA should not propose resolutions, boycotts, etc.	22/62	42/43
Biology and culture (BC):			
BC1	Evolutionary "universal" explanations misguided	42/32	25/52
BC2	Evolutionary biological perspectives contribute	45/29	65/18
BC3	Social construct of "race" has no biological reality	73/18	78/13
BC4	No genetics to "racial" behavioral differences	80/8	75/8
BC5 ^b	Foraging societies in prehistory more peaceful	43/31	32/35
BC6 ^b	Tribal conflict was a principal selective force	24/41	44/28
BC7 ^b	Taste for fat and sugar prewired by evolution	56/26	72/10
BC8 ^b	Men have evolved desire for more sexual variety	23/51	37/29
BC9 ^b	Multiple sexual partners in prehistory common	45/20	38/20
BC10 ^b	Intelligence of Ashkenazi Jews genetic component	10/62	17/53
Controversial legacies (CL):			
CL1	Mead romanticizes sexual freedom of Samoans	59/8	71/8
CL2	Mead duped by Samoan informants	20/30	34/22
CL3	Freeman's criticism of Mead unconvincing	36/17	36/21
CL4	Mead ideologically motivated	41/14	54/8
CL5	Chagnon harmed the Yanomami	51/21	34/43
CL6	Chagnon ideologically motivated	59/14	39/31
CL7	Chagnon's research credible	25/36	47/28
CL8	Tierney's <i>Darkness in El Dorado</i> discredited	41/13	49/13
CL9	Stoll accurately documents Menchú	29/3	24/4
CL10	Stoll's search for misrepresentations reprehensible	16/17	9/17
CL11	Stoll's research was carried out poorly	11/2	6/7
CL12	Stoll politically motivated	20/5	16/6

Note. AAA = American Anthropological Association.

^a Percentages rounded and collapse "agree/strongly agree" and "disagree/strongly disagree."

^b BC5–BC10 percentages collapse "plausible/highly plausible" and "implausible/highly implausible."

larger minority of biological anthropologists (21% vs. 14% for the full sample) finds plausible the incendiary hypothesis by Cochran et al. (2006) of a genetic component to Ashkenazi Jewish intelligence (BC10).

Biological anthropologists' appraisals of Napoleon Chagnon represent a dramatic rift in the field. In fact, they differ significantly from their colleagues on each item. Only 17% (versus 58% of cultural anthropologists) believe that Chagnon harmed the Yanomami by acting unethically in his fieldwork, and only 20% see his research as ideologically motivated. In contrast to their archaeological and cultural colleagues, solid majorities find

Chagnon's sociobiological research credible (64%) and dismiss the major claims in Tierney's book (69%).

We suspect that the findings here would hardly surprise anthropologists. Might we attribute them simply to differences in subdisciplinary training and practice? At home with the norms of the natural sciences (and steeped in evolutionary theory), one would expect biological anthropologists to be less congenial to epistemological intricacies and more prone to support their sociobiological brethren. We do indeed contend in our discussion that subfield socialization plays a role in cultivating anthropologists' orientations to these controversial issues. Yet the

Table 6. Distribution of survey responses by political orientation

Survey item	Question description	Radicals (% agree/disagree) ^a	Liberals (% agree/disagree) ^a	Moderates (% agree/disagree) ^a
Science and epistemology (SE):				
SE1	Scientific advances have improved well-being	83/3	89/1	97/0
SE2	Society better off if scientists heeded on policy	86/0	89/2	79/9
SE3	Traditional indigenous knowledges no less true	42/17	30/45	29/44
SE4	Science is just one way of knowing	29/37	22/64	18/64
SE5	Best theories represent reality truthfully	53/12	68/10	84/0
SE6	Depictions reflect power more than “truth”	26/29	18/50	9/39
SE7	Field is undermined by antiscientific attitudes	22/56	59/25	65/12
SE8	AAA removing reference to “science” a worthy effort	31/34	10/70	6/76
SE9	Postmodern theories made important contribution	70/6	60/25	55/24
SE10	Advocacy and fieldwork kept separate for objectivity	6/69	41/43	59/24
Moral relativism and advocacy (MR):				
MR1	Support UN Declaration of Human Rights	92/0	91/0	83/6
MR2	“Brutal” cultural practices should not be condemned	14/42	17/61	17/27
MR3	Female genital mutilation is brutal and unethical	62/14	74/9	65/3
MR4	I consider myself to be a moral relativist	30/38	31/37	17/33
MR5	Should stand in solidarity with marginalized groups	78/6	54/16	32/32
MR6	AAA should not propose resolutions, boycotts, etc.	6/83	36/50	41/29
Biology and culture (BC):				
BC1	Evolutionary “universal” explanations misguided	51/23	31/54	24/41
BC2	Evolutionary biological perspectives contribute	31/43	62/21	55/12
BC3	Social construct of “race” has no biological reality	85/9	75/16	70/15
BC4	No genetics to “racial” behavioral differences	91/0	78/10	63/9
BC5 ^b	Foraging societies in prehistory more peaceful	56/15	36/35	29/50
BC6 ^b	Tribal conflict was a principal selective force	18/52	38/33	44/28
BC7 ^b	Taste for fat and sugar prewired by evolution	47/35	72/14	52/15
BC8 ^b	Men have evolved desire for more sexual variety	9/59	33/36	36/36
BC9 ^b	Multiple sexual partners in prehistory common	55/6	43/21	29/23
BC10 ^b	Intelligence of Ashkenazi Jews genetic component	6/68	14/61	19/39
Controversial legacies (CL):				
CL1	Mead romanticizes sexual freedom of Samoans	59/8	65/10	72/6
CL2	Mead duped by Samoan informants	12/35	29/25	38/22
CL3	Freeman’s criticism of Mead unconvincing	47/12	36/17	34/31
CL4	Mead ideologically motivated in Samoa	44/12	49/11	50/3
CL5	Napoleon Chagnon harmed the Yanomami	74/9	38/38	31/41
CL6	Chagnon ideologically motivated	82/0	45/27	28/31
CL7	Chagnon’s research credible	15/64	39/28	55/15
CL8	Tierney’s <i>Darkness in El Dorado</i> discredited	19/22	48/12	67/0
CL9	Stoll accurately documents Menchú	24/6	26/4	23/0
CL10	Stoll’s search for misrepresentations reprehensible	30/12	10/18	0/26
CL11	Stoll’s research was carried out poorly	10/3	10/5	0/5
CL12	Stoll politically motivated	34/0	15/7	10/5

Note. AAA = American Anthropological Association.

^a Percentages rounded and collapse “agree/strongly agree” and “disagree/strongly disagree.”

^b BC5–BC10 percentages collapse “plausible/highly plausible” and “implausible/highly implausible.”

suspicion that more is at play is confirmed by attention to gender and political identity. Our findings regarding both gender and politics surpass expectations and bolster our call for a social-psychology of knowledge production in the sciences.

Turning first to gender, we see that females diverge significantly from males on 21 of the 38 items (see table 6). They differ on every item specific to epistemology, evincing much greater receptiveness to relativism (e.g., about half affirm that indige-

nous “knowledges” regarding medicine and cosmology are no less “true” than modern scientific explanations), and they show much less inclination to separate advocacy and fieldwork for the sake of objectivity (26% vs. 49% of males). Although analogous to men on questions of advocacy and moral relativism (with the exception of the AAA’s political activities), women diverge notably in their appraisals of evolutionary biology and Chagnon’s legacy. Women are significantly less inclined to value

Table 7. Ordinary least squares regression results

Survey item	Unstandardized coefficients ^{a,b}							R ²	n
	Subfield			Gender	Political orientation				
	Archaeology	Biology	Culture	Female	Radical	Liberal	Moderate		
Science and epistemology (SE):									
SE1	.254	.416*	.084	-.095	-.007	.308	.351	.087	283
SE2	.104	.192	.060	.011	.515*	.645**	.318	.058	282
SE3	.252	-.390	.328	.852**	.401	.033	.248	.200	280
SE4	-.264	-.960**	.030	.836**	.526	.181	.295	.233	283
SE5	-.075	.287	-.074	-.212*	-.017	.158	.467	.056	276
SE6	.318	.310	-.027	.371**	.627*	.180	.254	.094	275
SE7	.400	1.095**	-.303	-.398**	-.492	.074	.130	.241	282
SE8	-.889**	-1.272**	-.387	.238*	.723*	-.026	-.016	.223	282
SE9	-.293	-.968**	.007	.296*	.748*	.535*	.554	.135	282
SE10	.441	.676**	-.090	-.539**	-1.408**	-.695*	-.267	.209	286
Moral relativism and advocacy (MR):									
MR1	.331*	.159	.175	.019	.596**	.546**	.199	.069	294
MR2	.043	.058	-.002	.118	-.155	-.413	-.336	.014	294
MR3	.208	.153	-.292	-.118	.149	.202	.175	.063	294
MR4	-.210	-.214	-.573**	.015	.555*	.434	.368	.046	295
MR5	.059	-.021	.277	.164	.845**	.385	-.023	.085	284
MR6	.297	.556*	.105	-.483**	-1.174**	-.279	-.040	.145	288
Biology and culture (BC):									
BC1	-.267	-.841**	.162	.542**	.629*	.144	.294	.163	278
BC2	.103	.591*	-.365	-.452**	-.460	-.011	-.199	.162	276
BC3	-.390	-.814**	-.226	-.178	.441	.295	-.054	.070	276
BC4	-.332	-.286	.078	.138	.583*	.192	-.140	.087	275
BC5	-.209	-.353	.120	.111	.835*	.325	-.009	.080	275
BC6	-.057	.130	-.215	-.455**	-.510	-.111	-.203	.080	273
BC7	.210	.661**	.122	-.417**	-.455	.024	-.452	.128	270
BC8	-.126	.327	-.133	-.518**	-.940**	-.307	-.366	.135	269
BC9	-.363	-.620**	-.299	.104	.830**	.614*	.495	.065	263
BC10	-.089	.008	-.396	-.209*	-1.038**	-.912**	-.530	.097	268
Controversial legacies (CL):									
CL1	-.076	.006	.008	-.136	-.155	-.161	-.077	.014	255
CL2	.394*	.378	.060	-.200*	-.724**	-.478*	-.360	.078	256
CL3	-.346	-.316	.040	-.037	.967**	.789**	.636*	.071	256
CL4	-.004	.042	.250	-.195*	-.326	-.180	-.191	.044	259
CL5	-.296	-.998**	.056	.565**	.960**	.347	.398	.214	258
CL6	.096	-.493*	.322	.431**	1.165**	.411	.272	.207	258
CL7	.429	.970**	.053	-.340**	-1.219**	-.533*	-.217	.238	257
CL8	.247	.822**	.047	-.221*	-.049	.322	.707*	.175	247
CL9	-.176	-.172	-.026	.013	-.130	-.026	-.002	.019	214
CL10	.024	-.043	.120	.070	.389	.083	-.095	.059	213
CL11	.042	-.017	-.062	.081	.128	.103	-.027	.029	207
CL12	-.294*	-.218	-.274*	.053	.258	-.033	-.027	.045	211

^a Intercept terms are not shown.

^b Significance tests are one-tailed.

** $P < .01$.

* $P < .05$.

evolutionary biological perspectives and much less receptive to accounts of a conflictual human nature or alleged male propensity for sexual variety. Finally, majorities of women affirm that Chagnon was ideologically motivated in his research (59%) and that he unethically harmed the Yanomami (51%).

Plainly the field appears split into gendered groupings as well, yet it turns out that political identity is comparably significant. As indicated in table 1, anthropologists are overwhelmingly liberal, with radicals and moderates collectively making up only

25% of our sample. Conservatives and libertarians constitute a mere 2% of respondents—confirming Starn's (2015) observation that one is more likely to encounter "an anteater in a shopping mall" than "a Republican anthropologist" (14).⁹ Reviewing table 6, we see that radicals differ significantly from their conservative colleagues on 20 survey items (while liberals diverge on nine

9. It is due to their sparse representation that we combine conservatives and libertarians as the reference group in the political orientation models.

and moderates on only two). Similar to females, radicals show more sympathy for epistemological relativism, reporting stronger support for postmodern theories, greater receptiveness to removing “science” from the AAA’s long-range plan, and virtually no interest in separating advocacy and fieldwork to protect objectivity. Radicals are significantly more likely to call for solidarity with the marginalized groups that they study, and they overwhelmingly reject the view that the AAA should not engage in politically oriented activities.

With regard to biology, radicals again echo women in being significantly more likely to reject evolutionary explanations of “universal” features of human cultures (indeed, a slight majority finds such attempts “misguided”). In pronounced contrast to their colleagues, radicals decidedly reject the evolutionary psychological chestnut of a male instinct for sexual variety (59%), with a majority (55%) endorsing the plausibility of the view—popularized by Ryan and Jethá (2010)—that polyamory characterized prehistoric societies with little concern for paternity. Finally, radicals were significantly more likely than their colleagues to reject the plausibility of a genetic component to Ashkenazi intelligence.

Regarding the controversies, both radicals (and to a lesser extent, liberals) were significantly more inclined to discard Freeman’s (1983) attacks on Mead and deny his allegation that she was duped by her Samoan informants. As to Chagnon, radicals overwhelmingly affirm (82%) that he was ideologically motivated to present a “bellicose” human nature (with zero percent disagreement). Seventy-four percent believe that he unethically harmed the Yanomami, and only 15% find credible his sociobiological interpretation of the unokai. Note that each of these items is statistically significant, vividly illustrating the pattern of differences among subfield, gender, and political groupings.

Space does not permit us to comment on all items. But we encourage readers to examine the results closely and note that even when items do not reach statistical significance, again and again we see a consistent direction of responses by gender and political orientation. To provide an explanation of these provocative findings, we will venture cautiously into the thorny terrain of political psychology.

Discussion

Anthropology’s Emotive Communities: A Social Intuitionist Interpretation

In a recent commentary, Glynn Custred (2016) applauds Chagnon’s work while condemning what he sees as a radical “faction” within anthropology, one that aims to transform the field into a “politically correct university outpost.” Revitalizing the “noble savage” myth, Custred decries, activists hope “to sweep down the memory hole” research such as Chagnon’s that reveals a deep history of human warfare. Regrettably, rather than being open to “biological links” to human behavior, “politicized anthropology” views any such link as “a heresy that must be extirpated.”

In sharp contrast, Ryan and Jethá (2010) condemn Chagnon for “swashbuckling into Yanomami lands,” “inflaming” violence wherever he turns, and “seducing” readers with “tales of derring-do among the vicious and violent ‘savages’” (223–227). Our “bullshit detectors go off,” they declare—reproaching both Chagnon and Steven Pinker (2011)—when scholars point to “violent chimps and a few cherry-picked horticultural societies” as evidence of “ancient tendencies toward warfare” (Ryan and Jethá 2010:223–227).

Our findings in this study suggest that readers’ appraisals of the above remarks likely reflect more than their knowledge of human prehistory. Indeed, we suspect that for some their attraction or aversion to Custred’s or Ryan and Jethá’s words implicate their professional and personal identities and underlying emotional sensibilities. Consider our respondents’¹⁰ opposing views regarding the credibility of Chagnon’s work on the reproductive success of Yanomami “killers”:

- Studies by many anthropologists have shown that his premises and use of statistics were very flawed.
- This is credible as this has been found in other human societies so has been replicated.
- This is one of the silliest arguments in anthropology. The simple demographic fact is that “if you are killed whether by male or female violence you do not contribute to the ongoing reproductive success of any group. End of argument” (respondent’s quotes).
- Ridiculous question—missing the point—get with the current concerns of indigenous peoples.

We received many such reactions *pro et contra* to most of our survey items. This may be expected when examining sensitive questions that have long vexed a field. Yet our aim here is not so much to take sides in the controversies as to map what we see as their underlying emotive dynamics. As we stress, issues that push social scientists’ emotional buttons are most likely to undermine empirical consensus. Even the facts surrounding the 2010 AAA mission flap prove contentious. Recall that we ask anthropologists whether the AAA’s proposal to remove reference to “science” from its long-range plan was a “worthy effort” (SE8). The event, however, hardly spurs consensus regarding its basic details, much less its implications for the discipline:

- The most important issue in anthropology today is the 2010 revision of the AAA representation of anthropology. It removed science and evolution from its representation of the discipline.
- It was horrible. It wounded us to such an extent that we are still recuperating from it. What a mistake it was.
- It was and apparently is widely misunderstood. It was not a major statement on the role of science but an attempt by

10. We were delighted to receive more than 1,000 remarks by anthropologists in the comment boxes across the 38 survey items.

some to overly nuance the balance between what some think of science (as pure) and others as more relative.

- This was a random clerical error that signaled nothing about the AAA's actual position.
- It was an oversight, a slip.

The "most important" issue in anthropology? A field still "recuperating?" A "misunderstood" attempt for "nuance?" A "clerical error?"

Such disparate interpretations show how even a single event—should it tap the nerve of an academic community—may elicit all manner of (re)constructions. Indeed, we suspect that constructions of "science" in this quarrel serve as a proxy for anthropologists' respective group identities and moral emotions. To sketch what we see as the emotive dynamics of anthropology's ongoing divisions, we draw tentatively from current research in political psychology. We find Jonathan Haidt and colleagues' work on "social intuitionism," elaborated in "moral foundations theory" (MFT), to be especially helpful in this regard (Haidt 2001, 2012; Haidt and Graham 2006; Haidt and Joseph 2004, 2007).

The literature on moral foundations is vast and growing and is beyond our space here to unpack in detail (see Graham et al. 2011 for an overview). As far as we know, however, our studies are distinctive in applying MFT to the contours of social scientific debates (Horowitz and Hughes 2017; Yaworsky, Horowitz, and Kickham 2015). Our basic argument is that scholars who share the same underlying moral emotions (or intuitions) tend to gravitate (consciously or otherwise) toward interpretive and emotive communities. Such communities often draw from their disciplinary repertoires the same recipe of methods and tools and interpret evidence in kindred ways. This occasions in-group/out-group dynamics both within their fields and without. This is not to deny, of course, the centrality of the empirical in adjudicating social scientific debates. Yet when it comes to morally charged matters, such as the controversies raised in our survey, contrasting intergroup intuitions is more likely to be activated.

As an intuitive model of moral reasoning, MFT stresses that people make judgments based on unconscious "flashes of approval or disapproval" toward morally charged affairs (Haidt and Joseph 2004:56). Conscious deliberation comes afterward, often serving to rationalize people's automatic (gut) reactions. Such automatic responses depend on people's particular mix of five moral foundations—specifically, their underlying intuitions regarding care, fairness, authority, loyalty, and sanctity.¹¹ Haidt and colleagues find that people on the left-liberal end of the political spectrum have a narrower moral template than

those on the right. Specifically, the twin foundations of care and fairness saturate the political morality of the left. The template of the political right, however, is broader and includes all five intuitions.¹² Hence care and fairness concerns are not the only triggers on the right. Right-leaning people tend much more than those on the left to respect authority, value group loyalty, and affirm purity with regard to conventional moral norms. While the left is spring-loaded to challenge authority on behalf of the vulnerable, the right honors hierarchical relationships and feels more affinity to equity and tough-love sensibilities than the egalitarian protectiveness of the left.

MFT views moral intuitions as inherited features of the brain that are both innate and modular, although revised by experience. The theory dovetails with a plethora of experimental and psychological literature confirming the biological roots of personality and consequent political orientation (e.g., Block and Block 2006; Hibbing, Alford, and Smith 2013; Jost 2009; Mooney 2012, 2014). As Mooney (2014) puts it, referring to a wave of new scientific evidence, "Long before they become members of different parties, liberals and conservatives appear to start out as different people." Yet neither personality nor politics are determined narrowly by the genes. People rather are born "prewired" with certain characterological dispositions that Haidt (drawing on Marcus 2004) calls a "first draft." Life experiences in interaction with the genes rewrite future drafts in ways that enable innateness to coexist with wide developmental malleability (Haidt and Joseph 2007).

Engaging the complexities of debate on the biocultural underpinnings of personality and politics would bring us far afield. Yet we have in mind the following speculative scenario with regard to aspiring anthropologists. Their liberal-minded sensibilities no doubt nudged them toward careers in academia—after all, it is widely observed that liberals far outnumber conservatives in university positions (Gross 2013). This is dramatically so in cultural anthropology, where the ratio reportedly stands at about 30:1 (Gross and Simmons 2007). Once ensconced in their graduate school subfields, they receive training in the corresponding frameworks, conceptual tools, and practices required to obtain their degrees. Such practices often nurture disciplinary identity and a privileging of one's academic turf, especially in light of the investment expended to master its techniques. Budding anthropologists are no doubt socialized beyond formal techniques, however, to underlying scientific, epistemological, and ethical assumptions. Indeed, we suspect that the breadth of subdisciplinary socialization plays a role in the marked contrast in responses we discovered between biological anthropologists and their (especially sociocultural) colleagues.

Yet people are not merely vessels of their professional enculturation. Carrying different mixes of moral intuitions and

11. MFT holds that these intuitions have deep evolutionary roots, meeting adaptive challenges in prehistory (e.g., reacting to human suffering with care helps ensure kin survival; feeling group loyalty and punishing traitors helps ensure advantage in the context of intergroup competition for resources; etc.) See Haidt and Graham (2006) for discussion of their evolutionary argument.

12. On their web page, <https://www.yourmorals.org/>, Haidt and colleagues suggest a candidate for a sixth moral foundation as well: "liberty," but this intuition has not been as thoroughly developed or consistently applied.

experiences with them (and consequently varied political identities), we would expect—if MFT is correct regarding the role of emotions in people’s judgments—that liberals and conservatives independent of subfield would differ in their interpretations of morally charged evidence. Plainly, this is precisely what we have discovered in our survey. Although we note above that there is scarcely a conservative among anthropology’s ranks, we do see that radicals (and to a lesser extent, liberals) differ significantly from their colleagues on questions of epistemology, advocacy, and the merits of evolutionary explanations. Women follow suit in this regard. We should stress as well that in contrast to radicals, liberals, and women, moderates diverge significantly from their outlying conservative colleagues in only two of the 38 survey items. This pattern suggests that moral intuitions may indeed play a role in the field’s disparate appraisals of controversial evidence.

Given a field predominated by left-liberal sensibilities, how might intuitionism help explain why political identity and gender are such reliable predictors of respondents’ varying standpoints? The question of advocacy presents perhaps the least difficulty. For MFT, the left’s acute sensitivity to care and fairness suggests that actions or policies perceived to harm or cheat the vulnerable are especially likely to trigger their moral emotions. It is not surprising, hence, that left-wing scholars who come together in emotive communities would reproduce a discourse of moral responsibility toward subaltern groups. This is only intensified in cultural anthropology, where the ramifications of colonialism loom so large in the subject matter. From this vantage point, we would anticipate much sympathy for Schepher-Hughes’s call to her colleagues to stand in solidarity with the marginalized groups they study (D’Andrade 1995; Schepher-Hughes 1995). (Indeed, 78% of radicals in our survey concur with the charge vs. 32% of moderates; table 5, MR5.) For Schepher-Hughes, the legacies of colonialism—and the brutal conditions that normalize, for example, maternal complacency toward infant death—render the sacred cow of political disengagement an utter abdication of ethical responsibility.

Of course the conventional critique of such engagement, classically formulated by Weber (2009 [1917]), is that value commitments distort the pursuit of truth: “When the man [*sic*] of science introduces his personal value judgement, a full understanding of the facts ceases.”¹³ In his debate with Schepher-Hughes, D’Andrade (1995:408) echoes Weber by arguing that “moral” and “objective” models cannot be effectively wed in science. “It’s nice to believe one can have both,” he notes, “but the evidence is strong that one cannot” (408).

D’Andrade poses a stark choice: either we pursue objective models of the world or we embrace moral models at the expense of such objectivity. Yet radical scholars have long challenged this premise. Might we be, as Engels wrote long ago,

13. Weber’s position is actually a bit more subtle, unsurprisingly, in that he argues that chosen scientific questions themselves have moral “pre-suppositions,” such as their worthiness of being known. Yet he plainly endorses a dispassionate orientation to research and teaching in practice.

utopian and scientific? Many of our respondents believe so—one noting what she sees as a “false polarity” between advocacy and objectivity, another opining: “I don’t see that they need to be separate,” and so on. MFT, however, aligns closely with Weber/D’Andrade. Not only are people typically unaware of the moral intuitions coloring their judgments but also their emotive communities tend to reinforce their biases in groupish ways. “Morality binds and blinds,” Haidt (2012:218) writes, in what he views as a foundational principle of moral psychology.

For Haidt (2012), the blind spots of left academics can be seen most clearly in their construction of “sacralized victims” (345). Once a group is construed as sacred—be they ethnic, religious, or other minorities, transgendered people, and so forth—it becomes much less likely to assess their circumstances accurately. That is, whatever the facts may be, the left is trigger-ready to interpret them in ways that avert potential harm to perceived victims. It is in this context that we might understand the relative embrace of epistemological relativism by female and radical anthropologists. Consider item SE3: “On the whole, traditional indigenous knowledges regarding medicine or cosmology are no less ‘true’ than modern scientific explanations.” Virtually half (49%) of the women in our sample agree with this, differing significantly from the rest of the field. Forty-two percent of radicals concur as well. In this case, knowledge of the disparate treatment afforded to native peoples over the centuries may act as powerful “primers,” nudging them into vocalizing support for a proposition that places indigenous culture on equal footing with Western society. Yet is it the case that indigenous understandings of medicine or cosmology are no less accurate than modern scientific accounts? No progress has been made by Western science in these arenas that has surpassed indigenous knowledge? Although we are reluctant to take sides in this study—we aim to understand ideology and not merely critique it—this extraordinary claim seems uninterpretable without the tools of intuitionist social psychology.

Females and radicals are significantly more likely than their colleagues to agree with item SE6 as well: “Anthropologists’ depictions of indigenous cultures reflect more their own power and interests as knowledge producers than ‘true’ descriptions of the reality of those cultures.” Notice the scare quotes and essential doubt regarding any scientific approximations of “truth” in both of these survey items. Recall, however, that anthropologists (including women and radicals) overwhelmingly affirm that scientific advances have improved human well-being over the years (SE1) and that society would be better off by heeding the consensus views of scientists in public policy (SE2). Respondents again and again comment unprompted about climate change in this regard, acknowledging that scientific practice, despite fits and starts, produces accurate knowledge over time.

It would be easy to call out our anthropology colleagues for contradictory views here. How can science make long-term advances that benefit society with knowledge claims regarding medicine or cosmology as relative or timeless as indigenous folklore? Perhaps sensing their epistemological ambivalence,

respondents stress contextual considerations in endorsing scientific consensus:

- Climate change today, yes; cigarette smoking in the 1950s, no.
- Some policy areas are more firmly graspable via scientific methods than others. Climate change: yes. Easing of income inequality: not so much. Eradication of racial discrimination: probably not at all.
- Pharmaceutical company-sponsored randomized control trials? When it comes to climate change yes, but tell a woman that mammography actually does not produce the results they imagine.
- Science is particularly good at some types of truthiness. But there are other approaches to knowledge that are more valid when it comes to science's blind spots.

We fully concur that science is all too often distorted by corporate and political forces that fund and harness it around their interests. Yet we wonder if statements like these conflate the method of science with its socially corrupting embeddedness in wider fields of societal power and interests. We agree that coming to consensus on social scientific questions of inequality or racial discrimination is much more difficult due to broader power relations and entrenched ideological divisions (the underpinnings of which we have only tentatively sketched in this report). Yet to bend the stick so far as the respondent does above—that science is good at “some types of truthiness” but that there are other “more valid” approaches to knowledge—strikes us as wholly unconvincing without identifying the epistemological grounds for consenting to such approaches.

The question of alternative epistemology may jog the reader's memory of our discussion of the Menchú/Stoll controversy described above. Recall that Stoll was widely condemned for uncovering misrepresentations in Menchú's testimonio. The reactions were surely understandable at the time. In the context of a brutal civil war, where hundreds of thousands perished, Menchú and her family suffered grievously but Stoll not so. It is hardly surprising in this setting that the progressive community would close ranks around Menchú—a “sacred” symbol of peasant resistance—and attempt to “purge” ideas that threatened to sully her reputation. Indeed, Stoll's attempt at an “authoritative” account of Menchú's life was all the more galling to progressive sensibilities given his status as a privileged Western observer.¹⁴

In MFT terms, the suddenly in-vogue descriptions of testimonio as a collective enterprise of “multiple positionalities,” or “ways of knowing,” may be seen as ad hoc rationalizations of the fictitious elements in Menchú's story (recounted in Burgos-Debray 1983). As part of the left-liberal camp ourselves, we should stress that we sympathize deeply with the collective

anguish that likely spurred such rationalizations. We appreciate as well the respondent who grants that Menchú's exaggerations were a “problem” but that they should be seen “as reflecting the limits of reaching a public that had ignored the genocide against Maya villages then going on in Guatemala.” We by no means believe that there is a pat answer regarding how and when social scientific or other knowledge should be divulged, should such disclosures have predictably injurious consequences in dire historical circumstances. Yet we find it critical not to let our piqued sensitivities “bind and blind” us, as Haidt would put it, into abandoning the scientific pursuit of truth. “No matter how sympathetic a figure is,” one respondent notes, “it is important to report the facts, and not let sympathy lead researchers into rhetorical poses that romanticize their subjects.”

Arguably the most famous critic of the romanticization of the indigenous in recent years is Steven Pinker (2011, 2016). Pinker assails the Rousseauian doctrine of the noble savage, highlighting wide-ranging literature in evolutionary biology, anthropology, and paleontology on the prehistoric roots of violence and war. The claim has proved quite divisive in social science today (as has Pinker himself) and unsurprisingly has polarized our survey results. Once again we see females and radicals in our sample standing apart from their colleagues. They are least receptive to the claim that tribal conflict was “a principal selective force in human nature” (BC6) and most receptive to the idea that prehistoric foraging societies were sexually permissive (BC9) and “more peaceful than later agricultural or industrial societies” (BC5). As noted above, females and radicals are less inclined to welcome evolutionary explanations of human cultures, with many rejecting widely endorsed propositions in evolutionary psychology. Indeed, a majority of radical anthropologists do not find plausible even the uncontroversial view that natural selection shaped humans' taste for foods with fat and sugar (BC7). It is hardly surprising then that a mere 9% would find plausible the view of an evolved male impulse for greater sexual variety than women (BC8).

We have reflected elsewhere on the likely emotive underpinnings of left academics' resistance to evolutionary psychology (Horowitz, Yaworsky, and Kickham 2014; see also Jonason and Schmitt 2016). Suffice to note that although claims regarding evolved, adaptive roots of current human behaviors cannot be tested experimentally—inviting their widespread dismissals by detractors as “just-so” stories (e.g., Gould 1997; Kimmel 2004)—from a moral foundations perspective, more is going on than simply intellectual disagreement about the credibility of available evidence. It appears that acknowledging an instinctual component of darker aspects of human nature (e.g., intergroup violence, sexual possessiveness) runs headlong into the left's intergroup identities and underlying collective emotions. As committed egalitarians, attuned deeply to the intuitions of care and fairness, ideas that appear to them to naturalize hierarchy or reward violence in any way trigger mechanical dismissal. The comparatively frosty appraisals of Chagnon's legacy by radicals (and to a lesser extent, liberals) can certainly be interpreted in this light.

14. Although we did not have enough respondents to gauge statistical significance, consistent with expectations we found that radical anthropologists were more likely to find Stoll's research “reprehensible.”

Although the gendered differences in responses to our questions were significant and widespread, to date, MFT has little to say on gender. We find this puzzling. A wide-ranging study (Graham et al. 2011) finds that women reveal significantly greater sensitivity than men to care and harm concerns when rendering moral judgments.¹⁵ Yet theorizing the reasons why lags in MFT. Some cognitive scientists (Baron-Cohen 2009; see also Christov-Moore et al. 2014) argue controversially that average differences by gender in the experience of empathy and compassion are in part innate and have evolutionary biological roots. This view need not contradict traditional social scientific emphases on cultural factors and socialization, even among sociobiologists. Hrdy (1999), for example, views female nurturing strategies, even those directed toward their own offspring, as calculated, context dependent, and subject to the forces of culture. Whatever its biological, cultural, or interactive roots, a keener intuitive sensitivity to care and harm among women may help explain the distinct gendered results in our survey.

Unsurprisingly, radicals were significantly less likely to entertain the most delicate items in our survey bearing on the potential reality of “race.” As discussed above, the field overwhelmingly denies a biological reality to race (BC3) and believes that “attempts to discover a genetic component to behavioral differences between ‘racial’ groups are bound to fail” (BC4). Anthropologists are deeply skeptical as well of a genetic component of the comparatively high IQ scores of Ashkenazi Jews (BC10). We hardly wish to pull the tiger’s tail in our final thoughts, nor do we endorse the view of biological races.¹⁶ However, notwithstanding the complexity and sensitivity of these issues, we wonder about the extent to which moral intuitions and disciplinary identities may render off limits even measured inquiry into these matters.¹⁷ The “racialist” arguments made by science writer Nicholas Wade (2015), for example, have struck a deep nerve as evidenced by the joint letter by 143 academicians rebuking his book (Coop et al. 2014). We do not have the space to elaborate further on the matter. We should note, however, that there is less consensus among anthropologists globally on

15. Graham et al.’s (2011) international database of 49,428 women and 68,812 men found that women were significantly more concerned than men about harm, fairness, and purity, while men evinced slightly higher concern for in-group loyalty and authority.

16. We concur with the anthropological consensus, going back most prominently to Boas (1912), discarding the notion of races as fixed or natural categories. Clinal studies support this conclusion, as do most contemporary anthropologists (see Wagner et al. 2016). Of course, scientists who favor the concept often point to its utility in forensic analysis (Gill 2000). They also highlight continental “clusters” of traits that for “racial realists” matter as much as clines (Wade 2015).

17. The following are only a handful of the responses revealing anthropologists’ widespread aversion to these questions: “As an Ashkenazi Jew, I really hate this argument,” “This is a racist question,” and “It would take pages to explain my objections to this question.” We should add that there were a comparable number of respondents who suggested that the research is not available or settled on the matter.

the disutility of the race concept than there is in the United States and Western Europe (see Štrkalj 2007).

Concluding Remarks

Some two decades after the science wars, we have aimed in this study to take stock of the field of anthropology. Although we have been loath to take sides in the controversies discussed, we hardly see ourselves on an enlightened perch above the fray of collective dispute. In fact, a chief limitation of this kind of study is that we do not carry out the hard work of engaging these contentious issues empirically. Although we appear to upbraid our fellow left-liberal colleagues for potential blind spots, by no means do we view their respective positions on these controversies as necessarily wrong. Indeed, we believe that the empirical chips should fall where they may—something that we hope is more likely to the extent that academics across the political spectrum recognize the tugs of their own moral intuitions and intergroup identities when gauging evidence. This can only aid in the pursuit of truth and consequent scientific consensus, to which we remain wholly committed. In today’s highly polarized political climate, a better grasp of the underpinnings of ideology—both inside and outside of academia—is sorely needed. We hope our report inspires future inquiry of this nature.

Comments

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Horowitz, Yaworsky, and Kickham (HYK) confirm that almost all anthropologists position themselves from the center to the left end of the political spectrum. Among anthropologists they find significant differences by gender, political orientation, and subdisciplinary specialization in attitudes toward science, epistemology, advocacy, and “evolutionary biological explanations,” and toward recent controversies that involve those broader issues. Their findings are interesting, although not surprising, and it is good to have some data.

HYK follow moral foundations theory (MFT), which proposes specific cognitive orientations that are “innate and modular, although revised by experience,” differentiating liberals from conservatives. Much work is ongoing about the evolution of human moral sensibility, and about neurobiological substrates of political orientations, from many diverse perspectives. MFT aligns with evolutionary psychology, an approach that has been critiqued for its concept of massive modularity, its approach to evolutionary process, and its lack of support from genetics, neurobiology, and cognitive science. Criticism has been directed

at MFT specifically (Suhler and Churchland 2011), of course with rebuttal. So Haidt and Joseph (2011) respond to Suhler and Churchland that they “have always treated moral modules as *functional* modules, not as physical, anatomical or neurobiological modules” (2119). I am generally skeptical about innate modularity of complex cognition and approaches based on that premise. But I do not understand how this theoretical orientation even applies to the data on hand? It is not about liberals versus conservatives but rather self-identified moderates, liberals, and radicals, plus gender and disciplinary specialization.

HYK elide attitudes about evolutionary or biological approaches to culture and behavior, with being pro- or antisocial. No anthropologist is skeptical about evolutionary approaches to evolution. The skepticism—I think—is about sociobiology, evolutionary psychology, and the simplistic biologisms so common today. Genes widely publicized as affecting abusive behavior (MAOA) and depression (5-HTTLPR) were later found to be, in those applications, nonexplanatory (Haberstick et al. 2014:25; Risch et al. 2009). Oxytocin, the famous trust hormone, in meta-analysis is not reliably associated with trust (Nave 2015:772). Even for the language gene, FOXP2, its supposed sweep of human populations was just found to be an inaccurate result of the earlier sample. “New hypothesis for language evolution needed!” (Atkinson et al. 2018). The list is easily extended. Many claimed biological breakthroughs about human behavior have been walked back.

Single genes are not the same as the evolved cognitive modules. Even if conceptualized as predispositions to learn rather than hardwiring, those are much more complex multigenic traits that are far less subject to confirmation or investigation by contemporary physical sciences. Skepticism about them as evolved adaptations is not antisocial but is rather consistent with much research on neurobiological developmental plasticity, systemically interacting with environment at many levels.

HYK frame opinions about recent anthropological controversies as differentiating commitment to science, from “post-modern sentiments [that] endure in the discipline, undermining scientists’ commitment to objectivity and capacity for dispassionate approximations of ‘truth.’” Women and leftists situate themselves in moral communities that may “bind and blind” them into “abandoning the scientific pursuit of truth.” “It appears that acknowledging an instinctual component of darker aspects of human nature (e.g., intergroup violence, sexual possessiveness) runs headlong into the left’s intergroup identities and underlying collective emotions.”

Sexual possessiveness falls within a larger discussion of gender differences. I would hazard, without statistics, that female anthropologists have greater interest in that topic than males. For those who study this area, there are sober, scientific, bio-social (beginning with the body) alternatives to evolved predispositions for all manner of gender specializations, including cross-culturally common male efforts to control female sexuality (Eagly and Wood 2003). There may be greater awareness of excesses of proclaimed evolutionary science, such as men having an evolved psychological module for rape (Travis 2003). They may know

better that sociobiology was employed from the get-go to undermine feminist objectives (Gould 1978). They are probably more attuned to the androcentrism of Chagnon’s portrayal of Yanomami. These are all good reasons for skepticism.

Skepticism of “tribal conflict” as “a principal selective force that shaped human nature,” or the “instinctual component of . . . intergroup violence” is not antisocial, although it is regularly tarred as political correctness. Dispassionate investigation from phylogeny (Gomez et al. 2016), archaeology (Ferguson 2013a, 2013b), and ethnology (Fry and Soderberg 2014) soundly refutes the evolutionary position of ubiquitous war through our species’ past. (On Chagnon’s role in fomenting conflict among Yanomami, and the validity of his claims of higher reproductive success, see Ferguson 2015b; on the scientific status of Cochran et al. [2006] on Ashkenazi intelligence, see Ferguson, unpublished manuscript, 2008).

HYK do not evaluate the merits of particular positions not relevant to their study. Certainly most anthropologists do not know the details of controversies. General reactions are shaped by their larger orientations, as HYK propose. The troubling subtext is how these findings will be read and cited.

The takeaway for many will be that criticism of such biological or evolutionary positions by anthropologists is political, not scientific. That is the standard refrain of sociobiologists and evolutionary psychologists. This article facilitates a political defense of positions that are challenged on empirical and theoretical grounds. Informed skepticism about general research orientations and rigorous criticism about particular findings based on those orientations is not antisocial, but practicing science.

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We wish to thank the authors for their thought-provoking article. Whether considering the researchers’ assessments of specific controversies or the more general items of the survey, we think that the main takeaway message is the need for serious self-reflection within our discipline. In seeing these findings as a call for self-contemplation by anthropologists, we are not advocating the return to the ’90s style of book forewords that divulged information about authors, but rather we are calling for serious self-reflection about assumptions, cultural beliefs, theory, training, and disciplinary history and how such factors influence the thinking and practice of anthropological science and scholarship.

One way to deal with biases is to regularly critique one’s own assumptions, motives, methods, and interpretations before manuscripts even go out for peer review. Clearly, a host of personal features, cultural beliefs, and theoretical biases can creep into research and interpretive processes. It could be useful if

anthropologists applied their knowledge about the learning and transmission of shared beliefs to their own academic thinking and writing. In considering the differences regarding respondents' subdisciplines, genders, and political orientations, Horowitz, Yaworsky, and Kickham offer an interpretation based on how scholars who share underlying moral emotions "tend to gravitate (consciously or unconsciously) toward interpretive and emotive communities." This may be true, but we wonder whether it might be useful to conceptualize the survey results as reflecting variations of belief systems, or different academic milieux, depending on subfield, gender, or political views, that is, to apply anthropological constructs to anthropologists themselves.

The type of self-reflective questions we think could be useful for a scientist or scholar include: Do I really have hard data to back up my interpretations? Are my methodological decisions as unbiased as possible? What would a skeptic say about my techniques and interpretations that would be valid points to consider?

The survey findings show anthropologists to be split on the item about peacefulness in prehistory prior to the development of agriculture. This is an area that we think could benefit from greater attention by researchers to the history and philosophy of science; a careful scrutiny of sampling, methods, and interpretations; a holistic consideration of multiple data types (e.g., Holocene archaeological sequences on the origins of war, social complexity, ecology and demography, nomadic forager social organization, the ethology of mammalian aggression and restraint, and so on); and last but not least, honest self-reflection—to the greatest degree possible—about one's own biases, motivations, and assumptions. If the goal in science is the pursuit of truth and understanding, then publications that draw upon cherry-picked samples, misrepresent the nature of the data, muddle key concepts, or ignore contradictory findings harm the practice of science and the field of anthropology (see Fry 2013, 2018; Fry and Söderberg 2014).

Horowitz, Yaworsky, and Kickham also report that anthropologists remain divided regarding what to make of Napoleon Chagnon's (1988) claim that Yanomamö killers have more offspring. In his memoirs, Chagnon (2013) criticizes his anthropological colleagues for being biased and unscientific, but he does not engage in the type of self-reflections that could benefit the practice of science. Chagnon (2013) simply republishes the original killers-have-more-kids findings without any serious engagement with the methodological, mathematical, and interpretive critiques of his original study (e.g., Albert 1989; Ferguson 1989, 1995; Fry 2006; Lizot and Dart 1994; Miklikowska and Fry 2012; Moore 1990) and without any mention of studies on the Waorani and the Cheyenne that report the opposite of his Yanomamö findings—namely, that killers leave *fewer* offspring than nonkillers (Beckerman et al. 2009; Moore 1990). Chagnon (2013) shows no willingness to rethink his original methods or interpretations in light of various kinds of critiques and subsequent findings reported over a 25-year period. Whereas closed-mindedness is antithetical to good science, the striving by researchers for high standards, self-awareness, and con-

sciousness about their own motivations and biases can aid the scientific endeavor.

Charles Darwin, as he contemplated and gathered various kinds of data in support of his theory of evolution through natural selection, did engage in self-critique and self-reflection. Darwin ([1887] 1958) insightfully realized his own tendency to dismiss observations that seemed to be unsupportive of his theorizing, writing in his autobiography, "I had, also, during many years, followed a golden rule, namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results, to make a memorandum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favorable ones" (123). We think anthropologists could learn from Darwin's mindful and meticulous approach. Unfortunately, today one does not have to look very far to find examples of writers selectively including only material that matches their arguments or that corresponds with their pet speculations, while simultaneously ignoring or even misrepresenting findings that contradict their views.

Horowitz, Yaworsky, and Kickham point out that according to moral foundations theory, people are typically unaware of the moral intuitions affecting their judgment. While not a panacea, we suggest that researchers can regularly ask themselves self-reflective questions and in so doing become less biased: What are my personal biases (political, cultural, theoretical, and so on)? Why do I have these biases? How did I get them? What implicit assumptions do I hold on the topics I study? How does the history of my subdiscipline and my training milieu in particular affect my thinking? What motivates me to do my science or scholarship? In my work, how can I use the most rigorous scholarship and methods? Am I true to seeking the truth, or do other factors influence my thinking and motivations (fame, glory, winning a debate, etc.)? How open am I to changing my mind when presented with new data or new interpretations that better fit the facts?

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Academia, like most communities that are conscious of being a community, seldom enjoys being the subject of an analytical critique that reveals tacit bias and unvoiced, albeit often acted upon, political positions. This is especially so when that community frequently claims it is a society of concerned thinkers interested in the discovery of truth. But what is truth, Pontius Pilate asked Jesus?

In this thoughtful overview, the authors provide us with a road map of tacit predispositions that many academics bring to the analysis of their own data as well as those of others. In the resulting intramural scrimmage, we are asked to once again revisit Max Weber's masterful analysis of the role of objectivity

in the social sciences. Roy D'Andrade in his 1995 *Current Anthropology* article sought to prod the field into reengaging with Weber's firm conviction that objective analysis can exist within a moral universe. His call for an open dialogue was overwhelmingly greeted with the reassertion that the best most "objective" analysis acts as a moral defense of the people we study. The implication is that because truth is socially constructed, we should focus on what we deem to be moral truths, which are objective enough.

Given that Donald Trump is the first postmodern president to not only embrace the relativity of truth but also advance the subjectivity of truth, that is, whatever he wants it to be, endorsed by a recent Trump spokesperson who informed us that the "truth isn't truth," (Moore 2018) it should come as a refreshing reminder that when taken to extremes, such a slippery view renders impossible any objective study to identify behavioral patterns and the reasons for their appearance, continuation, and disappearance. In an increasingly insecure culture where the nature of truth is up for grabs, we as a discipline may be devolving into little more than some kind of association grounded in the ethics of social justice and defined by the instability of our often-shifting core values and personal preferences.

Here the authors are spot-on in recognizing that we have formed competing epistemological tribes that talk past each other—if we can assume that another person is even listening. We no longer argue with interpretations that we disagree with but rather refuse to recognize that those interpretations were ever published or even exist. In the intramural back and forth, we have lost the benefit of being intellectually pushed out of our comfort zones. This raises an important and troubling question: Is there any meaningful benefit to having a multiplicity of perspectives? The answer should be of course there is, but it currently does not go without saying; it happens only if we actually talk to each other and not disengage or dismiss views and interpretations for which we have no sympathy.

The authors are furthermore correct in showing that one of the core dividing lines derives from our persistent skepticism about the value of biology in contributing to the understanding of human behavior. The ongoing withdrawal from, when not outright rejection of, the psychoanalytical paradigm has landed anthropology on the shakiest of terrain—a discipline with a once historically articulated mission to study both the subjective and objective dimensions of human behavior is now erratic in its attitudes and benighted in its mission.

A few anthropologists adopted a cognitive model of the mind, but this approach does not seem to have attracted a wide interest or dependable focus, thus leaving it on the discipline's periphery. Given the absence of a viable conceptual framework for probing the subjective sphere of the mental life, it is not surprising (although it does seem to have surprised many) that evolutionary psychology gradually replaced the Freudian paradigm. Over time the evolutionary approach brought biology back prominently into the equation. For most cultural anthropologists, the reintroduction of biology, no matter how seemingly valid, was

greeted with intense, almost irrational, resistance, accompanied by an insistence that there is nothing new to examine or reinterpret because everything is social and that is that. The by-product of rejecting psychoanalytical and evolutionary analysis has left us without an agreed-upon "psychology of humanness." By default, most of the field is stymied by some de facto tabula rasa (publicly rejected while privately tolerated) that elevates social structure or society over everything else. Since societal forces accordingly shaped everything, there was no need to engage with psychological research or its findings. But without concurrence of conceptual framework or language in which to discuss the subjective sphere, many anthropologists have been inclined to sidestep, if not reject, the exploration of the subjective in favor of personalized ethics.

The retreat into conceptual or, in the authors' words, moral emotional camps of epistemological faith has resulted in a dilution of intellectual skepticism, diminishing the willingness to question underlying assumptions and the conclusions that follow from them. A new kind of cultural anthropology has taken over: a morally engaged social activism.

The authors' survey further reveals that this shift in intellectual orientation wears a political and gendered face. The authors do not comment on this, but I suspect that the political orientation wears an additional face: one of class. It is increasingly more common in research universities, and especially Ivy League institutions, than it is in rank-and-file state universities. An important question has come into play: Do we assess a person's character by how he or she views the world? That is, if we identify with the view, do we therefore think highly of the person? Or if we do not agree with the view, do we therefore think little of the person?

If so, can we as a discipline objectively analyze the work of people whose theoretical orientations we do not like? This should not be surprising, given that our political and social identities have merged together so that one predicts the other, so one depends on the other.

Given these personalized analytical preferences, we are faced with a practical issue: How can journal submissions receive a more balanced and useful review? After reading this modest and thoughtful analysis, I wonder whether editors, when selecting reviewers, should not take into consideration a reviewer's political orientation as well as his or her given expertise. I also wonder whether, 20 years from now, *Current Anthropology* will once again publish papers dealing with the field's reluctance to engage Weber's challenge to distinguish the personal from the objective.

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Horowitz, Yaworsky, and Kickham have provided an interesting picture of the continuing division between proscience and

antiscience factions within anthropology. As they note, the removal of the term “science” from the American Anthropological Association’s (AAA) mission statement as it was presented in the 2010 Long Range Planning Committee (LRPC) report was the catalyst for a vocal critique of the AAA and its leadership. An important issue that the controversy focused on was AAA leadership, a focus that Horowitz, Yaworsky, and Kickham do not carefully examine. What I provide here is a personal and admittedly subjective history of the growing frustration with AAA leadership that played a seminal role in the 2010 controversy.

Conflict with AAA leadership simmered throughout the *Darkness in El Dorado* controversy and boiled over in 2002 when the AAA’s executive board (EB) accepted the *Darkness in El Dorado* task force’s report. The report violated the very ethics guidelines that supposedly underlay the task force’s work, and one only needs to read *Anthropology News* from 2002 and 2003 to see that many AAA members viewed the report not only as damaging to the AAA’s reputation but also as creating distrust of anthropologists among indigenous communities and potentially undermining efforts to carry out large-scale inoculation programs.

Accepting the task force report was not the only divisive action made by AAA leadership in 2002. Another such action involved the rejection of several scientific panels—panels having National Academy members and other senior anthropologists as organizers or presenters—which were subsequently left out of the program for the 2002 annual meeting in New Orleans. In response, a group of disgruntled anthropologists organized a salon des refusés meeting held parallel to the AAA meetings and made up of the rejected panels and several others. AAA leadership was not happy about this, and some members expressed their displeasure in impolitic ways. The direct result was the creation of the Society for Anthropological Sciences (SASci) and a movement to create a separate section within the AAA for scientific anthropology (in part because sections were allowed two symposia at the annual meeting).

In 2003, a resolution was put forward that repudiated the accusation that Napoleon Chagnon and James Neel had fostered a measles epidemic among the Yanomami. The resolution caused considerable debate within the AAA and in fact was declared null and void by the EB due to errors in its wording, although swirling gossip suggested that the EB did not want the resolution introduced because of pressure from the task force and several members who held personal grudges against Chagnon. Significantly, when the resolution was reintroduced it passed overwhelmingly—1,526 to 134—suggesting that AAA leadership either was either out of touch with the members or being disproportionately influenced by a small group of members working to keep the resolution from being introduced.

The following year, concerns about small groups influencing leadership expanded when the EB decided to move the annual meeting from San Francisco to Atlanta without consulting section leadership. The move had reasoning behind it—the conference hotel had locked out its unionized workers in a labor dispute, and unionized AAA members refused to attend the

meeting and raised the issue that the AAA should side with labor and not management in this dispute. There was a ballot and the move did pass. But the vote was done quickly, sent only to those who had registered for the meeting, and had a very low response rate. The impact on attendees, particularly graduate students and contingent faculty, was dramatic, as was the financial impact on the AAA itself due to canceled contracts with the Hilton Corporation. The decision to move the meeting suggested that AAA leadership had become more interested in social activism than in representing the needs of its members (again, all one needs to do is skim through *Anthropology News* for the months following the meeting to see how strong the concerns over leadership had become).

For five years, SASci worked with AAA to create a section. This proved more difficult than expected. Vigorous pushback came from several members of the EB, who argued that anthropology was a science and that there was no reason for such a society under the AAA umbrella. In essence, their argument was that the AAA was already a society for anthropological sciences, but despite this pushback the Society for Anthropological Sciences (SAS—organizationally separate from SASci, although in practice the same) became a provisional section of the AAA in 2007.

In SAS’s first *Anthropology News* column, Stephen Lyon (2008) explained why SAS was created: “The new section is an important addition toward fulfilling the AAA mission to promote anthropological sciences.” Two years later, the LRPC removed the word “science” from the AAA mission statement. As then-president of SAS, I viewed this removal as not only an attack on scientific methods in anthropology but also a direct attack on the newly created SAS. We were a “provisional” section at that point, one that at least some members of the EB disliked and I feared might act to remove—if science were no longer part of the AAA mission, then why have SAS as part of the AAA?

For me, the “science” controversy was as much about a decade-long bias in AAA leadership as it was about divisions among AAA membership. Horowitz, Yaworsky, and Kickham illustrate that divisions among anthropologists still exist and are a cause for concern, but that is only one aspect of the bigger story behind the controversy. The failure of AAA leadership to balance these divisions was equally to blame.

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Horowitz, Yaworsky, and Kickham question whether anthropology is “divided between proscience and antiscience factions.” Their primary evidence is survey questions framed in “familiar and unambiguous language.”

“Familiar” and “unambiguous” are not the same thing. The first question asks whether respondents agree that “scientific advances have improved well-being.” An overwhelming majority (89%) responded positively. On its face, that puts to rest

the claim that anthropology has fractured between pro- and antisience factions.

But that large majority masks deep divisions in all three key terms: science, advances, and well-being. Someone who agrees with the proposition that advances in plant science have improved food security and human nourishment might well disagree with the proposition that advances in the scientific study of kinship systems during the colonial era improved the administration of colonial law and the welfare of subject populations. The disagreement in this case is not merely a matter of the substantive claims. It involves different ideas of science, advances, and well-being.

The ambiguities go even deeper. Some anthropologists hold that ethnographic approaches that emphasize the emotions, personal insights, and experiences of the fieldworker are an advance in the science of anthropology that benefits the subjects of research. Other anthropologists decry such methods as profoundly unscientific. Are "interpretive ethnographies" a form of "science," or are they a branch of imaginative literature?

The word "science" generally retains positive connotations, and even those who reject much of what science has entailed historically may fancy themselves in favor of "science" as something taking place in a laboratory or as a general rubric for their own not-very-scientific work. "Science" conceived as something so elastic as to encompass nearly any practice is not science. The "science" of palm reading is not science.

The basic thesis of the article is thus doubtful, although the authors go on to offer much of interest. Horowitz, Yaworsky, and Kickham evoke the divisions among anthropologists over how "engaged" the discipline should be with power, political responsibility, and "authoritative representations." Their treatments of the controversies over Chagnon, Stoll, and Freeman are framed as efforts not "to adjudicate conflicting claims" but to disentangle "the empirical and the normative" elements in the disputes. In this effort they are moderately successful, although Sahlins's denunciation of Chagnon's field methods, Pratt's (and others') rebukes to Stoll, and Shankman's attack on Freeman are presented as though their points remained unanswered. Granted it was not the authors' goal to give a rounded picture of these controversies but rather to provide a springboard to a key question: Does distrust toward scientific objectivity characterize contemporary anthropology? Their summary of the Chagnon, Stoll, and Freeman affairs strongly suggests that the answer is yes, at least for a prominent segment of the anthropological community—notwithstanding the overall results of their survey.

Their survey does, nevertheless, provide a valuable index of how deeply antisience epistemology has rooted itself in the discipline. That nearly one-third (31%) of anthropologists hold that indigenous cosmologies are "no less" true than modern scientific explanations is astonishing. If we look at the American academy at large, we know where we are most likely to find faculty members who believe that witchcraft explains infectious disease to a degree comparable with microbes; that sorcery explains success in growing food crops at least as well as seed stock, fertilizing, and adequate irrigation; and that various deities explain the apparent motion of the planets and stars at least as well

as modern astronomy. Do 31% of anthropologists really uphold this sort of crude relativism?

I suspect that a much smaller percentage would stake their health or their lives on the "truth" of Azande medicine or Trobriand garden magic, but the willingness of so many to affirm "no less true" in response to a survey question plainly registers a widespread social attitude. The authors draw from this the bland observation that "the ambivalence of the field regarding epistemology is unmistakable." They also capture the contradiction between the strong relativistic stand on "indigenous cosmologies" and the emphatic support (89%) for the UN Declaration of Human Rights, although the percentage that countenance female genital mutilation (28%) tracks pretty closely with the percentage of those who find indigenous cosmologies "no less true" (31%).

Horowitz, Yaworsky, and Kickham offer an explanation for all this that makes a good deal of sense, although it by no means upholds the principle that anthropology as presently construed can be treated as a serious science. Moral foundations theory, they write, suggests that "more is going on than simply intellectual disagreement about the credibility of available evidence." The epistemological disagreements are really surface manifestations of the moral template of the group to which the anthropologists belong. In this instance, the political left in anthropology finds its group foundations in "care and fairness," which "saturate" its "political morality." The anthropologists drawn into this charmed circle "come together in emotive communities [that] would reproduce a discourse of moral responsibility toward subaltern groups."

As the authors present themselves as "part of the left-liberal camp," and conclude their paper by affirming that they are situated not "above the fray" but with their "fellow left-liberal colleagues," I am willing to accept their confession of bad faith. Their commitment to "the pursuit of truth" and to "science," which is also reiterated in the last paragraph, sits awkwardly alongside their other commitment. Indeed, the "group" identity they claim and their belief that its moral foundation in the pursuit of an emotive and therapeutic ethic of care and fairness plainly supersedes any mere intellectual attachment to science or truth.

The article thus turns back on itself in a wide circle. The authors end up providing substantial new evidence that supports the picture of a rift in anthropology between those who adhere to actual scientific standards and those who reject science in favor of something else. What they have clarified is that the "something else" isn't exactly "antisience." It includes antisience, but it is better understood as the self-regard of a moral clerisy.

Reply

We thank the commentators for their thoughtful reflections on our paper. It is gratifying to have our work appraised by such distinguished scholars.

R. Brian Ferguson takes issue with evolutionary psychology, particularly its conception of the brain's functional modularity,

which has affinities with moral foundations theory (MFT). He points to various evolutionary hypotheses that had to be walked back, including ballyhooed genetic or dispositional accounts of such diverse phenomena as language, abusive behavior, depression, and even rape. He cautions that this should make us skeptical of sociobiology.

Fair enough. We hardly endorse evolutionary psychology wholesale, much less genetic reductionism. We are open, moreover, to ongoing discoveries in brain science suggesting far-reaching developmental plasticity. As we stressed, our use of MFT was tentative. It strikes us as a plausible explanation of average emotive differences between the left and the right. It is an explanation, we might add, with a feasible evolutionary backstory (of adaptive moral emotions) and consistent with large-scale twin studies on the substantial heritability of virtually all psychological traits, including political orientation (Hatemi et al. 2014).

Ferguson expresses confusion, however, regarding the applicability of MFT: "I do not understand how this theoretical orientation even applies to the data on hand? [*sic*] It is not about liberals versus conservatives but self-identified moderates, liberals, and radicals, plus gender and disciplinary specialization." Notwithstanding that MFT has been applied, independent of political ideology, to gender, occupation, and more (e.g., Graham et al. 2011), here we assumed our reasoning was plain. Probing anthropologists' views from the far-left to the political middle (given virtually no conservatives or libertarians in the field), we suspect a tempering in the intensity of their left-wing moral sensibilities (i.e., their collective instincts to protect vulnerable groups and to mistrust "authoritative" claims perceived to derogate such groups). Indeed, we interpret the predictive power of political orientation in our data in precisely these terms. Recall moderate anthropologists' more pronounced tendency to reject epistemic relativism and to affirm evolutionary biology and Chagnon's legacy (with correspondingly less congenial views of indigenous cosmologies, alleged prehistoric peaceableness, and promiscuity, etc.).

Oddly, Ferguson skirts this central thrust of our findings. Perhaps due to MFT's innatist footing, he appears intent to dwell on what he sees as the project's normative implications. He cites a variety of work (including his own) that "soundly refutes" evolutionary accounts of prehistoric war, Yanomami reproductive success, or the merits of Cochran, Hardy, and Harpending's (2006) thesis on Ashkenazi intelligence. He views these "rigorous criticism[s]" of sociobiology as "dispassionate" and "informed skepticism," unduly dismissed as "politically correct," and he worries that our paper provides a "political defense" for evolutionary positions challenged on expressly "empirical" and "theoretical" grounds.

Maybe. As we concede throughout, we do not aim to adjudicate the controversies. If Ferguson is right, it happens that the more left-leaning anthropologists better track the truth in their views on these sensitive questions. Many moderate colleagues would no doubt demur. We can only advise caution. Awareness of ourselves as both scientific and emotive communities may

help explain why our politics tends to align with our interpretations of evidence. That Ferguson sidesteps this essential connection is a pity.

Douglas Fry and Geneviève Souillac highlight our desire for anthropologists to engage in serious self-reflection about biases. We welcome their suggestion that our survey results could be usefully interpreted with other frameworks. They offer a host of questions that scholars might bring to bear in their research in a spirit of critical reflexivity.

We certainly claim no settled position on prehistoric war or Chagnon's (in)famous killers-have-more-kids finding. With respect to Fry and Souillac's critique of Chagnon, we would expect work that piques the moral emotions of a field to invite a range of interpretive and other challenges. A higher level of methodological scrutiny perhaps goes with the territory. Time may tell, in any event, whether special attention to evolved moral intuitions bears scientific fruit.

Fry and Souillac suggest that anthropologists should better attend to the history and philosophy of science. For what it's worth, although hardly professional philosophers of science, we are drawn to a middle ground between realism and constructionism. After all, our theories are constrained both by the facts "out there" and by our cultural precepts and interests as knowledge producers. We wonder whether scholars' attraction to these philosophical stances might coincide in some way with their respective moral intuitions. (A question for future study perhaps.)

We appreciate William Jankowiak's laudatory appraisal of our project. Echoing Wrong's (1961) classic critique of sociology, Jankowiak laments what he sees as inadequate psychological grounding in anthropology's analyses of human behavior. He finds more to lament, in fact, in the drift of postmodern relativism from the humanities and social sciences into—of all places—the Republican Party. In such an "insecure culture," where truth is "up for grabs," anthropology founders by disavowing psychoanalytic or evolutionary insights into human subjectivity.

Jankowiak finds much to recommend in our sketch of the field's "epistemological tribes." His concern that anthropologists too often talk past each other or refuse to recognize opposing interpretations seems, sadly, to ring true. While praising our emphasis on contrasting moral emotions, he goes further in proposing a *class* dimension to anthropology's relativist-activist subculture. We find the hypothesis intriguing. His astute observation reminds us of the anthropologist-cum-journalist Sarah Kendzior's (2012) wry portrayal of the plight of debt-ridden adjuncts at an AAA conference. We cannot help but quote Kendzior: "When I expressed doubt about the job market to one colleague, she advised me, with total seriousness, to 're-evaluate what work means' and to consider 'post-work imaginaries.'" Whatever the reach of such a "patrician" sensibility, we welcome efforts to map anthropology's emotive terrain beyond our focus on moral intuitions.

We learned much by reading Peter Peregrine's analysis of the AAA's inner workings. His observations provide useful context

for our paper. Peregrine's comments about the sudden change of venue from San Francisco to Atlanta dovetail with the concerns raised in the abovementioned Kendzior essay. His observations about the refusal of the AAA to host some of the scientific panels also corresponds with Jankowiak's position of a field talking past each other. We are delighted this forum provided a space for Peregrine to share his important account of leadership dynamics in the AAA.

Peter Wood draws attention to some of the ambiguities in our questionnaire. Arriving at surefire language in a survey is well-nigh impossible. That does not invalidate his criticism, but about all we can say is that we did our best and the significant results and patterns are not easily explained away. Wood suggests that although our thesis is "doubtful," we "offer much of interest," and he evaluates us as being "moderately successful" at disentangling the empirical and normative aspects of anthropology's controversies. He notes the contribution we made by documenting the depths of "antiscience epistemology" in the field. And he finds that our explanation makes "a good deal of sense."

We are of course pleased with these assessments. Wood in fact captures a key thread of our argument well: "The epistemological disagreements are really surface manifestations of the moral template of the group to which the anthropologists belong." Yet in elaborating, he makes inflated claims that appear to render our argument contradictory. Wood suggests, for example, that our open identification as left-liberals amounts to a "confession of bad faith," which he accepts. It is not altogether clear what he means. Apparently he views political commitment as inherently antithetical to the pursuit of scientific truth. The authors' "commitment" as left-liberals, he notes, "sits awkwardly alongside" their "commitment to the 'pursuit of truth' and to 'science.'" He continues with his most provocative point, which we should quote at length: "Indeed, the 'group' identity they claim and their belief that its moral foundation in the pursuit of an emotive and therapeutic ethic of care and fairness plainly supersedes any mere intellectual attachment to science or truth."

Here we believe Wood overplays his hand. We demonstrate, to be sure, the impact of scholars' moral intuitions on their interpretations of evidence. Our attention to the "politics of knowledge" certainly contravenes naive views of value-free knowledge production in the academy. Yet must we bend the stick so far as to reduce social science to a jockeying over identity and values?

In his intuitionist model of moral reasoning, Jonathan Haidt (2012) designates our reasoning faculty the "rider" and our underlying moral intuitions, the "elephant." Drawing on David Hume, Haidt stresses that the elephant usually rules the day, dwarfing the influence of the tiny rider. Yet again, we stress *usually*. Haidt differs from Hume in suggesting that the rider acts as a lawyer and that the lawyer can sometimes persuade the client, overriding her underlying emotions. That is to say, reason is the lawyer and sometimes the lawyer's counsel has effect. This is our fundamental point. If we wish to overcome the blunders caused by our biased attachments to our seemingly intractable

moral foundations, then we had better heed the lawyer when the lawyer is informed by science. This is by no means an easy task. But we reiterate, and we believe without contradiction, that recognizing our emotive roadblocks might just clear a path to better approach the truth.

—Mark Horowitz, William Yaworsky,
and Kenneth Kickham

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