

**On the Means and Ends of Anthropology**  
**with Special Reference to the U.S. American Academe:**  
**A Reply to Gupta and Stoolman's 'Decolonizing U.S.**  
**Anthropology'**

by Alexander M. Thomson

**Introduction:**

It seems that Anthropology has been in crisis mode for some time now. Some of the crises that Academic Anthropology has overcome (or at least learned to live with) include: the obliteration of its traditional subject matter –that is, the destruction of traditional societies– by the spread of the global economy (Goffman 1983); the compulsory recursion to the study of our “own” society (Ortner 2003); the ever-widening gap between the four fields, plus the off-splitting of new subfields (e.g. psychological anthropology); the post-modern morass of “writing culture” (Clifford and Marcus 1986); the quarrels over self- and other representation (Brettell 1993); the habituation of our research communities to academic jargon (Thomson 2021); the ‘interdisciplinary’ pilfering of concepts –mainly, from philosophy<sup>i</sup>– in implicit recognition that traditional anthropological concepts no longer do the trick; the ethical dilemmas occasioned by the study of communities whose values are not alien but repugnant<sup>ii</sup> to us (Teitelbaum 2019); and now, the question of how to deal with the new men (*homines novi*, Lat.) in our departments—the question of whether our canons and our methods be amenable to their use or, contrarily, these be inherently exclusionary (read: racist). This final point lies at the heart of Gupta and Stoolman's

presidential address, “Decolonizing U.S. Anthropology” (2022).

It may be that I have overstated my case. In *The Study of Scientific Revolutions* (2012 [1962]), Thomas Kuhn distinguished between the detection of “anomalies” (or “counter-instances”) by scientists in the course of “normal science” conducted under a paradigm and the full-fledged of an anomaly into a “crisis” which he calls “extraordinary science,” a crisis which necessitates the abandonment of the previous paradigm and the development of a new one. I am not completely sure that we have reached the breaking-point where our methods cease to work and where our canons cease to be intelligible without translation<sup>iii</sup> (yes, we have to cross out the word ‘primitive’ whenever we come across it in our readings, but this feels less like a paradigm shift and more like a terminological one.). Then again, it may be that we are in a state of “quiet crisis”—that the canonical works *have* in fact stopped speaking to us and that we would have realized this sooner but that we *tacitly* abruptly our reading of these works rather than *explicitly* striking them from the canon.<sup>iv</sup> (I was never was assigned Boas, Benedict, Herskovits, Kroeber, Lowie, or Mead during my graduate studies at UCLA. The core seminar series for socio-cultural anthropology started with Marx and Weber and ended with Baudrillard and De Certeau. I did read Sapir with Michael Silverstein at the AAAs one year. He remarked that “we, anthropologists, do not eat our own as much as other disciplines do.” Maybe the conspicuous absence of cannibalism –or, to change the metaphor, parricide– among anthropologists was merely a sign that we had found another way to dispose of our undesirable relations, that we had locked them up in an oubliette, sent them away to an asylum, all the while pretending that they were simply away on vacation. Maybe the unchristening of Kroeber Hall is a sign of a more

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iii See Kuhn's description of the attempt to ‘derive’ Newtonian dynamics from Einsteinian dynamics (pp. 100-103).

iv “When a prior paradigm is repudiated, a scientific community simultaneously renounces, as a fit subject for professional scrutiny, most of the books and articles in which that paradigm had been embodied.” (Kuhn 2012 [1962]:166)

honest policy: “Whereas allegations have recently sullied his name and whereas his work, having been rendered arcane and archaic by the passage of time, does not support his claim to fame, let us forget this ‘Kroeber’ character once and for all...”)

Whatever the case may be, I do not believe that Gupta and Stoolman were trying to induce a crisis through their presidential address. (And I hasten to add that were this their intent there should be no shame in it for, according to Kuhn, science only ever progresses via crises, the image of science as a steady accumulation of facts, laws and theories, i.e. “science-as-accumulation,” being little more than a myth.) To hear Gupta tell it, he was taken aback by the hostile response to his address. To hear Gupta tell it, he saw his presidential address as a call for reform and not for revolution.<sup>v</sup> Yet by treating our hardly-read but highly-revered founders so unceremoniously, Gupta and Stoolman may have heralded a real revolution in the discipline of American Anthropology—a revolution which demands that we reject many of its founders' premises, methods, theories and conclusions; and which, by removing them as authorities and remanding us to our own custody, relieves us of all guard-rails and guarantees, and not merely those that Gupta and Stoolman may deem ‘problematic.’<sup>vi</sup> The purpose of this piece may well be to drive this antinomian point home. As the Good Book says, “Do not think that I came to bring peace on earth. I did not come to bring peace but a sword.”

## **The Paradoxical Origin and Orientation of U.S. American Anthropology:**

The centerpiece of Gupta and Stoolman's address (2022:4) is the question: “Why did mainstream US Anthropology fail to move from a *liberal antiracism* to a *decolonizing project*?”

This question contains two major presumptions: firstly, it presumes that the original and

authentic mission of U.S. American Anthropology –or, more precisely, its mission under the charter of the AAA's third president, Franz Boas– was antiracist activism within a liberal socio-political sphere; and secondly, it presumes that their ‘decolonizing project’ is a logical –yea, eschatological– development that should have transpired on its own save for the intervention of some catastrophe. (It will not have escaped the reader's notice that Gupta and Stoolman have crafted a familiar “fall-from-grace” narrative.) To discover the cause of this catastrophe, Gupta and Stoolman propose a revisionist reading of the discipline's history and, specifically, the works of Margaret Mead.<sup>vii</sup>,<sup>viii</sup> One of the central ‘findings’ to come out of this revisionist reading is that “the project of cultural relativism eclipsed that of antiracism” (2022:4). For although “the Boasian project brought together antiracism and cultural relativism” (ibid.), his prodigal children have since encouraged one these co-equal terms to smother the other. Another important finding to arise thence was that Mead and her colleagues were conducting their research amid White Supremacist structures, blithely unaware that they and their public dwelt on the *light* side of the veil while their research subjects dwelt on the *dark* side of the veil. This point is expressed more clearly in the pre-publication version of their address: “In a nutshell, that was the problem with the Nacireman project as a whole: the familiar that it was estranging was a white, middle-class self; it presumed that the subject [i.e. target] of anthropological critique was a white, middle-class ‘we.’” (Gupta and Stoolman 2021:13).

As far as destin-errant narratives go,<sup>ix</sup> Gupta and Stoolman give us with a fine example (although some may quibble that their prefatory criticisms of Boas detracted from their message inasmuch as these criticisms undermined the identification-with-the-father that we, the readers, required to turn against the Nacireman heresy and to be re-turned –back-but-also-forward– to the purified antiracist creed that our founder and forefather, Franz Boas, would doubtlessly profess

were he alive to-day). From a more analytical standpoint though, I would contend that Gupta and Stoolman do not ascend to a high enough level of generality and so, misrecognize the source of all our troubles: to wit, Boas created a monster when he joined “liberal antiracism” and “cultural relativism” –or, more generally, “moralistic subjectivity” and “scientific objectivity”– together. The Nacirema project –that is, the project of using anthropological “science” as a cover for a reformist agenda back home– is common to Boas and Mead. The only difference is that Boas wagged his finger at American racism, while Mead wagged her finger at American sexism (and prudery). Both of them were identically, fundamentally intellectuals posing as scientists.<sup>x</sup>

This becomes most apparent when one tries to square “cultural relativism” with “socio-cultural reformism” (both “liberal antiracism” and “sexual liberalism” fit the bill). Here we have a paradox. On the one hand, U.S. American Anthropology insists on the relative worth of every socio-cultural system (i.e. “cultural relativism”) and on the ethnographer's duty to refrain from passing judgment on any particular socio-cultural group (i.e. “the ethnographic epokhe”), especially when the judgment-to-be-passed is based on the ethnographer's domestic standards and so, might be presumed<sup>xi</sup> to raise the ethnographer's home group above the study group (i.e. “ethnocentrism”). This studied neutrality, we were always told, had a scientific purpose: one had to suspend judgment so that one might understand each specific culture in its own terms and eventually, maybe, arrive at a holistic Understanding (*λογία*) of the entity known as Man (*ἄνθρωπος*). On the other hand, U.S. American Anthropology calls for the anthropologist to use his<sup>xii</sup> knowledge of other socio-cultural groups for the amelioration of his own society or, more grandiloquently, the whole world. What this amelioration should look like need not detain us. It may be antiracism, or sexual liberation, or even militarism provided that the war be just (and all wars are either just or justifiable). What *really* matters is that an anthropologist, who supposedly

starts his study without any moral standard, can derive a superhuman moral standard from a few years spent living with another socio-cultural group, a moral standard which is *so secure* that it may be used to adjudicate current affairs and re-engineer an entire society.<sup>xiii</sup> This is farther than Descartes ever got with his radical doubt!<sup>xiv</sup> Laid side-by-side, these two doctrines show us that cultural relativism moves in a direction counter to charity: it starts abroad and ends at home.

Since Gupta and Stoolman have already started to scrutinize Mead, let us use her work to illustrate the function of the aforementioned cultural reflex. Whereas Mead claims that “masturbation is an all but universal habit beginning at age six or seven,” and that “homosexual activity is regarded in native theory as imitative of and substitutive for heterosexual activity,” and that “casual homosexual practice [is viewed] simply as *play*, neither frowned upon nor given much consideration” in Samoa, mainstream U.S. American Society can stand to liberalize its puritanic laws and mores around juvenile sexual activity.<sup>xv</sup> Were it to “take a page from the Samoan playbook,” the youth of this country might suffer from fewer psychological complexes.<sup>xvi</sup> The unmistakable implication of such fallacious statements is that there is something *wrong* with the (desuete) practice of punishing our young masturbators and homosexuals—that America's sexual mores are minimally *dysfunctional* and maximally *immoral*.<sup>xvii</sup> Such ratiunculæ are anything but scientific. Far from elucidating the generico-specificities of human sexuality, i.e. all the ingenious ways that humans have devised and do aye devise for to regulate their libidines, the image of anthropology that emerges from Mead's work is one of a pseudo-science which uses the pretext of scientific objectivity (“value freedom”) to pursue a clandestine socio-political agendum, viz. the reformation of American sexual mores, according to value schemes (“value thrallldom”) that had little to do with Samoa. Meanwhile, the image of the professional anthropologist, i.e. the anthropologist at the lectern, which emerges

thence and therewith is one of a corrupter of the youth, a sexual libertine who would poison the well of American virtue and tear at the foundations of this Great Civilization.<sup>xviii</sup> At least such an image would emerge if one were on the other side of the culture war.

## **Hodiernal Anthropological Inquiry:**

We all know the joke: “What is the difference between an anthropologist and a sociologist? A sociologist can do math!” But let's be serious now. Wherein does the difference between these two disciplines actually lie? The answer, I would venture, is that sociology was conceived as the study of our society (i.e. the “west,” the “global north,” you name it!), while anthropology was conceived as the study of other societies. This is to say that anthropology was not established on the principle of “everybody studying everybody” *pace* Prof. Lewis. It was only after such a joke became tellable –that is, *after* our traditional subject matter began to crumble away under the inexorable weight of capitalism and its creative destruction, *after* we had turned our sights back on our own society and started to vie with sociologists for research projects, *after* we had realized that Anthropology, or one moiety of it at least, was being held together by nothing more substantial than its predilection for ethnographic methods and not, *deo gratias agamus*, surveys and statistics– that anybody or anything could be deemed an appropriate subject for anthropological research, provided that ethnographic methods were employed. It was only after our existential crisis and comedy that such a falsity could acquire a sheen of verity.

First our fieldsites changed and now we ourselves are changing (a fact which calls for a heap of quotation marks around all the pronouns and deictic co-ordinates in our texts from now until doomsday). The changing composition of our *corps professionnel* is consubstantial with the

changing demography of the United States, more broadly. While this demographic shift feels unprecedented, it is not devoid of historical parallels. Neither are theoretical primitiæ lacking. Only these signal theories are not to be found in the discipline of Anthropology, but in Sociology (again with the Sociology!).<sup>xix</sup> Robert Ezra Park, the one-time student to Georg Simmel and amanuensis to Booker T. Washington, converted his doktorvater's *Fremde* into the *Marginal Man*

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- i Below we will discuss Kuhn's theory of scientific revolutions. In that context, it is important to underscore his statements on the scientist's turn to philosophy in times of extraordinary science. Kuhn (2012 [1962]:88) writes: "It is, I think, particularly in periods of acknowledged crisis that scientists have turned to philosophical analysis as a device for unlocking the riddles of their field. Scientists have not generally needed or wanted to be philosophers. Indeed, normal science usually holds creative philosophy at arm's length, and probably for good reason." This statement certainly holds true for the period preceding the emergence of quantum physics. For instance, it is known that Albert Einstein's notion of time-space was partly inspired by his reading of Arthur Schopenhauer (Howard 1997). It seems plausible that the same pattern may hold for social sciences like Anthropology. Then again, it may be contended that less distance exists between Anthropology and Philosophy than Physics and Philosophy. If true, the diminished distance may allow for a more ordinary (and orderly) communication among the first two disciplines. It is therefore uncertain whether the 'phenomenological' proclivities of anthropologists like Duranti and Throop ought to be read as a symptom of a current or impending crisis ("extraordinary anthropological research").
- ii This is a curious word, *repugnant*. What or who is being fended off? I would like to suggest that it is we, the anthropologists, who are here doing the off-fending. We repugn these 'repugnant' others for fear of losing our bearings –our sense of self as well as our sense of right and wrong (i.e. our system of values)– by permitting ourselves to play the participant-observant ethnographer in their social world. We repugn them for fear that we might "go native." So, except for a few rule-affirming exceptions (Teitelbaum), we eschew the study of these repugnant others, quietly intoning: "There but for the grace of God go I."
- v During the colloquy organized by Vilnius university (Sobo et al. 2022, 1h18m), Gupta makes the following statement: "Contrary to what Dr. Lewis said, we are not saying, we never said, we don't say, that we should throw away what we have learnt from the past, that we don't consider it useful. On the contrary, we think it's extremely useful. We are not launching an assault on anthropology's history. All we are saying is, like every other generation has done, we need to ask new questions of that record –of the ethnographic record– and we have to broaden what we consider the ethnographic record [to be]. So when Native American scholars and communities, and African-American scholars and communities, and Latine scholars and communities, and Asian-American scholars and communities, tell us that you have misrepresented –that you anthropologists have misrepresented– our past, we need to listen to that. We need to listen to them. We can't be telling them 'as anthropologists, as the experts, we know that what you are saying is incorrect.'"
- vi Kuhn (2012 [1962]:94) offers the following comparison of political and scientific revolutions: "Because they differ about the institutional matrix within which political change is to be achieved and evaluated, because they acknowledge no supra-institutional framework for the adjudication of revolutionary differences, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force... Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has this character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular..." What this comparison shows is that during a paradigm shift the appeals to authority implicit to normal scientific activity (cf. Kuhn 2012 [1962]:80) must give way and a decision among competing paradigms must be made on the basis of their relative explanatory power (cf. Kuhn 2012 [1962]:167). The pertinence of this insight to Anthropology's current crisis is this: whether or not Boas intended for U.S. Anthropology to have an antiracist agendum as Gupta and Stoolman aver he did (2022:4)



against the chronotopic backdrop of the U.S. American City at the start of the 20<sup>th</sup> Century and the going-concern of human ecological encounters among ethno-racial groups.<sup>xx</sup> His student, Everett Cherrington Hughes, wrote about the challenges that new men (*homines novi*, Lat.) face whenever they enter a profession from which they were erewhiles excluded. Hughes' assumption –the assumption of the twentieth century– was that a small number of new men could assimilate

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becomes a moot point once one calls the Boasian charter (i.e. paradigm) into question as Gupta and Stoolman do in their presidential address.

- vii For the sake of transparency, it should be noted that Gupta and Stoolman's retro-speculative exercise is heavily indebted to the book chapter, "Skeletons in the Anthropological Closet" by William S. Willis, Jr. (1972) and that, although Gupta and Stoolman do make reference to Willis' work via citation and quotation, they do not recognize him for the « metteur-en-scène » that he was. A side-by-side reading of the two pieces would demonstrate that they quite literally "stole the show," making only the most minimal of alterations to the cast of characters: so, the "color bar" (Willis 1972:123) is swapped out for the "color line," which Gupta and Stoolman (2022:5) attribute to W.E.B. DuBois (1903:15); the *argumentum contra istud flagitium* "Malinowski" (Willis 1972:140) is swapped out for one against "Mead" (Gupta and Stoolman 2022:4); and Willis' argument on the de-politicizing effects of the culture concept (Willis 1972:126, 137 et passim.) is re-named and re-presented as the "Nacirema Project" (Gupta and Stoolman 2022:4). Franz Boas for his part receives slightly more favorable lighting in Gupta and Stoolman's production (cp. Willis 1972:141). All in all, one wonders why Willis was not listed as a co-author on the presidential address, since he gave them the symbolic and interpretive template for their work. "In any case, data collection and interpretation are closely bound in interpretive and symbolic approaches, so that the anthropologists' understanding of their interlocutors is inseparable from the work of these intermediaries. A decolonized anthropology would consider seriously the 'authorship' of this group of people..." (2022:11).
- viii A second aspect of Gupta and Stoolman's retro-speculative exercise after their revisionist reading of Mead is the development of a "counterfactual history" of anthropological research (2022:5-8). This "counterfactual history" amounts to a set of research topics that early anthropologists might have studied "if the discipline had been constituted as a decolonizing project" (ibid.). These topics include: 1) The Study of Genocide and Mass Killings, 2) The Study of Slavery and Structural Violence, 3) The Study of Legal Treaties and the Political Systems that Enable Their Abrogation or Enforcement, 4) The Study of Forced Migration and Internally Displaced Populations, 5) The Study of Kinship and Other Nonhuman Animals, 6) The Study of Reparations, Landback Initiatives, Truth and Reconciliation, Redistributive and Restitutive Justice, and the Redressal of Historical Wrongs, 7) Critical Approaches to Borders, Nationalism and Citizenship, and 8) Critical Approaches to the Study of Extractive Industries, Industrial Agriculture and Monopoly Capitalism (Gupta and Stoolman 2022:5-8). While I nowise wish to dispute that the anthropologists of the early 20th Century might have studied some of these topics, I would push-back against the claim that Gupta made in response to Dr. Gitka De's intervention at the Vilnius Colloquy: namely, that this set of research topics is not "arbitrary," but much to the contrary is a replica of one which stood ready for anthropological adoption circa 1900 or 1910 ("I just want to clarify one thing about, you know, when you said that the list of topics was arbitrary that we selected. I'll tell you why it wasn't arbitrary, because what I was saying there, what I was thinking at that point, was that if I was looking circa 1900 or 1910, if I was looking at what had happened in the immediate past to the communities with which I was working, namely, Native American communities and African American communities, what would be the main concerns that they might have had? So that list comes out of there. That's what generates that list. It's not saying that in today's world these are the most important questions..."). My first objection is that this topic-set contains references to social practices which, not existing at that period, could hardly have been made a topic of anthropological study. For instance, the social practice of Truth and Reconciliation Commissions developed gradually over the course of the *Mid-to-Late Twentieth Century* with Uganda's "Truth Commission: Commission of Inquiry into the Disappearance of People in Uganda since 25 January, 1971" often cited as the first of its kind.

into a large stock of ‘old’ men (*homines veteres*, Lat.) and that, by demonstrating their mastery of the working-codes, they should gradually win the acceptance of their clients and their colleagues.

He did not, to the best of my knowledge, dwell on cases where the working-codes were so wrought as to make the profession all but impracticable for someone with some such identity.

Perhaps he did not believe that such cases existed.

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My second objection is that the list contains research topics which, although theoretically amenable to study during the aforespecified period, derive all of their import from subsequent world-historical events. Take “Critical Approaches to Borders, Nationalism and Citizenship” for instance. One of the main reasons why Benedict Anderson’s *Imagined Communities: Reflections on the Origin and Spread of Nationalism* became the single most-cited angloglossic text in the social sciences was the collapse of the Soviet Union and the end of the Cold War as Anderson wot well. In his memoir *A Life Beyond Bounds* (2016), Anderson recounts how he “received a frantic telephone call from a high official at the Kennan Institute, one of the key centers for Soviet studies.” The official on the line “begged” Anderson “to fly down and give a talk at his institute.” When Anderson asked why his expertise should be sought with such urgency—his book had received few plaudits at first publication and he himself was esteemed little more than an area specialist by his colleagues—the official told him that “Soviet Studies is finished, money is not coming in anymore, and our students can’t get jobs. Everything in the former Soviet Union is about nationalisms, and almost no one here has studied them. You are among the few people in the country who can help us get back on our feet [Anderson 2016, Ch. 5].” Hobsbawm corroborates Anderson’s comments on the stimulus that the Nationalism Studies received from the Fall of the USSR in the second edition of *Nations and Nationalism since 1780* when he remarks (1992 [1990]:163) that “more new nation-states have been formed [in the past two years], or are in the process of formation, than at any time in this century.” It is ridiculous to assume that Native Americans, living eighty to ninety-one years *before* the Fall of the USSR—that is, the *geopolitical event* that launched Nationalism Studies into the heavens of academic inquiry—, were beseeching obstinate Anthropologists and Ethnographers to drop the documentation of their dying cultures and languages (“salvage anthropology”) so that they might instead conduct “Critical Studies of Borders, Citizenship and Nationalism.” The fact that a concept like “citizenship” may exist in one’s lifeworld does not mean that anybody is prepared to “thematize” it academically.

- ix The concept of “destinerrance” is borrowed from Derrida, who introduced it amid his reflections on Poe’s purloined letter in *La Carte Postale* (1980) but prefigured it even in his reflections on Rousseau’s « Essai sur l’origine des langues » in *De la grammaire* (1968). For a good piece of secondary literature, the reader is referred to J. Hillis Miller’s “Derrida’s Destinerrance” (2006).
- x “This type is not easy to define. The difficulty is in fact symptomatic of the character of the species. Intellectuals are not a social class in the sense in which peasants or industrial laborers constitute social classes; they hail from all the corners of the social world, and a great part of their activities consist in fighting each other and in forming the spearheads of class interests not their own. Yet they develop group attitudes and group interests sufficiently strong to make large numbers of them behave in the way that is usually associated with the concept of social classes. Again, they cannot be simply defined as the sum total of all the people who have had a higher education; that would obliterate the most important features of the type. Yet anyone who had—and, save exceptional cases, nobody who had not—is a potential intellectual; and the fact that their minds are all similarly furnished facilitates understanding between them and constitutes a bond.” (Schumpeter 2008 [1940]:146). And a few pages later: “Intellectuals are in fact people who wield the power of the spoken and the written word, and one of the touches that distinguish them from other people who do the same is the absence of direct responsibility for practical affairs. This touch in general accounts for another—the absence of that first-hand knowledge of them which only actual experience can give. The critical attitude, arising no less from the intellectual’s situation as an onlooker—in most cases also as an outsider—than from the fact that his main chance of asserting himself lies in his actual or potential nuisance value, should add a third touch.”

Whatever Hughes may have thought, Gupta and Stoolman *do* believe that our professional norms are inhospitable to BILPOC.<sup>xxi</sup> They ask (2022:9) their readers to critically reflect on “white-norming,” which they define as “the many (unwitting, unremarked, un-self-conscious) ways departmental practices assume that the faculty member is a white, upper-middle-class, cisgender, heterosexual, able-bodied person?” Whereupon Gupta and Stoolman

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- xi It is hypothetically possible that the ethnographer might meet with a foreign socio-cultural group that exhibits all the virtues and none of the vices that his own group has identified as such. This would make for a “more Catholic than the Pope” type situation.
- xii I use the ‘generic he’ in this essay. The rhetorical device stresses singular-universal quality of the anaphor. Obviously, actual anthropologists may have any number of gender identities and use any number of pronouns for themselves. The use of the generic ‘he’ is not intended to erase the diversity of gender-identities in our *corps professionnel*. Nor does it—at least, it does not erase them anymore than Gupta and Stoolman’s notion of “white-norms” erases the other axes of diversity that it purports to include, viz. socio-economic status, gender identity, sexual orientation, and dis/ability. Just as the White Anthropologist who is subject to the stigma of a disability, or rural background, or humble socio-economic origins, or a non-masculine gender-identity (cf. Goffman 1963:128) must identify himself with category of BILPOC and not make the mistake of identifying with the sort of “whiteness” and “white-norms” under critique given Gupta and Stoolman’s statement in support of All The Struggles™, I trust that my readers shall have no trouble reading themselves into the ‘generic he’ given my acknowledgment of the gender-spectrum present in our *corps professionnel*.
- xiii The fact that certain current anthropologists disagree with earlier anthropologists’ judgment (Gupta and Stoolman 2022:8) does little to temper their own ambitions. For though their disciplinary ancestors may have been wrong about, say, participating in the Allied war-effort during the Second World War, they were *right* in recognizing the world-historical mission of the anthropologist.
- xiv Descartes (1987 [1637]:4) writes: « Je ne saurais aucunement approuver ces humeurs brouillonnes et inquiètes, qui, n’étant appelées, ni par leur naissance, ni par leur fortune, au maniement des affaires publiques, ne laissent pas d’y faire toujours, en idée, quelque nouvelle réformation. Et si je pensais qu’il y eût la moindre chose en cet écrit, par laquelle on me pût soupçonner de cette folie, je serais très marri de souffrir qu’il fût publié. Jamais mon dessein ne s’est étendu plus avant que de tâcher à réformer mes propres pensées, et de bâtir dans un fonds qui est tout à moi... La seule résolution de se défaire de toutes les opinions qu’on a reçues auparavant en sa créance, n’est pas un exemple que chacun doit suivre; et le monde n’est quasi composé que de deux sortes d’esprits auxquels il ne convient aucunement. A savoir, de ceux qui, se croyant plus habiles qu’ils ne sont, ne se peuvent empêcher de précipiter leurs jugements, ni avoir assez de patience pour conduire par ordre toutes leurs pensées... Puis, de ceux qui, ayant assez de raison, ou de modestie, pour juger qu’ils sont moins capables de distinguer le vrai d’avec le faux que quelques autres par lesquels ils peuvent être instruits ... »

One must remember though that Descartes was not the censor here but the censured. He was writing from the weaker position—from the moral low-ground. Oh! How much depends on the weakness of one’s position! Always and everywhere it is the weaker class of men who counsel patience, and tolerance, and respect for difference. The moment one gets a few draughts of power in him, his principled commitment to ‘freedom of conscience’ and ‘freedom of speech’ go out the window. Hence this heuristic for determining the power relations in any situation: Ask whose views may be proclaimed apodictically and whose views may not be uttered without apology; ask who can compel others to endorse his views on pain of expulsion and who, having stated his conscientious objection thereto, has seen himself expelled from the group; ask, in short, who wields the censor’s pen; then you will have a much clearer idea of the power relations than you could ever garner from the discourse itself (e.g. talk of being made to “feel unsafe” by another’s words).

- xv Another example of the cultural reflex can be found in the commentary that Dr. Lars Krutak, a research associate at the Museum of International Folk Art in Santa Fe, New Mexico, supplied to the New York Times for a

make several suggestions for reforming these white-norms including: the canonization of BILPOC authors, the deputization of BILPOC persons as interpreters of the new BILPOC canon, the favoritization of BILPOC persons during hiring decisions,<sup>xxii</sup> the valorization of work done *pro bono communitario*, and the normalization of multi-author publications<sup>xxiii</sup> *inter alia*. If these suggestions seem somewhat *ad hoc* or *hodge-podge* in nature, this appearance may be due to the fact that Gupta and Stoolman do not thematize the structural contradictions<sup>xxiv</sup> in which our

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newspiece on the laws and mores pertaining to the tattooing of children and youth in the United States (Nir and Berner 2022). In said piece, Krutak is quoted indirectly at first as stating that “limiting tattoos to adults is a relatively modern, Western practice” and that “from Japan to Kenya to Borneo, tattoos for children marked life stages...”. He is then quoted directly to suggest that “maybe decolonizing the Western thought concept of ‘age appropriate’ tattoos could be enlightening,” a suggestion which he walks-back in the next sentence when he expresses some reservations around 10- and 11-year olds getting tattoos on the grounds that “they still have a lot to learn about the world.” All qualifications aside, the suggestion is that we might reform our laws and mores on childhood tattooing in view of the *fact* that some unquantified subset of the socio-cultural groups scattered around the globe do actually have or have historically had a custom of tattooing children. This suggestion leaves a lot to the imagination including: the function and significance of tattoos within each socio-cultural group, the function and significance of tattoos in the United States to-day, and the effects of ‘transposing’ any cultural element from one group to another. An authentic “cultural relativism” would restrict itself to observing that ‘neither prescriptions nor proscriptions on childhood tattooing are universal human practices’ and refrain from exhorting the U.S. American public to ‘de-colonize its thought by abolishing the concept of age appropriate tattoos’ (i.a.v. to exchange its practice of proscribing childhood-tattooing for one of prescribing or, what is more probable, permitting the tattooing of children.). Indeed, an authentic “cultural relativism” would take pains to state plainly that the current dispute over childhood tattooing may be supposed to have socio-cultural determinants unique to our society and that whatever resolution is ultimately reached, whether affirmative or reformative, it may be expected that it should have repercussions for the rest of our socio-cultural system, where neither the determinants nor the effects should have any *real* relation to other socio-cultural groups and their practices. An authentic “cultural relativism” would thus stress that the cultural practices of other groups are *completely irrelevant* to our socio-cultural (re-)formation.

xvi See the fourth chapter of Robert Morrison MacIver's autobiography *As a Tale is Told* (1968) for a first-person account of “the deep impression of the perils of sex that the hushed and solemn attitude to the subject” prevalent in the island community of Stornoway (Scotland, UK) made on him as well as his own developmental reaction thereto.

xvii Foucault shows himself to be a stronger and more clear-sighted theorist when, in the course of his *History of Sexuality*, he challenges the ontological reality of sexuality, dismantles the whole narrative of sexual repression and sexual liberation, and demonstrates how the bourgeois notions of *sex* and *sexuality* –which he contradistinguishes against the aristocratic notion of *blood*– were instrumental in the production of its social body (corps social, Fr.).

xviii Behold the asymmetry of “cultural relativism.” It is permissible to praise other societies, but to praise one's own is immediately suspect.

xix Gupta and Stoolman seem to intuit as much when they propose that we admit W.E.B. Du Bois into the anthropological canon (2022:4). Only they reason that he should be admitted as a BILPOC author (ibid.), which just goes to show that they regard him primarily as a Black Man and only secondarily as a Sociologist. One wonders whether Du Bois would have been pleased or displeased to be their “diversity hire.” Certainly, he complained in his day that: “Johnson and Bunche and I myself are working in Negro universities, not because the subject matter which we teach is purely ‘racial’; or because science should have a ‘racial’ tinge; or because ability should be segregated by color; but for the obvious reason that no white university in the United States is going to give us a chance for teaching and research (Du Bois 1939:558; see also Du Bois 2007 [1903]:187-8).”

*homines novi* befind themselves; nor, for that matter, the contradictions wherewith our *homines veteres* are now beset. They never ask *whether* the Nacirema project, which they misdate to the era of Mead and which they mistake for a heretical aberration from the Boasian project (Q.E.D.), be practicable for these *homines novi*; and if not, how the anthropologies that the *homines novi* have made-shift for themselves square with the older anthropological paradigm still supported by their veteran colleagues.

Assuming then that ethno-racial difference be the most important axis of diversity –an assumption based on Gupta and Stoolman's systematic erasure of class, gender, sexual orientation and dis/ability from their address through phrases like “decolonization,” “BILPOC” and “white-norms” (re-read the aforegiven definition of white-norms and tell me if you can't find the *contradictio in terminis*)–, it behooves us to ask: “Is it possible for BILPOC to *do* anthropology as traditionally conceived? Could African-American Anthropologists, for instance, conduct an ethnographic study of an Anglo-American community, without the aim of exposing the community's errors or setting the community straight, but with the aim of understanding it in its own terms and ultimately, carrying-out a cultural critique of their own group on the basis of the insights garnered from the ethnographic study of another (so, a ‘Nacirema-Kcalb’ or a ‘Nacirema-Nacirfa’ Anthropology)?<sup>xxv</sup> More ambitious still, could African-American Anthropologists conduct an ethnographic study of a Native-American or Polynesian community with the selfsame purpose?” This is a dangerous question for such a project undercarries the implication that there may be something *wrong* with Black America (as the puritanic prohibitions on juvenile sexuality present in earlier phases of White America were categorically *wrong*). And *that* is a racist premise for, as Ibram X. Kendi (2019) teaches us, “To say that something is wrong about a racial group is to say something is inferior about a racial group [and] to say that

something is inferior about a racial group is to say a racist idea.” (*Obiter dictum*: Booker T. Washington, W.E.B. Du Bois and Thomas Sowell have all articulated suchlike ‘racist’ critiques of Black America, albeit not necessarily on the basis of long-term participant-observation among another socio-cultural group. See also Mondesire's critique [2022:3] of the notion of *The Talented Tenth* so central to W.E.B. Du Bois's self-image and life's work.<sup>xxvi</sup>) For my part, I own that, although I can see no reason why African-American Anthropologists could not or should not conduct scientific studies of other socio-cultural groups, I find it both impractical and improbable that they should subsequently turn this knowledge to the task of reforming Black America. African Americans –like all socio-cultural groups– ought to be understood, not reformed. The moment the anthropologist tries his hand at reforming a group, he announces his status as a moralist and renounces his status as a scientist. The moment he tries his hand at reformism he acts *ultra vires et facultates*. His words then carry no more weight than the layman's.

But *really* what are we playing at here?! Whether or not a *Nacirema-Kcalb* or a *Naidni* or an *Eekorehc* anthropology be possible, this is not the kind of anthropology that our *homines novi* have improvised amid their struggle to fit into the slot formerly occupied by the White Anthropologist. The subject (i.e. target) of their cultural critique is not their selves and their ilk – Black America, India or the Cherokee Nation– but as ever mainstream (i.e. White) U.S. American Society. If they do not see a fellow-traveler in Mead, the reason must be that, although the *subject of the cultural critique* remains the same, the object of the critique has undergone a transformation *entretemps*. Unlike Mead, they<sup>xxvii</sup> do not seek to make this *capitalist, racist, settler-colonist, militarist, imperio-nationalist* society better; or rather, the only way they can see to make it better is to undo it—to call it every dirty name in the book<sup>xxviii</sup> and berate those

communities and individuals who identify themselves with it.<sup>xxix</sup> If they cannot see Mead as a fellow-traveler, the reason must be that they have another moral compass (n.B. this is not to say their moral compass is *better* or *worse* than Mead's. It is only to say that both are in the business of moralizing and that, like most moralists, they have much to disagree about.). Among the articles of their faith, one is liable to find some version of Ta-Nehisi Coates' tweet: "There's nothing wrong with Black America that the eclipse of White America would not fix."<sup>xxx</sup>

If though the *subject of* BILPOC anthropologists' *cultural critique* tends to be mainstream U.S. American Society, what may we ask is the subject of their study? Admittedly, this question does not admit of a univocal answer. BILPOC anthropologists go about their work in many ways. Yet there is a clear *tendency* for BILPOC anthropologists to study their "own" ethnic, racial, social and/or cultural groups. To the extent that such self-study is rigged-out with rationalizations and justifications, it is said that, unlike their White colleagues, they do not have the *privilege* of transgressing the boundary-lines that separate social groups of their own volition and decision, without any other justification than their desire to have a look-see. Furthermore, it is implied, if not stated outright, that BILPOC anthropologists (qua BILPOC persons) control an empathetic and experiential wellspring which permits them peerless insights into BILPOC communities. While I cannot –and dare not– weigh the merits of such claims, it stands to reason that they are not adduced *apropos of nothing*. They are bids to *legitimize* auto-ethnographic studies and concomitantly, to *authorize* the attendant knowledge over and against one's non-BILPOC colleagues and contenders. They are signs of a metabasis in anthropological praxis from a *logic of alterity* to a *logic of ipseity*. They are signs of a « repli identitaire » (i.e. an identitarian backfolding).

## Conclusion:

By way of conclusion, I will simply recapitulate the central points that I have made in this essay. The discipline of U.S. American anthropology is in a state of crisis, one which has been simmering under the surface for sometime now and of which Gupta and Stoolman's presidential address is the most recent sign. This crisis is multifactorial: first, there is the unholy marriage of “cultural relativism and moral reformism” –or again, “scientific objectivity and moralistic subjectivity”– performed by Franz Boas and his students; second, there is the dissolution of anthropology's subject of study (“traditional communities”) by the global economy; third, there

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xx See Robert E. Park's “Human Migration and the Marginal Man” (1928). See also Chad Alan Goldberg's “Robert Park's Marginal Man: The Career of a Concept in American Sociology” (2012) for a thorough-going treatment of the concept's development since Park.

xxi The acronym BILPOC stands for “Black, Indigenous, Latine People of Color” (Gupta and Stoolman 2022:14).

xxii This proposal raises legal questions and ethical dilemmas. In California, where Gupta and Stoolman are based, the State Constitution has forbidden the State and its agents (incl. public universities) from “discriminat[ing] against, or grant[ing] preferential treatment to, any individual or group on the basis of race, sex, color, ethnicity, or national origin in the operation of public employment, public education, or public contracting” since 1996. Yet public universities have turned this constitutional article into a dead-letter by using diversity-discourse and other such machinations to accord preferments to BILPOC students and faculty. Thus, one finds the following statement among the minutes of the UCLA - Anthropology Graduate Student Association (AGSA) meeting for November 27<sup>th</sup> 2020, which Jessie Stoolman distributed to the grad-student listserv three-days later: “Admin can't use race/gender, but Indigenous folks not included under the Prop 209 ban, so there are ways to work around [it]; also the DEI contributions are a way to consider folks backgrounds while not violating Prop 209” (AGSA 03.27.2020). Without weighing-in on the morality of either obeying or disobeying a law like this, a law with a plebiscite behind it, it is always edifying to study how institutions and individuals *really* behave (cf. MacIver 1968:30). This enables one to “surmise the self-centered interests and prejudices that frequently lie back of the noble professions and high-minded principles so easily proclaimed by power-hungry leaders” (MacIver 1968:34).

xxiii Gupta claims that the place of pride that anthropologists accord to “the single-authored monograph” and, to a lesser extent, single-authored peer-reviewed articles is “purely arbitrary.” That it is “just a disciplinary practice” without any “real reason” to back it up. Gupta then points to the norm of multi-authored publications in the natural sciences (e.g. biology, physics, psychology, etc) to suggest that, as the contributions of co-investigators are often recognized by listing their names on the by-line, so anthropologists ought to recognize the authorial claims of our research subjects. The main thing that this line of reasoning misses is the changing *meaning* of book-hood and author-ship (or, better, authority) as one enters these other disciplines. Ethnographies, being descended from travelogues, have an artisanal quality foreign to much of contemporary science. Today the typical ethnography is a composite of ‘new’ data, ‘new’ concepts, ‘new’ analyses and diegeses, plus idiosyncratic justifications of the concepts and methods that one has taken-up from one's colleagues and equally idiosyncratic refutations of those concepts –though not methods, usually– circulating in the disciplinary ‘conversation’ that one disdains to take-up. It is as though each ethnographer endeavors to write *the* authoritative account of either the community under study (if he be in a modest mood) or human nature and culture writ large (if he be in grand mood). Once again, this mode of research and notion of authority differs *toto coelo* from the natural sciences as this quote from Kuhn (2012 [1962]:20) makes clear: “When the individual scientist can take a paradigm for granted, he no longer, in his major works, attempts to build his field anew, starting from first principles and



is the quixotic effort to carry-on doing “ethnography as usual” in industrial and post-industrial societies; fourth, there is the changing demographic composition of these societies, whereof our faculty is a piece, due in part to neoliberal governmentalities (Gerstle 2022); and fifth, there is the divergence of majoritarian (“White”) and minoritarian (“BILPOC”) ways of doing ethnography—the matter of choosing a *subject of study* and, since we are still thrall to Boas's moral reformism, a *subject of critique* when the ethnographer is purveyed with this or that *personal identity*—in these (post-)industrial societies, a divergence that to-shakes and to-breaks the last bond keeping our discipline together. More indeed than a divergence, what we have here is a cross, whereby majoritarian researchers persist in choosing their field sites according to a *principle of alterity* even as minoritarian researchers insist on choosing their field sites according

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justifying the use of each concept introduced. That can be left to the writer of textbooks... Today in the sciences, books are usually either texts or retrospective reflections upon one aspect or another of the scientific life. The scientist who writes one is more likely to find his professional reputation impaired than enhanced. Only in the earlier, pre-paradigm stages of the development of various sciences did the book ordinarily possess the same relation to professional achievement that it still retains in other creative fields.” The existence of hundred-author articles in the natural sciences is a testament to the *collaborative* nature of the natural sciences—to the fact that they work as a team on common data-sets and problems with common techniques. The proposition that ethnographies be written by committee (or, *ceteris paribus*, the research community) is destined to diminish our sense of grandeur without bringing us one inch closer to the “science of man” that our discipline purports to be. A more radical proposal may be to stop collecting data like so many magpies a-gathering baubles and instead read the ethnographic record, not as a book or even as a library, but as an archive to aid us in the formulation and formalization of a modal theory of human nature and culture under certain variable conditions (e.g. plague or prosperity). This is not a call for another discussion group. It is a call to stop our ethnographic excursions—to stop sending one graduate-student cohort after another off to “do the field”—and redirect our collective energies to the systematic revision of the ethnographic record, so that we might determine what if anything we have achieved since Boas. The proceedings of the new anthropological research teams may be null-authored or multi-authored equally for these are ultimately one and same. Night has set on the age of exploration.

xxiv Despite having chosen to speak of “structural contradictions” above, my thought on this head is highly indebted to Bateson and his notion of the “double-bind situation” (1969). Briefly, a “double-bind situation” is one where an individual receives two opposing injunctions, e.g. “do X” and “do *not* X,” via two channels of communication so that they cannot be arrayed as a simple contradiction. The victim of a double-bind situation has the feeling that “no matter what he does, he cannot win.” For instance, the BILPOC anthropologist may receive the message that a *real* professor must be prepared to speak *on* any subject at a moment's notice (« S'il n'est rien dont il ne puisse parler, lutte des classes ou inceste, c'est que sa situation, sa personne et son personnage impliquent la < neutralisation > de ses propos... » [Bourdieu and Passeron 1970 :136]) even as he receives *another* message on *another* wave-length, viz. professors like him (i.e. BILPOC professors) are *especially well-equipped* to speak *on* matters of race and ethnicity, *for* their ethno-racial communities, and *from* the standpoint of their ethno-racial identities. Inversely, the White anthropologist may receive the message that, if there is one thing on which he cannot hope to speak with any measure of authority, it is the matter of race and ethnicity (incl. both the abstract logics of racism and the lifeways of any ‘racialized’ groups), something which is mighty inconvenient since Anthropology was purposed to be the study of precisely these populations.

to a *principle of ipseity*. The predictable result is that the two factions should enter into competition over the same research communities and, what is more, competition for the right to make *authoritative* statements *on* –or, *mutatis mutandis*, *on behalf of*– these research communities. The tension is thick. The pressure is high. And it is apt to erupt into recriminations with the former accusing the latter of contributing to the silencing of intra-communitarily marginalized voices through their posturing as community-spokesmen (cf. McDonald 1988; E. Ardener 1975)<sup>xxxi</sup> and the latter accusing the former of ignoring community concerns, exposing community secrets to the “white gaze,” and treating the community as a means to its own selfish ends, be they scientific or moralistic, no matter. It was in this spirit that one of the BILPOC graduate students at UCLA characterized the discipline's history in these terms: “It is as though anthropologists are running a science experiment and people of color are the mice.”<sup>xxxii</sup> The crisis induced by the current communitarian and identitarian zeitgeist casts a pall over Anthropology's history, but it also, paradoxically, shines a new light on other aspects of the same. Amid this chiaroscuro, amid this play of light and shadow, we have begun to discern our discipline's dilemma: to wit, Anthropology must either radicalize its scientific commitments (e.g. “cultural relativism”) or it must radicalize its moralistic commitments (e.g. “antiracism”), but in no case

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xxv While Carolyn Rouse (2021:336) has spent several years studying low-income, rural, elderly White Californians, her critique does not quite fall in this category (i.e. the Nacirema-Kcalb category). It rather amounts to the following: “despite having benefitted their entire lives from American biopolitics, in the summer of 2016 my interlocutors explained that they were voting for Trump because they felt aggrieved by Black and Brown people who they claimed took their jobs—[menial] jobs they also told me they didn't want [to keep].” Thus her hackneyed critique is that her research subjects ‘vote against their own economic interests’ due to misplaced racial resentment. (Never mind that Rouse acknowledges a few lines earlier that her interlocutors' fathers had experienced “almost no job competition from people of color, immigrants, or women” during the mid-twentieth century. That is to say, the *positive* bio-policy of limiting –yea, excluding– immigrant labor during the mid-twentieth century benefitted their bottom line, if not necessarily the bottom line of business interests; while the *negative* bio-policy of unlimited immigration harmed their bottom line by placing them into competition with immigrant labor for working-class jobs.) This differs both from the traditional anthropological research of exotic others (“Look at their cultural achievements! Look what they can teach us!”) and the more modern anthropological critique of endogenous others (“Look how people make-do under conditions of social discrimination and disorganization! Look how even *prima facie* misbehavior may be rationalized as a response to structural constraints!”). Rouse's critique is more along the lines of “look how entitled and undeserving these people are!” Which needless to say makes for quite the change of tack!

can it continue to be, as it has been since Boas, a moralistic enterprise profiteering from its pretension to scientific objectivity. After this decision, we will have other decisions to make: most notably, the decision concerning the fate of ethnographic research and reportage. These decisions cannot be made by anyone person; they can only be made by the collectivity of professional anthropologists. To lay my own cards on the table though, I hold that we should fête Boas, the scientist and eat Boas, the moralist. And that we should surcease our ethnographic

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xxvi“Progress in human affairs is more often a pull than a push, the surging forward of the exceptional man, and the lifting of his duller brethren slowly and painfully to his vantage-ground” (Du Bois 2007 [1903]:67).

xxviiThis is a generalization. It is obviously not that case that *all* BILPOC anthropologists engage in ‘cultural critique’. Neither must they critique the United States *ex necessitate*. Yet there is a pronounced tendency to critique the United States among those that do ‘cultural critique’ for the force of the intellectual’s critique lies in its “actual or potential nuisance value” (Schumpeter 2008 [1940]:147). As flees bite the dog on which they ride, intellectuals critique the societies of which they reside; and the United States is still the biggest dog on the block.

xxviiiPlease do not pretend that these epithets are ‘neutral’ or ‘objective’ descriptions of the society. They have the same rhetorical flavor as the word ‘socialist’ in the mouth of a Republican.

xxixNotwithstanding the facial antisemitism, Nietzsche’s remarks (1999 [1886]:267) on the ambi-valuation of the master morality into the slave morality remain indispensable here. « Die Juden sind es gewesen, die gegen die aristokratische Werthgleichung (gut = vornehm = mächtig = schön = glücklich = gottgeliebt) mit einer furchteinflössenden Folgerichtigkeit die Umkehrung gewagt und mit den Zähnen des abgründlichsten Hasses (des Hasses der Ohnmacht) festgehalten haben, nämlich „die Elenden sind allein die Guten, die Armen, Ohnmächtigen, Niedrigen sind allein die Guten, die Leidenden, Entbehrenden, Kranken, Hässlichen sind auch die einzig Frommen, die einzig Gottseligen, für sie allein giebt es Seligkeit, — dagegen ihr, ihr Vornehmen und Gewaltigen, ihr seid in alle Ewigkeit die Bösen, die Grausamen, die Lüsternen, die Unersättlichen, die Gottlosen, ihr werdet auch ewig die Unseligen, Verfluchten und Verdammten sein! » So too Chamfort’s remarks –which Nietzsche likely encountered in quotation apud Schopenhauer– on the ‘conspiracy’ of the stupid servants against the wise masters: « À voir le soin que les conventions sociales paraissent avoir pris, d’écarter le mérite de toutes les places où il pourrait être utile à la Société, en examinant la ligue des sots contre les gens d’esprit, on croirait voir une conjuration de valets pour écarter les maîtres. »

xxxI encountered this tweet in quotation *apud* John McWhorter. McWhorter gives two versions of the same tweet though, one the phrase, the other the paraphrase, presumably. Here is the other version: “There’s nothing wrong with black people that the complete and total elimination of white supremacy would not fix” (Coates apud McWhorter 2021).

xxxiTo their credit, Gupta and Stoolman (2022:11) acknowledge that “communities are not singular, nor do they speak with one voice” despite what the term ‘community’ may imply. Unfortunately, this acknowledgement of intra-communitary power-relations comes across as a moment of recognition, soon forgotten amid a larger call for more community-work. They do not persevere on the topic. They do not consider how the kind of community-investment they counsel, esp. advocacy on behalf of the community in academic and political contexts, two things which compel the anthropologist to align with the community’s apex or center rather than its base or periphery, might further silence the already hushed voices of the “marginal men” that anthropologists so long found to be the most insightful informants. They do not consider how a new norm of community-authorized ethnographies might affect the reputability of ethnographies that rely on these muted voices and that, by virtue of this reliance, cannot receive the community’s –that is to say, the community-leaders’– seal of approval. The overwhelming majority of Gupta and Stoolman’s remarks suggest that anthropologists *ought* to take their cues (incl. cues for research topics) from the community and, furthermore, bow to community orthodoxy when analyzing the ethnographic record (see endnote V).

excursions forthwith.<sup>xxxiii</sup>

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xxxii However just or unjust this statement may seem to the reader, one must bear in mind that anthropology was constituted not as an experimental science but as an observational one. While one may thus read of mice being sacrificed (i.e. killed) and harvested for histological samples in medical journals (e.g. “After 5 min, 1, 24, or 72 h, mice were sacrificed and blood, lungs, liver, spleen, kidneys, and bone marrow removed.” [Eggenhofer et al. 2012]), I venture that one would be hard pressed to find comparable statements in anthropological journals.

xxxiii This is not a call for a return to “armchair anthropology.” Swivel chairs are more comfortable. See endnote XXIII.

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