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# The *Hau* of the Article and Dividual Authors

## Reimagining Authorship in Anthropology

**Abstract:** Despite repeated calls for change, social and cultural anthropology is still dominated by single-authored works. I consider two thought experiments that might disturb the status quo in interesting ways. Anthropologists could publish anonymously, treating ourselves in the same way as we treat our anonymised informants, for example, using pseudonyms. Alternatively, we could treat our colleagues in the field not only as equals but also as co-authors. Both these options have implications concerning the 'dividual' author (perhaps now thought of as an 'auth'), and involve rethinking the '*hau*' of publication.

**Keywords:** authorship, collaboration, informants as co-authors, pseudonyms, thought experiments

Anthropologists love ambiguous, vague or unclear terms: often they are what we analyse. We also hate them as unfit for use in analysis. Some anthropologists have taken seriously calls to collaborate in anthropology. In social anthropology, at least, such calls are not reflected in published works, which continue to have single authors. Scholarship increasingly shows that 'author' is one of those 'weasel words' crying out for anthropological analysis: a polythetic term hidden in plain sight, needing analysis along with 'collaboration'.

This article presents initial thoughts about how the complex processes of research are belied by the formal structures of authorship. Two extreme thought experiments are used to explore possible ways of managing authorship in anthropology. In different ways, these illuminate the title. Just as Melanesian scholarship has shown how the western 'individual' is not necessarily the atom of human society (through the idea of the 'dividual'), so too authorship may have to be rethought to allow more granularity, including sub-divisions of the very idea of what constitutes an author. With an attribution as author goes responsibility and what comes is credit (for junior academics this is more important than royalties). This has a parallel in long-established discussions of the *hau* of the gift, and whether a 'free gift' is possible. If the *hau* of the gift 'demands' a counter-gift, the *hau* of the anthropologist author seeks tenure! As we shall see, some forms of authorial pseudonymity can accommodate this.

In October 2021, the European Association of Social Anthropologists produced guidelines for managing collaboration between anthropologists. Published in the EASA newsletter, they address issues raised by anthropologists working in research teams whose individual members control neither the work they do nor the use of their results. These issues emerged from a wider concern, shared by all academic



disciplines, about precarity in universities. Although helpful, the guidelines' reach is limited. They focus on groups of unequal researchers but overlook non-academic collaborating partners. This article concentrates on such collaborations, which occur across various divides, including those of nationality and economic and political privilege.

Soon after those guidelines appeared, in December 2021 a group of articles on the use of pseudonyms for collaborators/informants/research participants in anthropology was published online on the *American Ethnologist* website (see Weiss and McGranahan 2021). In her individual contribution to this collection, Erica Weiss points out that pseudonymisation is a form of 'anti-citation' (2021). There is a power dynamic implicit in the distinction between authors (named and cited) and those once called informants (unnamed/pseudonymised and uncited).<sup>1</sup> And in a different context, Diane Duclos discusses the calls for recognition (being named) by the artists she worked with and the tensions between this and presumptive anonymity as mandated by codes of conduct and ethics regimes (2019: 179).

Erica Weiss and Carole McGranahan and their contributors consider the many and various problems with more or less default assumptions that informant names should be removed or replaced. However, none consider the possibility that pseudonymisation could start at home by pseudonymising or removing the names of the authors as well as those they have talked with! This possibility is considered in one of the thought experiments below.<sup>2</sup>

Before examining authorship in anthropology, I briefly consider the position in other academic disciplines.

## Academic Authorship

The complex and fascinating history of authorship cannot be explored here.<sup>3</sup> As well as work on the development of copyright and the ownership of rights, much research has been done, especially in science and biomedicine, on ways to conceptualise the individual contributions to an article made by multiple authors. This is demonstrated by the rise of hyper-authorship in physics (Chawla 2019). Often said to have started in particle physics before spreading to other fields, the number of academic articles with more than 1,000 authors has more than doubled in the past five years (Adams et al 2019). In 2021, the record was held by an article with 5,154 authors, published in *Physical Review Letters* in 2015, 24 of whose 33 pages are devoted to the authors' names and affiliations (Castelvecchi 2015).

As a consequence of this trend, taxonomies have been developed to model the idea of authors being multiple and disparate 'contributors' to an article. The best known is 'CRediT' (Allen et al 2014, 2019; Brand et al 2015), a contributor-role taxonomy adopted by numerous publishers, including Elsevier (see Table 1), and formalised as an ANSI/NISO standard (Z39.104-2022) in February 2022. Under this framework, anyone who added something to the project is now treated as a contributor; some variants not only distinguish authors from contributors but also add a third category of 'guarantor', who assumes overall responsibility for the publication.

**Table 1.** CRediT: contributor role taxonomy (adapted from Brand et al 2015: 153)

Term	Definition
Conceptualisation	Ideas; formulation or evolution of overarching research goals and aims
Methodology	Development or design of methodology; creation of models
Software	Programming, software development, etc.; design of computer programs; implementation of the computer code and supporting algorithms; testing of existing code components
Validation	Verification (either as part of the activity or separately) of the reproducibility of experiments/results and other research outputs
Formal analysis	Application of statistical, mathematical, computational or other formal techniques to analyse or synthesise study data
Investigation	Conducting a research and investigation process, specifically performing the experiments and/or data/evidence collection
Resources	Provision of study materials, reagents, materials, patients, laboratory samples, animals, instrumentation, computing resources, or other analytical tools
Data curation	Management activities to annotate (produce metadata), scrub and maintain research data (including software code, where necessary to interpret the data) for initial use and later re-use
Writing: original draft	Preparation, creation and/or presentation of the published work, specifically writing the initial draft (including substantive translation)
Writing: review and editing	Preparation, creation and/or presentation of the published work by those from the original research group, including critical review, commentary or revision, both before and after publication
Visualisation	Preparation, creation and/or presentation of the published work, specifically visualisation/data presentation
Supervision	Oversight and leadership responsibility for planning and executing the research activity, including mentorship external to the core team
Project administration	Management and coordination responsibility for planning and executing the research activity
Funding acquisition	Acquisition of financial support for the project leading to the publication

Elizabeth Gadd (2020) was concerned about the CRediT framework, including its potential to embed the status quo around output-based evaluation and the negative systemic effects this might produce. Moreover she asked: ‘I wonder whether single-authors, if called upon to strictly adhere to CRediT, would find themselves obliged to list others as “contributors” (librarians maybe?) where historically in their disciplines they might not do so’ (2020: np). The question reveals the contradiction, especially in the humanities and adjacent disciplines such as social or cultural anthropology,

between the recognition that ‘our creative practices are largely derivative, generally collective, and increasingly corporate and collaborative’ and the fact that we continue to ‘think of *genuine* authorship as solitary and originary’ (Jaszi and Woodmansee 2013: 195; emphasis in original). If individuation engenders an impoverished view of knowledge production (McSherry 2013) then how might we think differently about authorship in anthropology if we moved beyond it? As the argument on personhood has taught us, we may need to think of dividuals rather than individual people, so perhaps the CRediT framework is a version of a parallel argument, decomposing unitary authors into dividual contributors (possibly ‘auths’)?

### Authorship in Anthropology

Although co-authorship is increasingly common in social sciences, anthropology – or at least social/cultural anthropology – appears to buck this trend. Analysing patterns of collaborative publishing in the social sciences between 1930 and 1990, Nicholas Babchuk and colleagues (1999) found that co-authorship in anthropology journals was considerably lower than in other fields. Although a more recent bibliometric analysis found that the median number of authors of anthropology articles had increased from one to two by 2013, this is probably because the analysis included all anthropology journals listed in the Social Sciences Citation Index, covering subfields such as archaeology and biological anthropology, where multiple authorship is the norm (Henriksen 2016). In other words, North American four fields anthropology includes disciplinary sub-fields in which co-authorship is common and those in which it is not, yet it is in the latter fields that calls for collaboration are common. My analysis of research articles published in 1999, 2009 and 2019 in three key anthropology journals (*American Anthropologist*, *Current Anthropology* and the *Journal of the Royal Anthropological Institute* – excluding this journal as similar to JRAI in scope and continental base) suggests that sole authorship remains the preferred mode in the discipline (see Table 2), at least in periodicals favouring social or cultural anthropological content. Over this twenty-year period there is no evidence of change.<sup>4</sup> The pattern is even more pronounced in books by anthropologists: for example, approximately 90 percent of anthropological monographs published by the University of Chicago Press in 2019 were single authored (see Table 3).

It may be objected that by concentrating on ethnographic publications I am missing some of the most important work, since many of the most influential books in

**Table 2.** Authorship in major anthropology journals 1999–2019

	Year			Total (n)
	1999 (n)	2009 (n)	2019 (n)	
Single author	77% (56)	79% (89)	81% (125)	79% (270)
Multiple authors	23% (17)	21% (24)	19% (29)	21% (70)
Total	100% (73)	100% (113)	100% (154)	100% (340)

**Table 3.** Authorship in Chicago University Press anthropology monographs over time (based on publicly available data downloaded from the University of Chicago Press’s website under the subject heading of anthropology)

	Year			Total (n)
	1999 (n)	2009 (n)	2019 (n)	
Single author	95% (19)	91% (44)	89% (28)	91% (91)
Multiple authors	5% (1)	9% (4)	11% (3)	9% (8)
Total	100% (20)	100% (44)	100% (28)	100% (99)

social anthropology have been co-edited collections. There is a discussion to be had about edited collections but it is the chapters within them and the introductions that garner the influence,<sup>5</sup> and it seems not unreasonable to describe them as building on ethnographies, which as we have seen tend to be solo authored. This pattern is typically attributed to the prominence of ethnographic fieldwork in social anthropology, which ‘may work against collaborative activity’ (Babchuk et al 1999: 6). This view has been extensively challenged in feminist, indigenous and postcolonial critiques of ethnographic methodologies and the attendant rise of what are known as ‘collaborative’ or ‘reciprocal’ ethnography (see Boyer and Marcus 2021; Gay y Blasco and Hernández<sup>6</sup> 2019; Lassiter 1998, 2001, 2005; Lawless 2019). However, while produced collaboratively, it is rare for published ethnographies to be formally co-authored,<sup>7</sup> except for works dealing with the life histories or perspectives of individual informants (for example, diary texts, such as Bizarro Ujpán and Sexton 1985). The rarity of co-authorship is visible even in the journal *Collaborative Anthropologies* which, despite its stated aims, publishes few multi-authored articles<sup>8</sup> A notable recent exception is the monograph *Phone & Spear: A Yuta Anthropology* by Miyarrka Media (2019), authored by an anthropologist Jennifer Deger and Yolŋu community members Paul Gurrumuruwuy, Enid Gurunulmiwuy, Warren Balpatji, Meredith Balanydjarrk, James Ganambarr and Kayleen Djingadjingawuy. They note, ‘what makes our anthropology *new* is that *Phone & Spear* has been co-authored and co-designed with the explicit ambition of claiming and reconfiguring anthropology’s relational potential’ (Miyarrka Media 2019: 16; their emphasis).

That collaboration so rarely extends to the authorship of ethnographic texts is noteworthy, particularly given the ‘relational potential’ highlighted by Miyarrka Media (2019), and the dependence of ethnographic knowledge on the strength of our relationships with our interlocutors and their insights. As Paloma Gay y Blasco observes:

ethnography depends on the creation of a distinctive authorial self who provides an innovative perspective on the discipline and the world . . . [T]his self has consistently been constructed as singular, with dialogic claims often working to shore up rather than undermine the anthropologist’s agency and control over both argument and representation. The generic ways this singular self is produced and gains legitimacy . . . is difficult to reconcile with the reciprocal ideal of full, nonhierarchical involvement of collaborators in the production of the text. (2017: 94)

Furthermore, doctorates are awarded to individuals, so by definition it seems that a doctoral thesis cannot be co-authored.<sup>9</sup> It also relates to writers' professional ambitions: ethnographic texts are aimed primarily at academics, far and remote from most occupants of stereotypical anthropological fieldsites (Lassiter 2001). Although reviewers queried this statement as being old-fashioned and no longer applicable, I think it does hold, since even when studying in developed nations most academic writing seems offputtingly far and remote from those outside academe in ways that may not translate to distances in kilometres. Simultaneously, Gay y Blasco (2017: 95) points out that the 'arcane' bodies of academic knowledge in conjunction with 'publish or perish' imperatives, are often of little interest to research participants. Similarly Veronica Strang's 2006 article in *Current Anthropology* argues for a collaborative understanding of anthropological research. She argues that theoretical developments should be seen as co-authored rather than extracted. Commenting on this, James Fairhead echoes Gay y Blasco: 'it may be that we should acknowledge fully the broader joint authorship in the development of anthropological theory and forms of analysis, but can we be sure that people will agree to be our co-authors? There is a tension here between co-authorship and co-optation that needs to be addressed, and this will need to be done in institutional transformations, not simply modes of intellectual recognition' (2006: 994–995).

Introducing *Collaborative Anthropology Today*, Dominic Boyer and George Marcus attribute the persistence and dominance of single authorship in anthropology to the influence of audit culture in academe and the importance of publications for young researchers seeking jobs. They note that 'it continues to be the case that dual (or more) research and authorship remain basically unthinkable from the point of view of establishing the requisite scholarly credentials to begin a professional career in anthropology' (2021b: 6).<sup>10</sup> The Ethnographic Terminalia Collective (ETC) addresses this, saying 'Over the years of working together we have grappled with questions such as: Whose curatorial vision is brought to life? Who supports this vision by completing the mundane yet necessary administrative tasks? Who should the members of the collective be, and how is this decided? Who co-authors? Who is a first-author?' (ETC 2021: 87). It is when anthropology overlaps with art (as with ETC) or with film that the most explicit attempts have been made to address questions of authorship, rather than in more mainstream social anthropology, which persists with the single authorial voice, sometimes despite its own calls for collaboration. Indeed, although the key text of the 'Writing Culture' debate was co-authored (Clifford and Marcus 1986), some of the most celebrated texts associated with it that called for 'dialogic' and 'multivocal' ethnographies were solo authored (see, for example, Crapanzano 1980).

In order to focus discussion of authorship within social/cultural anthropology (dominated by sole authorship), consider two thought experiments at contrasting extremes. Reflecting on them, perhaps they are not as extreme and unworkable as it may first seem.

### Thought Experiments: Rethinking Authorship in Anthropology

The following thought experiments explore two different ways of dealing with anthropologists in publications in the same way as those referred to in the literature as 'infor-

nants'. It is surely a good ethical starting point to treat ourselves in the same way as we would treat our fieldwork colleagues. The two extreme possibilities: first, to name no one as author and second, to name everyone who has participated in the research. I note that Akhil Gupta suggested these alternatives in 2014 in the context of a discussion of how anthropologists should recognise the work of research assistants in their publications.

### Thought Experiment 1: Publish Anonymously/Pseudonymously

This thought experiment supposes that publications are anonymous, under a title only or with pseudonymous authors. *Contra* Michel Foucault (1998), there is a long history of anonymous publication in both literary and scientific texts. Robert Griffin, writing about the former, notes that 'The motivations for publishing anonymously have varied widely with circumstances, but they have included an aristocratic or a gendered reticence, religious self-effacement, anxiety over public exposure, fear of prosecution, hope of an unprejudiced reception, and the desire to deceive' (1999: 885). As Mary Terrall (2013) documents, early modern scholars also used anonymity to hide their gender (especially female scholars) or their nobility, or as a means of advancing controversial ideas that would have been penalised within the academy.

Indeed, there is a long history of anonymity in radical texts, both literary and scholarly. Nicholas Thoburn (2016), among others, observed that in both France and the United Kingdom injunctions against anonymity were indexed, not to a regime of property, but to a regime of regulation, as a deterrent to transgressive discourse. For Marx, anonymous authorship was 'an anti-capitalist textual form' (2016: 175).<sup>11</sup>

When dealing with research data, the difference between anonymity and pseudonymity is profound (see for example, Barker 2016), but for the purpose of this thought experiment they collapse into being near synonyms. It might be possible to deal with a publisher consistently under an anonymous mask but this would be difficult. Even if the process of review and revision were satisfactorily completed, the final problem would be signing the formal agreement to publish. Even if an article or monograph is to be open access under a Creative Commons licence, someone (a legal individual or set of individuals) has to assert ownership in order to repudiate it! In effect, the model discussed is one of pseudonymous publication since the publisher will know the identity of the author (just as the *Social Anthropology/Anthropologie sociale* editors know the 'legal' identity of this incarnation of Luther Blissett). This also has particular relevance to maintaining ethical responsibility, where if publications shelter behind pseudonyms then the publisher will be the primary target for the objections mentioned below. This may well be a downside to the proposal – one could imagine that publishers would be reluctant to take on this responsibility and would be told by their lawyers to implement a bureaucracy of disclaimers whereby authors have to accept full responsibility for their words. But, in fact, this is already the case!

One precedent for pseudonymous publication in anthropology is John Doe's book *Speak into the mirror: a story of linguistic anthropology* (1988). In the preface, 'Doe' explains his (the author was later revealed to be male) decision as follows:

The name ‘Mercedez Benz’ {sic} has sold a lot of cars and garnered a great deal of prestige. In like fashion, the names ‘Franz Boas’ and ‘Ferdinand de Saussure’ have sold a lot of anthropological and linguistic ideas, and earned sizeable reputations. And there is something unfortunate in that . . . Cars contain hidden workings that sellers know about and that buyers want to know about. . . [B]uyers need to be able to trust the word of the sellers. But anthropological ideas should not contain hidden workings unavailable to readers. There should be nothing between – or outside – the lines that readers cannot become aware of. In the realm of ideas, unlike the realm of cars, you should be able to see what you get. (1988: preface)

He also argues that

authorly names should be suppressed for reasons on two fronts: First, names should be suppressed for fear that ideas will be accepted or rejected because of the names of those who write; and, second, names should be suppressed because those who write about others ought not be celebrated for themselves. (1988: preface)

Pseudonymising removes personalities and reputation (individuals or institutions); issues of gender, age and race neutrality follow quite naturally. Moreover, anonymity may also provide additional protection for our interlocutors, especially where ethnographers are studying illegal activities and risk being subpoenaed to give evidence about their research, being pressured to name their informants. As John Lowman and Ted Palys (2014) discuss, researchers have no legal protection to maintain confidentiality: in such cases they may be forced to choose whether to break the law or promises of confidentiality. Author anonymity can also protect informants whose identification would cause social rather than legal risks, which is why the anthropologist Sjaak van der Geest used a pseudonym when writing about his early research on sexual relationships in a rural Ghanaian town. Decades later, he explained the reasons for his decision:

When I started to write up the data from my first and second fieldwork experiences, I discovered the awkwardness of my promise of confidentiality. . . I soon realized that in this case such a measure would be an insufficient guarantee of confidentiality. Ghana’s academic community is like a village. Through my (the author’s) name it would be simple to trace the identity of the town and consequently of the informants. (van der Geest 2003: 15)

As ‘Wolf Bleek’, van der Geest published numerous articles on his fieldwork, although he highlights the way it inhibited his capacity to share his work with the community: ethical trade-offs were involved.

It has been suggested to me that anonymity or pseudonymity are sufficiently different that they should be discussed separately. An obvious parallel is with anonymous data in contrast with pseudonymised data, where the key to who the individuals/places/businesses are has been retained but is not widely/publicly shared. But this breaks down since the author(s) knows what they have written so true anonymity is not possible! The point of authorial pseudonyms is that they enable readers to find a set of work by the same person, to follow the development of their work over time, in a way that dealing with a practically infinite set of ‘Anon’ authors would make it hard to do.



Anonymous or pseudonymous authorship also provides protections to researchers who are ‘studying up’ (Nader 1969), especially those who risk censorship or censure by high-status and powerful groups. David Mosse (2006) and Edward Simpson (2016) have written sensitively about the political, legal and ethnographic complexities of working in such contexts. Simpson highlights anthropology’s inability to grapple with the implications of ‘the age of objection’. He notes, ‘Objection has yet to be embraced, solicited, or understood to signal the possibility that anthropology needs to be done differently’ (2016: 115). Anonymous publication is not a licence to say whatever we like, nor a means to avoid our responsibility for harms perceived to be caused by our representations. Instead, we should value its potential to address the very real dilemmas raised by Mosse and Simpson about research contexts in which powerful groups engage in ‘a deliberate and strategic attempt to control what is publicly known about them’ (Simpson 2016: 125) and where defamation proceedings might otherwise ‘rule out the possibility of such ethnography’ (Mosse 2006: 951). Some early readers have raised concerns about whether this model gives anthropologists the licence to say whatever they like, using anonymity to evade problems of ethics and accountability. In response, I would say that anonymity or pseudonymity may provide some shelter but not so much as to license unaccountability. The publishers may become the first recipients for objections, but liability will remain that of the authors, so the limit of an author’s pseudonymity, in principle, lies in the courts. If unambiguous authorial responsibility is an advantage of single author publishing then this is not removed by the use of pseudonyms.

Finally, just as pseudonymous authorship does not remove legal and ethical responsibility, it should be noted that pseudonymous publication need not deprive researchers of credit for their work (and hence jobs and grants). Authorship could be noted in confidential vitae, with certification from publishers to prevent spurious claims being made. Although not available online for all to see, the authorship of this article will feature on my CV, but not in my h-index score. However, Luther Blissett now has an ORCID id number, which will help establish their own h-index in time.<sup>12</sup>

## Thought Experiment 2: Interlocutors as Co-Authors

Rather than anonymising authors, now consider the opposite: anonymising no one. Anthropologists have long discussed whether informants should be anonymised, while respecting their ‘voices’, often leading to extensive verbatim quotation. This practice ignores the copyright issues. Few anthropologists recognise that under western copyright law everyone owns their words.<sup>13</sup> Spoken or written, words belong to their speaker whether ownership is asserted or not, and that ownership is not removed by removing names. Anonymising informants makes their words into ‘copyright orphans’, in which copyright exists but the owner is hard to trace. This makes the use of such quotations legally questionable.<sup>14</sup> It may be surprising that none of this was mentioned in the *Writing Culture* debates, but perhaps awareness of the problematical implications of copyright has followed on the heels of the development of online publishing subsequent to *Writing Culture* (Clifford and Marcus 1986).

Anonymising informants may be mandated by institutional ethics procedures, but this can have ethically questionable entailments, such as robbing material of probative value in court since, by definition, it cannot be linked to the people concerned – for example, in a land claim (see Barker 2016).<sup>15</sup> As Alexandra Murphy and colleagues note in *Annual Reviews of Sociology*, ‘where the risk associated with being named is low, and where participants view seeing their name in print as a benefit, naming may be more ethical than masking’ (2021: 50). Furthermore, anonymisation bestows power on the researcher who writes using material provided from now anonymous ‘others’. Hence, anonymity may undercut, rather than meet, our ethical responsibilities. Nancy Scheper-Hughes observed that anonymity ‘makes us unmindful that we owe our anthropological subjects the same degree of courtesy, empathy and friendship in writing as we generally extend to them face to face in the field where they are not our “subjects” but our boon companions without whom we quite literally could not survive’ (2000: 129). In a critique of anonymisation, Katja Guenther says that ‘by guaranteeing their confidentiality, I was in effect denying my respondents the right to be heard; in renaming them through the use of pseudonyms, I was denying them the basic right to be who they are’ (2009: 414). Following this, Michelle Brear points out that ‘Using pseudonyms may undermine the human right to freedom of expression, which includes the right to impart information and ideas . . . This right is denied when a named author/s “writes up” and attributes another’s ideas to a pseudonym because it shifts ownership of and recognition for the ideas to the author’ (2018: 723). Her more general point is that ‘The power to name and represent research participants is exactly that – power. This power is vested primarily in White people and is constituted within the academic system’ (2018: 738). A reviewer has raised a further point about the power of a co-author to say no. What if we work with people who disagree with us not only about subtleties of analysis but about the whole analytic frame? The example given was whether an ethnographer of political extremism, for example, would want to give their informants the power of co-authorship in what is written about them? It also has great relevance when studying up.<sup>16</sup> When talking about power imbalances, it is too easy to assume that anthropologists are powerful and their informants not. We should not make such blanket assumptions. Those written about may be powerful and object to their characterisation in quite different ways from ways in which powerless ‘others’ may object.

All this sits uncomfortably with what Nancy Scheper-Hughes says in the quote above. I am not going to pretend to have a simple answer here, but it seems to me we can have a clear normative statement from which exceptions may be made. Just as when going through ethics review one may have to argue that, for example, when studying political extremism (be it on the far right or left), as well as in some cases when studying up, prior informed consent cannot be obtained since they are so hostile to academic researchers, yet it may still be ethical to undertake research. (Moreover, it may be unethical *not* to undertake the research.) In such cases, it may be impossible for them to be co-authors. This clearly points to practical limits to how far the model can be realised, but, to reiterate the point, such exceptions need not obviate the norm.

Both the legalistic concerns about copyright and the general ethical injunction to take our moral responsibilities to others seriously lead to the ethical proposition

to accept our principal informants as co-authors, with all the legal and administrative consequences that this entails. Most importantly, this corrects some of the moral imbalances pertaining to the ethnographer's power to represent the lives of others. Adding named co-authors may be a small step towards addressing the structural violence identified by Rosemary Coombe (1998) as she asks, in effect, 'who has the right to write?' More recently, Paul Farmer (2016: 280) also raises the violence (symbolic and structural) implicit in the representation of others, which collaborative authoring can be a small step towards addressing. To some extent, adding co-authors exemplifies one of Victoria Reyes' three models for transparency (2018). It can be a positive response to Abhilasha Karkey and Judith Green, who ask: 'Could collaborating on writing with the hosts and including disparate reviews have enriched the analysis . . . Should there have been other co-authors, if the paper appears to have emerged from a collaborative research endeavour?' (2018: 496). Moreover, it provides a way of simultaneously acknowledging and protecting sources: publications may still mask who made a specific statement, but quotations are now the words of one of numerous (although not directly attributable) co-authors. Third, archiving of source material ('research data') is either unchanged (with embargoes and anonymisation to protect sensitive data) or facilitated since copyright matters will (have to) be made explicit early on: along with authorship goes ownership and possible archival donorship. However, under such a multi-authored framework, 'authorial responsibility' must be reconsidered, a concept whose lack of clarity has been made increasingly apparent by the issue of hyper-authorship. Admittedly, few of the potential authors may be interested in the niceties of academic gameship (indeed, the authors may not share ontologies). The academics among the authors would therefore presumably have to manage most of the burdens of writing, production and revision after peer review in ways that may be little changed from the present. They may take on the role mentioned in the discussion of the CRediT framework above, acting as 'guarantor' – which also addresses the worry about ethical responsibility. However, the most general potential problem with an attempt to realise this suggestion in practice may be that some but not all of our collaborators may want to be named as co-authors, others may consent to participate in the research but not to be named as co-authors, seeing no point in this 'game of names'.<sup>17</sup>

## The *Hau* of Publication and the *Hau* of Anthropological Products

These thought experiments represent very different responses to questions of authorship, and I recognise that there would be significant ideological and structural impediments to implementing either of them. Infrastructurally speaking, the current system of publications, citations and credit is so wedded to the concept of individuated authors that it seems impossible to dislodge, despite the recent accommodations made for 'contributors'. As Mario Biagioli notes:

So much has been hung on it from different sides that, despite its inherent instability, scientific authorship has become virtually unmovable. While there is an implicit awareness that the category of authorship needs to be reconstituted . . . the proposed

solutions find themselves chasing their own tails, often reproducing some of the very tensions they try to solve. (1998: 13)

However, there are precedents for both the models described that could be reconciled with existing infrastructures. Co-authorship seems to present fewer infrastructural problems than pseudonymous (let alone truly anonymous) authorship, but its implementation is clearly not straightforward (see Sinha and Back 2013). Conversely, the challenges posed by pseudonymous authorship are not insurmountable, as illustrated by van der Geest's (2003) experience. Other significant exemplars of 'dividual authorship' are Gibson-Graham, Nikolai Bourbaki and Luther Blissett. The portmanteau author Gibson-Graham does not have a separate ORCID and is publicly recognised as being a joint enterprise of feminist economic geographers Julie Graham and Katherine Gibson that continues since Julie Graham's sad death in 2010. Nikolai Bourbaki was a group of mathematicians who were influential on the Oulipo movement in art and literature as well as on Claude Lévi-Strauss since his collaborator/co-author André Weil was a member (see David Aubin 1997). Luther Blissett started being used by cultural activists in 1994 in north Italy. Unlike the other examples, it is a 'multiple-use name' in which no continuity of membership is asserted between different uses. This article is its first use in anthropology.

The greatest problem for anonymous authorship is that academe is driven by the symbolic currency of publications, which are traded in a material economy of jobs, promotions, salaries and benefits (Eve and Priego 2017). Notably, *Speak into the mirror* was published after William Washabaugh received tenure, so he could 'afford' to publish as John Doe, just as the authors of this article can afford to be Luther Blissett. In current times, which junior scholar dares to publish anonymously and waive the symbolic capital that accrues to publication? Experiments in co-authorship, such as Miyarrka Media, that downplay the authorial status of the anthropologist entail similar professional risks. This raises parallels with Marcel Mauss on the gift. In Mauss's terms, to publish anonymously is to disown the *hau* of publication (similar to an anonymous contribution, the so-called 'free gift' whose possibility Mauss brings into doubt). For most academics, especially early in their careers, this would be professional suicide. This is because publication is a system of generalised reciprocity. Most academic authors expect no direct financial return from publication, as if the author was making a free gift. However, the symbolic capital they acquire through publication brings career advancement: the indirect return gift. As McSherry notes, academic efforts 'are sent out into the world to bring an equivalent gift back to the donor' in the form of status and recognition, yet the scholar 'cannot publicly admit to any expectation of reciprocity, lest she be suspect of less than perfect devotion to the production of truth' (2013: 231). This is clearly a simplification: there are many reasons why academics write, in addition to those of gaining academic symbolic capital. Separately, Alpa Shah and Irina Silber (both 2022) have discussed some of the complexities and multiple responsibilities involved in publishing, particularly the political imperatives. There is a wider picture within which sit anthropologists and the people they work with. Often academic writing is irrelevant to that wider picture, so anthropologists have to write in different ways for different audiences. But accruing academic reputations remains cen-

tral (at least for those in academe) and this has driven a certain mode of authorship, on which we need to reflect. The argument of this article applies only to this admittedly restricted aspect of writing: why anthropologists put their names to what they write in academe.

Complicating matters further, the creator–work relationship is the basis of value in a very different way from literary authorship because the author’s name guarantees both a product and a truth-seeking process (McSherry 2013). Failure to recognise this function is perhaps the key flaw in Foucault’s arguments about the differences between literary and scientific works. Being enmeshed in a scientific discourse is no guarantee of authority; the author’s name is crucial to the warranting process, albeit in a very different manner from literary works. Here we see the ways in which academic authorship remains caught between different conceptions of the gift. On the face of it, anonymous authorship would free a work from claims of interestedness and ambition, allowing its content to survive or fall on its own merits. However, doing this would simultaneously discredit its authenticity by disconnecting it from the authority of the author (the *hau* of the author). As van der Geest observes of his attempts to explain to journal editors the ethical reasoning behind his decision to publish as Wolf Bleek, ‘it seemed irreconcilable with their concept of scientific work’ (2003: 15). Co-authorship (especially hyper-authorship) raises similar concerns around truth-seeking and responsibility, so, paradoxically, the assignment of authors does not necessarily resolve the basic issue. This is primarily because the figure of the individual author continues to underwrite mainstream models of co-authorship, as evidenced by the concerns about ‘inflated’ authorship produced by rising levels of co-authorship (see Biagioli 1998, 2013).

At the risk of caricature, in the hard sciences the arguments come down to being about position in the sequence (am I 127th author or 227th author? Should I be 120th author when xxx is 110th but has contributed less than me?). It remains the case that a few lead authors draft, revise and establish the argument on the basis of the data and its analysis painstakingly generated by the large team of co-authors without which nothing could have been written (for more nuanced accounts, see contributors to Biagioli and Galison 2013). It is those lead authors who eventually get the Nobel prizes.

In anthropology and similar subjects, there clearly are real practical problems that a multi-authored text would have to address. For example, when it comes to questions of interpretation, whose voice should prevail? Can a multi-authored text accommodate multiple perspectives, let alone disagreement? Who assembles and orders those voices and what choices would that entail? Such questions are not new. They were raised as criticisms of the ideas of polyphony and multivocality in the *Writing Culture* debates of the 1980s. For example, Philip Crang (1992) discusses arguments about polyphonic composition in anthropology and its relevance for human geography. Crang is sceptical about the larger claims made for how adding multiple perspectives can solve larger political problems, and equally cautious about negative claims that polyphony in effect becomes cacophony, necessarily removing a coherent authorial perspective. As he sees it, polyphonic composition can assist the production of delicate, nuanced accounts that do not pretend to a coherence that may be desired but that may not be widely shared. For him ‘the process of textual construction will still be one of contested authority; the best one can perhaps say is that a polyphonic text

opens up those contestations and allows the possibility of representing them in some form to the reader' (Crang 1992: 543). This sort of caution reminds us that there are power imbalances between people in the world that cannot be addressed by disputes over co-authorship (or anonymisation for that matter).

Indeed, it may be that the 'corporate' model of authorship (Biagioli 2013; Galison 2013; Strathern 2013) in particle physics and as codified in the CRediT taxonomy may not have an answer to the dilemmas about how authorial voice can be established if there are many authors. However, Marilyn Strathern has discussed anthropological approaches to multiple, partible ownership that is relevant to our discussion (2005, 2006, 2013). Discussing cross-cultural intellectual property rights, Strathern (2006) considers copyright in art production and sciences (patents on cell lines), exploring possible ownership claims from various groups (e.g. families with specific genetic mutations and Melanesian groups making 'art') over the items in question. These groups are comparable to groups of contributors to a research project who may or should be included as authors. Strathern ends her article by emphasising both the interdependence of things and people in Melanesian thought, and that understanding these mutual relationships is not helped by Western notions of property and ownership. Elsewhere, Strathern (2005) discusses the multiple authorship of funerary Malanggan images: 'Everything may be differentiated minutely in the way entitlements between persons are worked out, yet claims are understood as embracing multiple, rather than individual, interests' (2005, 16). Later, she makes it clear that the people who contributed to the creation of Malanggan images (who might be glossed as 'owners' of the images) have rights to disseminate and reproduce, but not to sell them. This complicates in interesting ways how one regards those responsible for the creation of a work (whether of art or of academic discussion).<sup>18</sup> As she points out, bridging the Malanggan and academic systems of creation, the fact 'that an item can be *validated* at all becomes part of its value' (2005: 23; my emphasis). She then discusses the collaborative enterprise involved in the processes of validation. These processes resemble the protocols for authorship (and responsibility) in collaborative big science, a point on which Strathern touches:

A similar additive perception of multiple workers allows scientific investigators to build on one another's work, so that they can distinguish the unique efforts of a team of inventors, who may publish as coauthors, from either the (contingent) technicians, funders, and others necessary to the outcome or else the work of antecedent or competitive teams to which the inventors 'add' the essential original input. (2006: 158–159)

These articles and the recent discussions of responsibility and collaboration in academic publishing (as summarised in the CRediT taxonomy discussed above) portray the author as a nexus in an interacting mesh of networks rather than an invariant, unified Western 'person'. This has a clear parallel with Strathern's earlier work on Melanesian 'dividuality', so we are left with the potentially powerful idea of 'dividual authorship' (where there are some examples, including Gibson-Graham and Nikolai Bourbaki, already mentioned above). To make it clearer, perhaps we should start talking of auths rather than authors? Referring to someone as an auth might make it clearer that their contribution, important though it may be, cannot by itself be identi-

fied with an author. If it takes a village to make a person, it takes a collective to make an author. As was said above, multiple authorship diffuses and hence questions the idea of 'authorial responsibility': 'author' and 'authority' have an etymological common root. Many auths may make it easier to accept and understand publications with complicated patterns of responsibility.

### **Concluding Thoughts: The Author is Dead, Long Live the Author**

Despite four decades of intense soul searching about anthropological writing practices, models of authorship in the discipline have remained strikingly stable, with sole authorship still dominant in social/cultural anthropology. This contrasts with other disciplines in the social, medical and natural sciences. As noted above, the norm in some disciplines is not just multiple authorship but hyper-authorship: articles with hundreds of authors are no longer uncommon in particle physics and biomedicine journals. Thus, of necessity, these fields have had to grapple (albeit inadvertently) with the questions these practices raise about the very nature of authorship: questions with which anthropology has not fully engaged.

At first glance, it seems easy to dismiss the rise of 'corporate' models of co-authorship as mere responses to the metricisation of academe and to treat anthropology's authorship practices as the natural outcome of our discipline's methodologies and epistemologies (and our heroic resistance to neoliberal encroachments upon them). Where the issue is considered at all, this is the explanation that anthropological commentators fall back on: ethnographic fieldwork rarely entails collaborations of the kind common in other fields, ergo, ethnographies are generally sole authored. This privileges collaboration (between researchers) at the expense of that between anthropologists and their interlocutors. It also treats authorship as a stable, coherent category: a view that most of us are prepared to challenge in the abstract, but perhaps not in our own work.

Of course, we *should* be concerned about the forces driving increases in co-authorship, especially the fetishisation of metrics and interdisciplinary 'collaboration' and the undervaluing of academic activities that do not produce measurable 'outputs'. As the demands on academics to publish more, and more quickly, seem ever-increasing, the intensification of co-authorship practices is a symptom of various academic ills. However, I am also concerned not to throw the baby out with the bathwater, not least because questions of authorship speak to issues that have long preoccupied social anthropologists regarding the ethics of representation and ownership, and it seems to me that they offer new insights into old problems.

Should social anthropology change its current authorship norms? We should consider whether either of the thought experiments are possible or desirable. The goal of this article is to encourage further discussion about how publication in anthropology is conceptualised. If the discipline is wedded to the practice of sole authorship, then we need to ensure that our defences of it are robustly reasoned. Indeed, discussions about authorship in anthropology have become more important than ever as our practices are increasingly challenged by our interlocutors. In light of the urgency added by

Black Lives Matter to the decolonisation of anthropology, questions about collaboration and authorship (as well as citation) demand serious thought and reflection within the discipline. Faced by such questions, the response that ‘this is what we’ve always done’ is likely to ring on deaf ears or, worse, to make us look tone-deaf. We cannot ignore the conversations that others are having. We should take care not to become trapped by the stereotypes, policies and regulations adopted in other fields (cf. Ribeiro 2006). So it could be that the time has come to take these models seriously (as more than just thought experiments) and ask how anthropology would have to change if work was routinely published anonymously or if we added our collaborators names alongside our own?

## Acknowledgements

This article has been written after extensive discussions with Kirsten Bell, whose influence is visible throughout. Her blog post (Bell 2023) on plagiarism and ChatGPT was published while this article was in press. Both she and a copy editor/discussant have graciously declined to be named as co-authors. I am very grateful to the *Social Anthropology/Anthropologie sociale* editors and their reviewers, who raised some important questions and helped me clarify many parts of the argument through several iterations.

## Funding details

This work was not supported by an external funding agency.

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## Notes

1. Indeed there is a power dynamic implicit in the distinction between authors (named and cited) and research assistants (who are all too often also unnamed/pseudonymised and uncited). This was discussed in a special issue in the journal *Ethnography* volume 15 issue 3 (2014); see Townsend Middleton and Jason Cons (2014), who cite some exceptions where anthropologists and assistants have co-published.
2. It also provides a very different possible response to the calls of Anne-Maria Makhulu (2022), Anne-Maria Makhulu and Christen Smith, (2022), Savannah Shange (2022) and others to #CiteBlackWomen.
3. Starting places are Mario Biagioli and Peter Galison’s collection (2013) and Rosemary Coombe’s work connecting legal history and the right to describe others (1998).
4. The chi-square statistic for this is 0.6454. The p-value is .724182, not significant at  $p < .05$ .
5. To name names, the instance that comes immediately to mind is the *Social Life of Things*, published the same year as *Writing Culture*. My suggestion is that the introduction (Appadurai 1986a) and Igor Kopytoff’s article (1986) get far more citations than the collection as a whole (Appadurai 1986b). This is indirect evidence at best and may be an artefact of the citation process but is evidence of a sort.



6. The collaboration between these two authors is an exemplar of what is possible: ‘we collaborate, acknowledging that ethnographic knowledge is made by ethnographers and informants, and should be owned by both’ (Blasco and Hernández 2012: 1).
7. For example, it once was common that only one of a married couple would be the author of what would now be regarded as joint works (and we know which one of the two it would be). We now read GERTRUDE’s collation (1978) of acknowledgements to the wives of anthropologists with something like shamefaced embarrassment: how could they be so brazen as to say these words but not acknowledge them as co-authors? But brazen attitudes continue. David Pontille discusses the case of a technician acknowledged as being essential for maintaining and updating a database but not thereby deserving of authorship: ‘In no way was she judged to be in a position to write *texts*, that is, articles publishable in scientific journals, like the doctors she worked with’ (2010: 64; emphasis in original).
8. Change may be coming: another journal in which co-authorship can be expected is *Human Organisation*, the journal of the Society for Applied Anthropology, in which single authors dominated (70:30 percent) in both 1999 and 2009 but flipped to being almost exactly the other way round by 2019 (28:72 percent).
9. This has made the inclusion of multi-authored articles in theses by publication difficult. Workarounds have been found but they remain a challenge to doctoral assessors.
10. Against this, some appointment committees find quantity as persuasive as quality: a social anthropologist may produce one solo-authored article in the time it takes for an archaeologist or psychologist to contribute to six (possibly as one of six co-authors). Some appointment panellists simply see one contribution against six!
11. Certain pseudonyms are used in the Marxist tradition, including ‘The Invisible Committee’ and my own appellation ‘Luther Blissett’, which Nicholas Thoburn defines as an “open reputation” that conferred a certain authority and capacity to speak – the authority of the author, no less – on an open multiplicity of unnamed writers, activists, and cultural workers, whose work in turn contributed to and extended the open reputation’ (2016: 187). It is also worth noting the very different tradition of anonymity represented by the Chatham House Rule for meetings on controversial or politically sensitive topics. By this rule ‘participants are free to use the information received, but neither the identity nor the affiliation of the speaker(s), nor that of any other participant, may be revealed’ (<https://www.chathamhouse.org/about-us/chatham-house-rule>).
12. As has been said, this is an open identity so others are free to publish under the name and to associate other publications with this ORCID. The earlier publications can also be added.
13. Prompted by the careful reading of a *Social Anthropology/Anthropologie sociale* reviewer, a qualification to this statement is needed: individual words or phrases may be trademarked and in that sense ‘owned’ by others (sadly I dare not give examples), although in general the words of a language are not owned by anyone. The automatic copyright of the speaker or writer applies to the phrasing – the way someone chooses to string the words together, which makes long, verbatim quotes problematic without explicit permission.
14. We are intrigued by the possibility that those anthropologists who have published their rejection notes (e.g. Stoller and Olkes 1986: 347) could be sued by the authors of those comments whose copyright has been violated. In literary fiction, JK Rowling and Doris Lessing’s publication of rejection letters received when writing as Galbraith and Somers raises the same questions. Some may justify their quotations as allowable as ‘fair use’ or ‘fair dealing’ (the terminology varies between jurisdictions). Quoting from unpublished letters illustrates the potential problem: the precise number of quoted words justified as fair dealing is unclear but is certainly small (quoting any more than about 15 percent of a letter is unlikely to be regarded as legally ‘fair’). Not only that, the authors (copyright holders) of the rejection letters probably wrote them in the expectation that their words would not be published, so there is a moral as well as a purely legal issue at stake!

15. This is the converse of the above reference to anonymity offering protection in the ‘age of objection’.
16. See Daniel Souloules’ articles on ethics and consent when studying powerful financial actors (2018, 2021).
17. This might be recorded by reporting how many potential co-authors declined the offer: a paper with 150 co-authors may note that 320 others had declined co-authorship during the consent process. More than declining to be named if a more formal permission is needed from all contributors then they become powerful. After this paper was accepted, Benoît Eyraud and colleagues published a paper (2022, but only appearing in 2023) discussing some of the implications for collaborations not only between academic and non-academic partners but also between those who might be categorised as disabled or abled. They emphasise the power of refusal where by refusing to sign a permission or a copyright form a participant holds a form of power over the other and usually dominant parties.
18. Rosemary Coombe makes a similar point about Aboriginal Title (1998: 245).

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## **Le hau du papier et l'auteur dividuel : réimaginer l'auctorialité en anthropologie**

**Résumé :** En dépit d'appels répétés pour une évolution de nos pratiques, l'anthropologie sociale et culturelle est toujours dominée par l'auctorialité au singulier. Je considère ici deux expériences de pensée qui peuvent perturber le status quo en la matière de façon intéressante. Nous pouvons ainsi publier anonymement, nous traitant ainsi de la même manière que nous traitons anonymement nos informateurs, par exemple avec l'usage de pseudonymes. Alternativement, nous pouvons traiter nos collègues sur le terrain non seulement comme des égaux, mais également comme des co-auteurs. Ces deux options ont des implications en ce qui concerne l'auteur « dividuel » (qu'il faut peut-être désormais penser comme un « auth »), ce qui implique de repenser le « hau » de la publication

**Mots-clés :** auctorialité, expérience de pensée, pseudonymes, collaboration, informateurs comme co-auteurs