FQS FORUM: QUALITATIVE SOCIAL RESEARCH SOZIALFORSCHUNG

Volume 22, No. 1, Art. 1
January 2021

Problems of Evidence in Ethnography. A Methodological Reflection on the Goffman/Mead Controversies (With a Proposal for Rules of Thumb)

Joost Beuving

Key words: Alice Goffman; direct observation; ethnographic evidence; Margaret Mead; methodological rules of thumb

Abstract: The contestation of ethnographic authority in the post-truth era revolves around the credibility of ethnographic evidence. This doubting of ethnographic evidence is usually explained as the consequence of postmodern relativism coupled with political opportunism and the social impact of the internet. I argue, however, that evidence in ethnography comprises a much older unresolved methodological problem that arises because: 1. ethnographers' unique observations are difficult to marry with the scientific ideal of replication, but what other tests are then available to support direct observation?; 2. social proximity to the community studied is essential for making direct observations, but how does that correspond to the ideal of outsider verification?; 3. facts are considered central in credibly reporting ethnographic thick description, but is it possible to write ethnography in an interesting way without resorting to the instruments of fiction? These methodological challenges are explored by juxtaposing two ethnographic controversies: Margaret MEAD's "Coming of Age in Samoa" (1973 [1928]) and Alice GOFFMAN's "On the Run. Fugitive Life in an American City" (2014). I conclude with a proposal for methodological rules of thumb for conducting ethnographic research in the 21st century in a way that is (hopefully) both effective and convincing.

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Forum Qualitative Sozialforschung / Forum: Qualitative Social Research (ISSN 1438-5627)

1. Introduction

A few years ago, American sociologist Alice GOFFMAN published an ethnography about the daily predicaments of the Afro-American underclass with the evocative title: "On the Run. Fugitive Life in an American City" (2014). The book received critical acclaim from both social scientists and the general public, but it also elicited sharp responses. The decriers focused their critique on, amongst other things, how GOFFMAN dealt with facts in her work: were they all true; did GOFFMAN not polish—or worse: make up!—key parts of the narrative; and did she not permit herself literary freedoms that were unscientific? These questions are of methodological interest: they refer to the veracity, or truthfulness, of ethnographic material and raise challenging questions about the relation between fact and fiction in ethnographic work. In the *post-truth* era of the early 21st century marked by a demise of a common world of facts (VAN DER PORT, 2017), these questions exceed mere academic frolicking; they may have far-reaching consequences. After all, if GOFFMAN in her work conflated fact and fiction, this fuels the thought that ethnography is not an objective exercise—objective here defined as processing experience beyond the writer—but merely one subjective opinion amongst many others. Hence, these are not innocent questions at all. A negative response will have serious repercussions for the academic prestige of ethnography—possibly materialising in cuts in public funding. [1]

The GOFFMAN case does not stand alone. Behind its contested reception looms a much older discussion about problems of evidence, which began when anthropologist MALINOWSKI and sociologist PARK pioneered modern ethnography, around a century ago.[1] For instance, there is the ideal of the ethnographic researcher submerging him/herself in some society, creating a time-specific snapshot—commonly referred to as the *ethnographic present*. Societies change continuously however, sometimes imperceptibly slowly, sometimes dramatically quickly, and the unique observations of the ethnographer are difficult to marry with ideals of replication that feature prominently in the *normal science* (KUHN, 2012 [1962]) practices of sister traditions, sociology and psychology in particular.[2] In the absence of replication, to which other tests can ethnographic observations be subjected? Further, ethnography is often visualised as a close encounter with a host society, whereby closeness is seen as key to directly observing social life. But when closeness takes the form of special access, how does this resonate with the scientific ideal of outsider verification? Also, ethnographers take special pride in their capacity to observe social life naturalistically, i.e. without creating a research situation, but is that enough to

1 Inspired by the work of KAPLAN (1964), I use the term *evidence* here to refer to the complex of empirical material that is produced in ethnographic inquiry, including the reasoning that applies to it in a theoretical argument.

2 I refer here to *normal* science in the sense of ontological positivism: the epistemological assumption that the natural world and the social world are ordered by law-like regularities (KAPLAN, 1964, p.36). The interpretive view on ethnography to which I subscribe in this article is also concerned with order, but the underlying assumption is that order follows from how humans understand their situation and act upon that (ATHENS, 2010; see also BEUVING & DE VRIES, 2015).

make ethnography scientifically worthwhile or should one perhaps also consider how observation features in processes of interpretation and explanation? To round off this preliminary inventory: ethnographic researchers are expected to report their interpretations in a narrative form known as *thick description* (GEERTZ, 1999 [1973]). Regarding style, ethnographic writing may be confused with literary work, which may lead one to conclude that it is not very scientific. A question remains, however, as to whether it is actually possibly to report ethnographic findings as dry facts. In other words, do facts result exclusively from observation or are they crafted in a process of comprehensive (and hopefully interesting) description? [2]

These are difficult methodological questions and I can only hope to make a small contribution to resolving them. They are not new questions but, as I clarify in a moment, the GOFFMAN case offers a unique opportunity to reassess them.[3] I do that in particular by juxtaposing the reception of Alice GOFFMAN's work (2014) with that of another prominent ethnographic researcher, Margaret MEAD.[4] Since MEAD published her "Coming of Age in Samoa" (1973 [1928]), questions similar to those regarding GOFFMAN's work have been asked. MEAD's work too was applauded but also received serious criticism. In fact, a serious literature has developed since then over precisely the types of methodological issues that I want to address in this article. [3]

To foreshadow a more elaborate discussion of her work later on: MEAD was interested in the transition from youth to adulthood, focusing specifically on the role of puberty. MEAD argued that puberty in Samoa was a more gradual transformation process than it was in the USA, from where MEAD originated. She posited that this was because adolescent girls in Samoa enjoyed greater sexual freedom than their American sisters. However, that conclusion was immediately disputed by Pacific experts who pointed out that she had ignored prevailing, rigid gender stratifications on Samoa that limited rather than promoted young women's sexual autonomy. This critique culminated in the publication of "Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth" by New Zealand anthropologist FREEMAN (1983). He openly questioned the veracity of MEAD's work, confronting it with his own restudy of the same Samoan villages

3 In an earlier manifestation, ethnographic debate became introspective. Known as the *writing culture debate (*CLIFFORD & MARCUS*, 1986)*, it became focused on the relation between *researcher* and *researched*, whereby it was argued that the former by virtue of his or her class position and/or social standing commands a greater power of defining the ethnographic situation (CRAPANZANO, 2004). Proponents insisted that this found its way into a style of ethnographic writing in which the researched was represented as an exotic other and more was said about the researcher and his or her personal views than about his or her object of inquiry (FABIAN, 1990). It was therefore focused mainly on a deconstruction of the rhetorical devices that ethnographers use in constructing their texts. Although introspection is not without its merits, with this article I subscribe to a more extravert perspective, seeing ethnography as a pragmatic attempt to tell about society and the problems it faces (BECKER & KELLER, 2016), rather than seeing it as a mirror in which to understand oneself.

4 The analysis developed in this article revolves around the juxtaposition of two cases in the sense implied by RAGIN and AMOROSO (2011 [1994]). The cases are seen as reflecting important differences in styles of ethnographic work, and a juxtaposition helps to sharpen ideas about the processes that shape each style. Whether these styles reflect distinctive phases in the historical development of ethnography is a relevant question, albeit one that is beyond the scope of this article.

that MEAD visited decades earlier—in later publications adding to his line of attack the suggestion that MEAD had been taken in by her informants. In other words, what was wrong with the evidence that MEAD presented in her work? [4]

The issue has been ongoing since the publication of FREEMAN's book. About eleven years ago, anthropologist SHANKMAN (2009) published "The Thrashing of Margaret Mead", arguing that both MEAD and FREEMAN had little eye for outside influences on Samoa; those of the (US) army and the Protestant mission in particular. About three years ago, Australian anthropologist HEMPENSTALL (2017) published a critical biography of FREEMAN, focusing on the history of mental problems that troubled FREEMAN, thereby reigniting the debate on MEAD, which is now almost ninety years old. In 2019, British historian KING added a new contribution with a balanced intellectual history of the group surrounding BOAS, whereby he positioned MEAD's work in a broader discussion with her contemporaries, such as BENEDICT and BATESON. In the remainder of this article, I review the main arguments in the works of FREEMAN, SHANKMAN, HEMPENSTALL and KING insofar as they are relevant for my argument about problems of evidence in ethnography. [5]

The controversies that surround MEAD's work are central in this article, but I want to pre-empt the possible misunderstanding that they are outdated. MEAD's work focused on Samoa in the early 20th century and that society has transformed very deeply. Closer inspection, however, suggests interesting parallels with the work of GOFFMAN. Themes relating to evidence that emerged during the MEAD controversy reappeared in an entirely different empirical context—the disenfranchised Afro-American underclass in North American cities, which has quickly formed since the 1980s (FORMAN, 2017)—and in a societal setting where social science as a *practice of facts* has come under pressure. So, whereas much of what follows relates to ethnographic work in a remote setting a long time ago, it is essential study material in a contemporary methodological discussion focusing on ethnography and evidence today. [6]

From the lessons learnt, I attempt to sketch the contours of an ethnography that is ready for 21st century challenges, including the resolution of some of the persistent problems surveyed in the article. Thus, I touch upon what may be considered the substantive core of ethnography: how can something useful be said about individual persons and the societies in which they live, as well as committing to ideals of scientific truth finding in order to fight the eroding claims of post-truth discussion (LUHRMANN, 2020)? I address that with a proposal for methodological rules of thumb for ethnographic research, revolving around the question: what are actually ethnographic facts and how can they be reported interestingly and in a way that makes society more transparent, at the same time doing justice to fundamental questions of evidence? [7]

I begin with a short presentation of the two ethnographic cases: the works of GOFFMAN and MEAD (Section 2). After that, I discuss three methodological problems as they appear in the two cases (Section 3), followed by the proposal for rules of thumb (Section 4) and a concluding discussion (Section 5). [8]

2. Margaret MEAD and Alice GOFFMAN: A Short Presentation of Ethnographic Cases

2.1 Margaret MEAD (1901–1978)

MEAD, a student of the German/American anthropologist BOAS (1858–1942), began her career as an ethnographic researcher on the Polynesian island of (American) Samoa in 1925. Her work was considered progressive by the standards of the time as she refrained from making a *Totalbeschreibung* [total description] of Samoan society, a style of ethnographic work propagated by MALINOWSKI (1978 [1922]) and BOAS (1894), instead directing her energies at a special problem: the study of coming of age, especially puberty. MEAD posited that puberty on Samoa unfolded without much drama; Samoan youth considered it as a relatively unproblematic passing into adulthood. In fact, puberty on Samoa had everything to do with a growing sexual awareness, avidly enjoyed by both young men and young women. MEAD crafted an image of sexual autonomy in which young women entertained extensive networks of sexual partners—the image of queen-bees suggests itself. MEAD contrasted this image with the, in her eyes, repressive sexual norms of mainstream contemporary American society where young people's budding sexuality was considered a major social problem, especially by their parents. MEAD presented her Samoan message of sexual liberalism as evidence of the cultural foundations of behaviour. It must be remembered that this was the heyday of the nature-nurture debate that widely divided opponents into biological or cultural camps. MEAD's work arrived at a juncture when both sides were at loggerheads, and it was seen as a decisive blow against biological determinism (KING, 2019). [9]

"Coming of Age" was an immediate hit, propelling MEAD to celebrity status: she was invited to public lectures and radio presentations across the United States, and in later decades she made frequent appearances on televised talk shows. Her message of sexual autonomy resonated strongly with the emancipatory mood of the 1960s. MEAD's work was considered an essential voice among young Americans in their stand against the prevailing power configuration of the post-war era. Young women especially saw in MEAD an influential figure in their struggle for gender equality and therefore an important voice in women's emancipation. Because of this, *Time Magazine* ranked MEAD among the twenty-five most influential women of the 20th century[5]. Scientifically, MEAD's work elicited a less positive, even sceptical response; throughout, her work has been contested by her peers. During the "Coming of Age" publication process, fellow anthropologists specialising in the era in which she had worked claimed that MEAD's portrayal of sexual life of young Samoan women was far too rosy. Her critics pointed to the paramount role of virginity in marriage arrangements: deflowered women were not eligible for marriage as they were considered symbolically impure. To avoid the shame of impurity through sexual adventures, young women were always chaperoned by their relatives, who kept a close watch; in practice, this ruled out the possibility of sexual escapades (FREEMAN,

5 See http://content.time.com/time/specials/packages/completelist/0,29569,2029774,00.html [Accessed: September, 24, 2020].

1983, p.228). To this key ethnographic problem of making rival observations, I shall return at a later point. [10]

2.2 Alice GOFFMAN (1982–)

GOFFMAN was a student of the urban sociologist (and ethnographer) Elijah ANDERSON, who trained at the University of Chicago. Like MEAD, GOFFMAN was interested in a special problem, namely, how the police state that successive American administrations erected around the so-called war on drugs reproduced the cycle of poverty and the impossibility in which the urban, black underclass found itself caught up. GOFFMAN got to know this world via her student job in a campus diner at the University of Pennsylvania where she met poor, black women as her co-workers and eventually began to visit them at home, several blocks away from campus. Slowly, GOFFMAN became closer to their community, a process that took the better part of three years to complete. GOFFMAN observed how her informants were implicated in the petty drugs trade that was key to the underground urban economy on which members of the black underclass drew and depended: few had access to formal jobs. Nonetheless, GOFFMAN did not victimise her informants. Criminality might pervade everyday life in the black ghetto, and most young men had had frequent brushes with the law (GOFFMAN visited prisons as part of her fieldwork), but the mothers and wives of her informants generally opposed criminality. Furthermore, not all young men participated in the illegal underground economy: GOFFMAN (2014) extensively portrayed informants who carefully sought to remain outside the criminal web—and were often successful in that pursuit. [11]

Like MEAD, GOFFMAN became the immediate centre of public attention upon publication of her work, and she embraced this readily. She featured in a much-viewed TED talk (GOFFMAN, 2015) where she spoke out against the police state that had effectively materialised around poor parts of the city where many Afro-Americans lived. Her argument: the more American law enforcement fixated on suppressing the underground drugs economy, the more likely black young men were to end up in prison, depriving their families of income, which in turn perpetuated the poverty that drove participation in the drugs economy in the first place. Given her analysis, GOFFMAN insisted on decriminalising black, poor neighbourhoods, starting with the creation of decently paid jobs that removed the necessity to make ends meet by engaging in the illicit economy. This outspoken message touched a sore spot of conservative, white America, which responded with a volley of criticism of her work, chiefly by magnifying inconsistencies in it (LEWIS-KRAUS, 2016).[6] It also triggered a juridical debate over the question of whether GOFFMAN, who had directly witnessed both the trade in and the use of drugs, was complicit in a criminal activity and should therefore be prosecuted. [12]

A more serious concern from an academic viewpoint was the responses from within the social sciences. In addition to praise from several heavyweight public

6 In conservative, political circles in the USA, there is increasing support for limiting mass incarceration, based not on humanitarian considerations but on costs. The TRUMP administration has prepared far-reaching legislation that has received bipartisan support.

intellectuals applauding GOFFMAN for her innovative field research (VAN MAANEN & DE ROND, 2017; ZUSSMAN, 2016), her work also elicited critiques. Chief among them for the purpose of the present discussion are several independent observers claiming that they could not corroborate the facts of police repression as GOFFMAN recorded them (LUBET, 2015)—although others emphasised that they had made observations similar to those of GOFFMAN (SINGAL, 2015). To this problem of describing ethnographic facts in relation to evidence in ethnography, I shall return later. First, I zoom in on the methodological problems outlined in the introduction: 1. the problem of the ethnographic present; 2. the matter of privileged access; and 3. facts in ethnographic text—the *thick description*. [13]

3. Methodological Problems in the Ethnographic Cases

3.1 The ethnographic present and "testing" observations

GOFFMAN and MEAD both faced the thorny methodological problem of the ethnographic present: whether the here-and-now of their observations in, respectively, Samoa and Philadelphia were also the here-and-now of the societies they studied. I briefly elaborate this point. GOFFMAN and MEAD developed their ideas about their respective field sites in the course of a delineated space of time that corresponded to their fieldwork (six years and one and a half years, respectively), but their ideas cannot be interpreted *ex post facto* without further reflection. Simply assuming that the here-and-now of ethnographer and society seamlessly overlap ignores the longer-term dynamics that make societies tick. Such dynamics cannot be directly observed; they reveal themselves in the results they produce: the path dependency of social transformation. Because of this, societies tend to present themselves to outside observers as a comprehensive whole of social practices and shared cultural ideas, and why not assume that this has always been so? This is a problem that is especially prevalent in, but not limited to, societies with a limited historical or oral history track record.[7] [14]

A major attraction of observing and writing about societies from the vantage point of the ethnographic present is that it seemingly confers scientific status on ethnographic work. That is, it corroborates the standards of empirical replication that has celebrity status in circles of normal social science: the same results when the original study is repeated. The previous point demonstrates the problem of this reasoning: societies change—sometimes fast, usually very slowly, sometimes dramatically, but often imperceptibly. This renders the scientific requirement of replication via repeat studies pointless. Rather, it is more useful to think about ethnographic observations as having *temporal validity*: it is impossible for anyone to travel back in time and redo the observation.[8] Societies are not experimental settings in the sense of a laboratory where variables may be kept

7 In this article, I subscribe to an interpretation of society that social thinkers such as SIMMEL (1950) and ELIAS (1998 [1939]) pioneered: as a network of interdependencies between persons. Thus, there can be a little, or a lot, of society, depending on the strength of the web of relationships; also, society can be small scale (a clan or a gang) or large scale (in the case of a region or the nation-state).

constant. Rather, societies have organic properties with change as the only constant; in the vocabulary of ELIAS (1998 [1939], p. 50), there is an *Eigendynamik* [own dynamic] to consider: an autonomous dynamic generated from within social figurations that comprise society as a whole. To be sure, questioning the ethnographic present is not a licence for making haphazard observation (i.e. observation unguided by a specific problem); as I argue below, not all observations are necessarily social facts. But how can the original observation then be further tested? [15]

At this point in the argument, an abyss appears to open up between GOFFMAN and MEAD in their different ways of dealing with external forces impinging on their respective cases. MEAD chose to reinforce the ethnographic present by ignoring external forces. She portrayed Samoa as a peaceful society and not without reason: peace was an important precondition to understanding why the alleged promiscuity of young women did not trigger all-out social conflict—elsewhere, this would have whipped up competition between young men. FREEMAN in his work repeatedly pointed out that, at the time of MEAD's fieldwork, Samoa was a colony of New Zealand with a far from peaceful situation. There had been uprisings of titular chiefs: local rulers whose social prestige was holding Samoan society together. Considering Samoa in this way sheds a rather different light on MEAD's work: the knowledge that Samoa was in fact a boiling political hotbed in which New Zealand colonial powers intervened with brutal force obviously prompts various challenging questions about the alleged state of peace in the Samoan paradise that MEAD described. A similar argument can be offered regarding the Protestant mission to which Samoa was subjected; it was essential to fully apprehend the presentation of MEAD's informants' sexual practices, yet she remained tight-lipped about the whole thing, FREEMAN (1983) argued.[9] [16]

SHANKMAN (2009) came to MEAD's rescue, arguing that the way she attributed a timelessness to Samoan society should be regarded as a rookie mistake: she was only in her early twenties when she committed herself to the formidable challenge of overseas fieldwork. Later in her career, however, this inclination repeated itself—for instance, about a decade after her Samoan fieldwork, during her visit to the Indonesian island of Bali where she worked with her then husband Gregory BATESON. Their visit was discussed by the Dutch journalist and historian Tessel POLLMAN (1990) showing that, similar to her actions on Samoa, MEAD on her Balinese visit did not genuinely mingle socially with the local population as the ethnographic ideal has it; rather, she regarded them at an appropriate distance—from the residence of Walter SPIES, an important cultural go-between in the Dutch colonial expatriate community. POLLMAN (1990) explained how SPIES portrayed a romantic view of Balinese society that MEAD (and BATESON) devoured: a harmonious society driven by a profound love of artistic expression (painting, dance, and theatre) that appeared to thrive under the Dutch colonial order. Again, MEAD missed the point: she readily accepted an

8 I thank Dr. Geert DE VRIES for suggesting this evocative term to me.

9 Moreover, SHANKMAN (2009) argued that FREEMAN (1983) was responsible for the same issues about which he accused MEAD by ignoring the American occupation of Samoa during the 1940s: the island had a strategic military position in the Pacific during the war with Japan.

image of society without questioning the wider frames of reference (in this case a repressive colonial regime) in which it was embedded.[10] [17]

In GOFFMAN's ethnographic work, we see a dramatically different image of society: not a timeless, essentially symbolic order but one that was constantly brewing and where the outside world, represented especially by the judicial apparatus and to a lesser extent the social care system, was manifestly present, often invading her informants' private lives. The outside world was a hostile one, which could not be wished or willed away. Extensive descriptions of it were as necessary for GOFFMAN's story as the numerous descriptions of her informants' everyday actions. GOFFMAN's readers become acquainted with various of her informants and are immersed in their—often conflicting and contradictory—considerations, for instance why they participated in the illicit drugs economy when this magnified their exposure to law enforcement. Her readers accompany her to arrests where police brutality was common (thus foreshadowing the tragic death of George FLOYD in 2020), and she showed the disillusion of her informants when they were brought to court and received serious sentences for minor offences. And she learnt how the disillusion invariably culminated in a self-fulfilling prophecy: the outside world imprinted upon her informants the message that they were good for nothing, which, over time, began to function as a behavioural format. GOFFMAN thus situated the Afro-American underclass in the context of America's war on drugs, made possible by the considerable expansion of the police force since the 1980s in a world where ideals of equal opportunity clashed with the realities of racial discrimination, producing a very unequal world indeed. [18]

It is relevant for our argument that GOFFMAN's ethnography does not stand in isolation. Her observations corresponded with a stream of articles in renowned magazines and newspapers corroborating what GOFFMAN also reported: members of the Afro-American underclass de facto lived in a police state that could not be easily escaped (FORMAN, 2017; WACQUANT, 2009). Relevant too: GOFFMAN did not leave it at merely describing the facts that comprised this police state. She added to it a layer of interpretation: the argument of the self-fulfilling prophecy. The critique, briefly discussed in the introduction, that GOFFMAN fabricated her observations in order to bolster a position (ideologically motivated) does not therefore seem very credible. GOFFMAN could be wrong about details. In her book, she sometimes mixed up the names of her informants, a point that she rebutted by pointing to the difficulty of consistently using pseudonyms in her fieldnotes: the possibility that her notes with incriminating information would fall into the hands of the authorities—possibly sending her informants to death row—was constantly on her mind. However, stating that she was wrong about her fieldwork seems farfetched considering the work of others who reported similar facts, independent of GOFFMAN. [19]

10 The tranquility, however, was of recent origin—a point that MEAD appeared to have missed—following a violent episode of a bloody, armed uprising against the Dutch colonial state. This point underscores the importance of contemporary historiography in ethnographic inquiry.

3.2 Privileged access?[11]

In their fieldwork, both GOFFMAN and MEAD were driven by the ethnographic ideal of social proximity, necessary for making direct observations of everyday life (WILLIS, 2000). Proximity as a precondition for ethnographic work is almost a platitude, routinely circulated through methods teaching and socio-scientific publications. (Debates on big data have reignited discussion over the primacy of observation in ethnography, but that is beyond the scope of this article, see e.g. BEUVING, 2019.) It must be remembered, however, that, not that long ago, the term ethnography was associated primarily with writing on the basis of observations by others such as colonial bureaucrats, missionaries, adventurers: armchair anthropology (KUPER, 2005). Many view the monograph "The Golden Bough: A Study in Magic and Religion" by the Scottish anthropologist FRAZER (2016 [1890]) as a pinnacle of this tendency. Dissatisfaction with its grand, rather racist, claims about "primitive" society gave rise to the famous Torres-Strait expedition of 1889. During the expedition, RIVERS, the later thesis advisor of MALINOWSKI, and SELIGMAN, head of the anthropology department at the London School of Economics, observed social life in this part of Melanesia with their own eyes. They arrived at markedly different insights about so-called premodern societies and trailblazed fieldwork-based ethnography (BARTH, 2005). [20]

Proximity has special consequences.[12] The group or society in which the ethnographer is interested must minimally tolerate, preferably even embrace, an outsider. Achieving this may take considerable time (GEERTZ, 1993 [1973]); PRICE & PRICE, 2017), especially when it is difficult to get to where the action is, either because of practical travel problems or because the group in which one is interested is not receptive to outsiders. It may therefore be tempting to view the relation that one establishes eventually as special, and special has the connotation here of: inaccessible by other outsiders. Reasoned from the maxims of normal social science, this immediately creates the problem of outsider verification: when access is difficult to negotiate, how can one be sure that observations are not brushed-up or downright fabricated? It also raises the question of whether a special relation automatically promotes focused observation: *fellow travelling* and *going* native in ethnographic discourse are tainted terms for good reason. GOFFMAN and MEAD adhered to a radically different interpretation of proximity however. MEAD travelled to Samoa in 1925 (an audacious adventure for a solo-travelling woman in those days) and stayed on the tiny island of Ta'ū, covering a mere forty square kilometres. She occupied

11 This debate has also a normative side in the form of the *politics of representation*, revolving around the question: who is to represent whom? Certainly, this is a point worthy of consideration, but it is not relevant to the methodological concerns of this article. In the US, the politics-of-representation card was played especially by an anonymous group of students from Eugene Lang College in May 2018 seeking to decry GOFFMAN's work by pointing to her privileged background. (The letter is no longer available online.)

12 A related matter that I can only touch upon here is that proximity changes through the virtualisation of society, via expanding internet use. As our social lives occur more and more online, the ethnographic researcher can no longer limit him/herself to one place. The follow-the-networks credo is more than ever valid and is acquiring the meaning of following relations that cannot be observed directly.

a room in the house of the American HOLT family who ran a local dispensary. MEAD received her informants in her room and the adjacent veranda, interviewing them about their sex life. Although MEAD visited the villages on Ta'ū (all three of them), she never stayed there. In "Coming of Age", MEAD (1973 [1928] presented all sorts of observations of events that she never witnessed herself; she reconstructed them from the stories of her informants, without checking them with her own eyes. Her argument: through ethnography, she sought to represent the experiences of the young women in whom she was interested and these could be inferred from their stories.[13] [21]

GOFFMAN had an altogether different research experience. Her fieldwork did not so much entail travel geographically speaking (although it constituted an enormous leap socially). From her student accommodation to the black ghetto was a matter of a metro trip and then a brisk stroll. Ghettos are grim places where few people would immediately feel at ease, but there are no special qualifications required to get there. The world that GOFFMAN observed was also accessible to outsiders who cared to make the effort—some of GOFFMAN's friends from the privileged white student bubble of which she was part visited her (and never liked it there). It proved much more demanding to avoid her presence disturbing the setting—something for which MEAD cared little. GOFFMAN wrote in the extended epilogue to "On the Run" (2014) that she tried to make herself as small as she could and that she considered it a success when an informant told her about some event and could not remember whether or not GOFFMAN had been present. As she stated, it was no more than an attempt to become a fly-on-the-wall: "I never blended in [...], I became an expected part of the scene" (p.233). [22]

The GOFFMAN case shows something else that impacts on privileged access, namely, the role of language in ethnographic fieldwork. Qualitative research courses often promulgate the message that command of a local language is essential in the pursuit of genuine access: how else is one to grasp other cultures' symbolically without a deep understanding of the vehicle that generates symbolic meaning: language? Reasoned to its logical conclusion, thus attributing a master status to language means that verification of ethnographic findings beyond those who are sufficiently versed in that particular language is extremely difficult. Further, it would impose a serious impediment on precisely those sites where a lot of action is going on ethnographically speaking: where individuals from various walks of life are thrown together in melting pots of globalisation where various languages are spoken. Moreover, it is difficult to meet the requirement to be fluent in a foreign language, especially when this concerns difficult-to-very difficult languages, especially considering the shrinking of fieldwork time following from the rationalisation of public funds for social research. [23]

That is not to say that language is not important. Language is important to participate in society, chiefly in the sphere of small talk or phatic communication:

13 Later in her career, MEAD, together with her then husband BATESON, studied *Iatmul* villagers on Papua New Guinea, again keeping an appropriate distance from her informants (KING, 2019).

seemingly innocuous conversations about mundane topics; say, the weather, traffic at rush hour, computer problems. Being able to converse, hence contributes to an accepted presence, which fosters the making of direct observation (DRIESSEN & JANSSEN, 2013). Additionally, making informed guesses about key concepts may be productive. Local fishermen on Lake Victoria, for instance, respectfully refer to prominent boat owners as *omugaga*, but that term is also used by the fishermen themselves in a joking way, typically when one of them displays too much ambition for the others' taste; in that case, being called *omugaga* has the effect of putting somebody in his/her place. The term thus provides an entrance into understanding the working of social stratification (BEUVING, 2017). [24]

A further methodological misunderstanding about language as privileging access is thinking that the words spoken by informants are pure facts. MEAD appears to have stumbled into this problem without a clear vision on language and took her informants' stories at face value, as facts (SHANKMAN, 2009, pp.96-100).[14] GOFFMAN had to verse herself in the street language of her informants (so-called African-American vernacular English), but, as she admitted in the epilogue to "On the Run", she never mastered that language; her elite pedigree combined with her white skin colour betrayed her, and GOFFMAN was reflexively aware of it. Although she deplored her linguistic incompetence, eventually she came to see it as an advantage: being able to ask questions about key concepts that she could not have posed had she been a fluent African-American vernacular English speaker. [25]

The above points suggest, in my opinion, that exclusivity of access in order to arrive at "true", or "real", or "deep" ethnographic insights is not a helpful myth. It is based on the misleading idea that ethnography entails doing something special—in an extreme case: viewing ethnography as specially tasked to unmask the big secrets of societies. This view is not uncommon, especially in the study of groups far above or far below the middle-class position from which many ethnographic researchers originate. GOFFMAN's work, but also bear in mind classical studies such as BECKER's on marihuana users (1963) or DOUGLAS' on nude beaches (1973), which strongly suggest that the verifiability of ethnographic work—for anyone who cares to be interested—contributes considerably to its stature in the socio-scientific discourse. Subscribing to a view on ethnography as a research practice that is about what anyone could see (but cannot always—the ethnographic eye requires careful grooming and a dash of talent, perhaps) obviously does not mean that ethnographic facts can speak for themselves. It is essential to add extra layers to the observation, namely, those of interpretation (*Verstehen* [understanding], WEBER, 1968 [1921/1922]) and [theoretical] explanation (BEUVING & DE VRIES, 2015). [26]

14 Making this statement deviates from saying that MEAD misunderstood her informants, and that she was even taken in by them, as FREEMAN (1983) claimed (SHANKMAN, 2009, pp.197f.). It is impossible to track down this claim and also not relevant for my critical point: whether or not language offers privileged access to understanding symbolic reality.

3.3 Ethnographic *thick description* as novel writing

GOFFMAN and MEAD reported their ethnographic insights in a narrative form. Ethnographic practitioners usually refer to the specific combination of description, interpretation, and explanation as *thick description*. The philosopher RYLE (2009[1968]) coined the term, and the cultural anthropologist GEERTZ (1993 [1973]) systematised thick description and designated it as the core of ethnographic research practice (BEUVING & DE VRIES, 2015). In addition to condensed description and interpretation, *thick description* contains elements of style associated rather with novel writing—plot, protagonist, and dialogue—than with the straightforward writing up of observed facts (I later explain why the latter is problematic from an ethnographic viewpoint). The works of both GOFFMAN and MEAD raise the question of whether it is possible at all to serve up dry ethnographic facts. Are not the literary instruments of the novelist (i.e. fiction) necessary to make tangible the conjunction between the observed facts, the recorded stories, and the broader historical context of the studied problem?[15] Methodologically, this points to the question of whether facts are the exclusive result of observations that are merely reported or whether they are crafted in a process of coherent description and interpretation. If so, what are the confines of the description; does anything go or should certain guidelines be respected? [27]

This question also addresses the problem of describing what has not been directly observed. In an extreme form, it plays a crucial role in the controversy surrounding MEAD. She did not regard her Samoan informants' stories about their sex lives as preliminary hypotheses, but as social facts in and of themselves. "Coming of Age"'s well-known opening passage presents a case in point:

> "As the dawn begins to fall among the soft brown roofs and the slender palm trees stand out against a colourless gleaming sea, lovers slip home from trysts beneath the palm trees or in the shadow of beached canoes, that the light may find each sleeper in his appointed place" (MEAD, 1973 [1928], p.14). [28]

There can be little doubt about the aesthetic beauty of this wonderfully touching description, but, as FREEMAN (1983) made clear, there is also a considerable problem with it: MEAD never observed this (and many other) scenes—but did not explicitly acknowledge this in her work. The American psychologist BUSS concluded drily: "She apparently relied heavily on her female informants rather than on direct observations" (2001, p.961). KING (2019) presented as an interesting explanation the idea that the BOAS group understood the term *society* as a collection of rituals and customs. Consequently, the objects of observation are rituals/customs (in MEAD's case: the alleged liberal sexual practices of young, unmarried women) and not, as MEAD's contemporary SAPIR argued, individual persons or social practices (KING, 2019). GEERTZ wrote about this cleavage: "The locus of study is not the object of study" (1993 [1973], p.22). The difference he later analysed as a contrast between a generic and a

15 If only because the instrument itself is a distillate of the real-life experiences of generations of people and great writers—those who wrote the Bible, HOMER, SHAKESPEARE, WOOLF—who jotted down their experiences (BEUVING & DE VRIES, 2015, pp.118-122).

configurational model of culture (GEERTZ, 2000). For MEAD, informants were entrances into the understanding of ritual and custom, rendering their stories to a high degree interchangeable. [29]

The anthropology of ritual and custom ran into trouble over the so-called *TWO-CROW* problem. This methodological problem, handed down by the 19th century ethnologist DORSEY, was elucidated by SAPIR (1938) as follows. Every time DORSEY recorded some customary practice among the North American Omaha native Americans, invariably a local chief, TWO-CROW, threw a spanner in DORSEY's carefully construed notes by denying everything that other Omaha informants had declared with great confidence earlier on. Rather than omitting TWO-CROW's viewpoint with the argument that a single dissenting voice surely could not make a difference (common in BOAS' circles), DORSEY made the dissent central in the analysis. It is not known whether GOFFMAN ever heard of chief TWO-CROW (or of DORSEY for that matter), but she followed his steps in her own work. She described a society that was admittedly deeply influenced by the paramount presence of the judicial machinery, which resulted in the above-discussed self-fulfilling prophecy, but her work included elaborate descriptions of so-called *clean people*: inhabitants of the black ghetto who succeeded in staying clear of the police-courts-rehabilitation triad and led a relatively untouched life; often because they somehow succeeded in finding jobs outside the ghetto, GOFFMAN suggested (2014, pp.163-194). [30]

To return to the role of observation in ethnographic description, it appears more complex than a simple yes/no scheme suggests. For instance, what should be done with so-called composite observations of social situations that are observed as several fragments and later meshed together into one coherent description? GEERTZ' Balinese cockfight (1993 [1973]) presented a well-known example of this. GEERTZ spent many hours with the Balinese villagers during cockfights and, on that basis, he described the fight as an ideal-typical scene. The ethnographic description did not refer to one particular fight that GEERTZ observed; through it, one sees the many different parts that he observed, now integrated into a seamless whole (see also BEUVING & DE VRIES, 2015, pp.186f.). This exemplifies the working of a type of observation that can be understood only in relation to the pattern of interrelations in which their symbolic meanings are produced. Describing observations as separate events—i.e. as they occurred naturalistically—in that case violates the wish to explain the meanings of the interconnection: cockfights as a complex symbolic battle, in GEERTZ' case (1993 [1973]). [31]

The reception of GOFFMAN's work reveals yet another problem: outsider scepticism. Whereas MEAD's work met with unabated enthusiasm (cementing her later role as a public intellectual), GOFFMAN's had a much more varied reception: praising in some circles, critical and even condescending in others. Of central importance: did GOFFMAN actually herself see what she purported to have observed? An anonymous document of around 60 pages, posted on the internet, represented the most vicious of GOFFMAN's commentators: it accused her of fabricating various observations (ANONYMOUS, 2015). Interestingly, an

inverse relation suggests itself between the quality of the observation and the reception of the work. Whereas MEAD focused on the problems of adolescence in America and Samoa, riding a wave of public awakening in a sphere of emancipation and presenting a solution for a social problem of the day, GOFFMAN offered a gloomier message. GOFFMAN's penetrating analysis of the black underclass revealed an inconvenient truth that implicates America's political elite in the woes of American society—in an interview given to the on-line *Slate* magazine, her thesis supervisor ANDERSON stated: "[F]indings of (...) ethnographers are often doubted simply because to believe them is to confront some of society's most damning failures" (NEYFAKH, 2015, n.p.). [32]

4. Discussion: A Proposal for Rules of Thumb

> "We should hold ourselves to high standards, probably higher than we have in the past. But there are few, if any, rules and we will not escape intellectual and moral ambiguities. It would be a pity if we rejected an entire method [ethnography] because it shares in the messiness of the worlds it studies" (ZUSSMAN, 2016, p.443).

I have confronted the methodological question of what constitutes the intellectual foundations of ethnographic authority, breaking it down into three smaller questions: 1. Is (social) proximity a precondition for ethnographic work? 2. How can the unique observations of ethnographic fieldwork be tested? 3. How are facts crafted in ethnographic description? Without doubt, there is much more to say about these questions than I can cover in this article. However, I feel that the exposition thus far suffices to develop further thoughts about rules of thumb for the ethnographic method. [33]

A short clarification of the term *rules of thumb* is probably in order to avoid pedantry. First, I do not advocate unnecessary codification and/or formalisation of ethnographic research practice. Contextuality of methods, the capacity to think with ethnographic material, and fostering an iteration between theoretical ideas and empirical data (or theorising, see SWEDBERG, 2016) cannot be easily fitted into a rigid methodological straightjacket. Second, ethnography is a dish with many flavours, encompassing various research traditions, and I do not pretend to claim some universal understanding of it (AGAR, 2006). There is no intellectual consensus about what constitutes "good" ethnography (KUSENBACH, 2005)—and it is certainly not my intention to hammer out one here. The term, rules, should be interpreted in the sense of heuristic devices that are good for thinking, in this case about the complex web of fact and fiction in ethnographic work. Thinking about rules of thumb, in that sense, is an essential antidote to the statement that ethnography is just another opinion. I therefore strongly disagree with ZUSSMAN's above quote where he writes that there are "few rules". There are! And the GOFFMAN/MEAD controversies suggest the following three. [34]

4.1 Rule of thumb #1: Not all observations are ethnographic facts

Direct observation of social life constitutes the Holy Grail of ethnography (WILLIS, 2000). However, that is not the same as saying that every observation can be considered a fact. The processual character of ethnographic work comprises a special challenge: it is not possible to define *a priori* rules about what counts as fact, and what does not. That is an emerging insight that the methodologist KAPLAN aptly termed "logic-in-use" (1964, pp.14f.): rules for selection that are not fully defined (*operationalised* in more formal language) once and for all at the beginning of one's study but are rather formulated as tentative guidelines to be subjected to rigorous testing. There are no generally shared standards, but it appears that intensive collaboration grounded in empirical material is recommended. The anthropologists PRICE provided with their usual clarity an inspiring example in their recently published "Saamaka Dreaming" (2017). During fieldwork among Surinamese Marrons spanning several decades, they shared their fieldnotes and commented on each other's work in the form of extensive annotations. Their comments often provided indications for discussions about what they considered to be the central facts of the Surinamese *Samaaka* society. [35]

It is important to avoid the naïve idea of facts as self-evident carriers of information: society is not simply a collection of directly observable factoids. Paraphrasing the Enlightenment philosopher KANT, the British sociologist SWEDBERG (2016) argued that, in ethnographic research, observations become facts in a process of focused observation (see also LUHRMANN, 2020). This may sound like a mouthful, but maybe a comparison with music can simplify the statement. A listener versed in Western classical music can easily find him/herself at a loss when listening to microtonal music. Consider for instance Hindustani or Oriental music; the intricate details and nuances that a trained listener will readily pick up are presumably lost to Western ears. This suggests that tones in themselves do not make music; it is in their interconnection, or the musical *system*, where the tones function that they acquire their melodical or rhythmical meanings. Similarly, visual input in itself is not observation. The colour of a person's shoes is relevant in a society in which strong dress codes operate and a person's position or social status can be related to his/her style of dressing. In cases where dress codes are loose—or absent: consider the aforementioned nude beaches—such detail is less relevant; it becomes an observation that has no place in a wider frame of reference and is therefore not a social fact.[16] [36]

The problem of intellectual self-confirmation, or cherry-picking evidence while ignoring conflicting evidence (for instance for political purposes), plays a special role here. SHANKMAN (2009) has shown for the MEAD case that she was driven

16 Ethnographic observation is viewed by ethnographers as having a distinctive theoretical aspiration, but this does not mean that there are generally accepted standards for it. Nevertheless, it is possible to formulate *a priori* considerations. An important test is whether observations are becoming repetitive and whether a sense of prediction is growing about what will happen in a particular social situation. Moreover, SWEDBERG (2016) pointed out how theoretical considerations inform observation: observations should be directed at solving a particular knowledge problem. The questions that this problem-solving generate ought to direct the observation.

by social questions circulating in early 20th century America which motivated her to look for opiniated informants with strong stories. Possibly she even fostered a sphere of competition that encouraged her informants to exaggerate their experiences, but this cannot be reconstructed. MEAD portrayed in her work only women with stories about sexual autonomy; there was no room for modest stories and mundane experiences, even though the circumstantial evidence that her academic peers offered pointed strongly to the oddness of MEAD's findings. Compare this with GOFFMAN who, in addition to a string of informants whose stories underscored her argument of a police state as a self-fulfilling prophecy, also portrayed families who were part of that world but whose young men did not embark on a pattern of subversive, criminal behaviour. [37]

A related question is whether there has been a concerted effort to unearth *counterfactuals* in the field—observations that appear to refute one's unfolding interpretations. At this point, another abyss opens up between the work of MEAD and that of GOFFMAN. MEAD did not look for counterfactuals, and she therefore ignored the TWO-CROW problem. Moreover, MEAD regarded her informants as relatively interchangeable because individual persons, in her worldview, were symbolic vehicles of a dominant culture. To be sure, this view was typical of the anthropology of ritual and custom that thrived in the intellectual circles surrounding BOAS—which their contemporary SAPIR justifiably critiqued (KING, 2019, p.227). GOFFMAN, on the other hand, deliberately looked for contrast in the material: the *clean people* forced themselves upon her and forced her to reconsider the possibility of a life beyond the police state. GOFFMAN thus carefully attempted to delineate the culture that she recorded, showing how it was not totalising, as MEAD (and others) sought to do, but instead that culture did not rule out choice. [38]

To round off this point: the relation between observation and social fact can be focused more sharply by asking persons from the studied society to read ethnographic work about themselves. This may cast the material in a different light.[17] Dutch anthropologist SEUR (1992), for instance, invited farmers in Zambia to read ethnographic work that development anthropologist LONG (1968) had collected two decades earlier. LONG's central observation was that farmers affiliated to the local Jehovah's Witnesses church were more successful economically than non-members. LONG explained this with reference to their religious ethos with the argument that this fosters a capitalist entrepreneurial spirit such as posited by WEBER (1968 [1921/1922]). SEUR read the relevant passages to the farmers but discovered that they had run out of economic steam by then, while continuing to practice their faith. Was LONG wrong? Yes and no. His description of the facts corresponded with what farmers remembered from past times, but LONG's explanation did not fit well. The farmers explained that, as Jehovah's Witnesses, they were convinced that they had a guaranteed place in

17 After completing her fieldwork, MEAD never returned to Samoa and we do not know, therefore, how her informants in the context of those days would have responded to her interpretations. FREEMAN (1983) succeeded in tracing some of them long after MEAD had passed away, but their insistence that they had fooled MEAD at the time is probably a reconstruction *ex post facto*; it cannot be ruled out that their accounts were tainted by a wish to distance themselves—now respectable middle-aged women—from who they were earlier in their lives.

God's kingdom, in contrast to Protestants who doubted this and had to resort to their plan B of working hard during their lives. Economic success turned out to be a by-product of their religious ethos; correspondence without causation. [39]

4.2 Rule of thumb #2: Ethnographic writing and the correspondence mechanism

As argued above, ethnographic facts are produced in a process of systematic interpretation of observations, with far-reaching consequences for ethnographic writing. Ethnographic writing requires instruments from literature to make an interesting, coherent story that (hopefully) reaches an audience of readers. Resorting to the instruments of literature ought to be delineated by what I term here, with KAPLAN (1964, pp.312ff.) the correspondence principle; with correspondence referring to the classification of facts as that which can be observed in the real world of everyday life. A key question is: does the ethnographic description contain similar or additional observations as were recorded elsewhere, for instance in journalism and contemporary historiography? To be sure, this is not the same as saying that ethnographic work must be confirmatory of earlier thinking to be of scientific value; far from it. An important goal of ethnographic writing, as espoused in the previous discussion, must instead be to subject social phenomena reported elsewhere to alternative interpretation—in normal science *methodologese*: to *falsify*. In MEAD's work, as her critics have convincingly shown, correspondence was far from obvious, for instance in omitting the paramount presence of the Western, colonial order. [40]

The distinction between actor and informant is crucial here. Staging certain events, or nudging persons into behaviour that they would not have pursued otherwise, transforms informants into actors in a story that the researcher writes. Obviously, that is a far cry from making observations naturalistically: in an everyday setting. From the veranda where she generally received her informants, MEAD encouraged the young women with whom she had conversations to aggrandise and exaggerate mundane instances into full-blown adventures, FREEMAN (1983) observed. Presumably she was driven by a strong wish to prove the point of sexual autonomy, which she needed to hold up a mirror to the puritan sexual morality of America's middle-classes, her real audience. As KING (2019) noted, MEAD never wrote for Samoans; all along she had her American readership in mind. Thus, she transformed the young women into actresses in a morality tale. GOFFMAN (2014) dealt with this altogether differently. The closing scenes of "On the Run" make this brilliantly clear. She described how, on the evening that her key informant (and by then close friend) Chuck was killed, she drove around with her informants in the neighbourhood where Chuck was shot down. They intended to hunt down the alleged perpetrators and revenge Chuck's death. Ultimately, they returned home empty-handed, but not before GOFFMAN described with honest detail how she felt what her informants presumably felt: bloodthirstily revengeful. Importantly, GOFFMAN never orchestrated the search for Chuck's killers: she was a passenger in a tragic drama, and not an actor in it, nor did she direct the scene. From the context it follows that the search would

have been carried out irrespective of GOFFMAN's presence; her presence was a mere side issue. [41]

To suggest correspondence in ethnographic texts, it has become commonplace to attribute to the author/researcher a prominent (if not downright intrusive) role (ATKINSON, 2011 [1990], p.46): ethnographic texts that have the author at the centre of writing in an attempt to account for the researcher's reflexive positioning. An unresolved question, however, is whether such accounting has to be done in the writing itself and, if so, how that improves the quality of the ethnographic study. In another publication, I subscribed to an interpretation that is critical of inserting the "I" of the author in the ethnographic text as a credible solution for the correspondence problem (BEUVING & DE VRIES, 2015, pp.182-185). In addition, the GOFFMAN/MEAD controversy suggests that making the author central in the *thick description* may mask all that was not observed, and/or what was observed but could not be accounted for—the TWO-CROW problem discussed above. Moreover, if one is taking MALINOWSKI's (1978 [1922]) research agenda seriously—understanding society in terms of its indigenous symbolic categories, or taking the native point of view—one should actually avoid the authorial point of view, instead writing through the eyes of one's informants; or, to strengthen the ethnographic correspondence principle, to write from the viewpoints of one's informants, perhaps going as far as to write ethnography as if looking through the eyes of those one is studying, in a fashion that conjures up images of the stream-of-consciousness literature that acclaimed novelists such as WOOLF (2017 [1928] and PROUST (1996 [1913] pioneered around a century ago (incidentally, coinciding with the publication of MEAD's Samoa work). GOFFMAN's work suggests to write oneself as author in the text only if there is no other way to tell the story (see also PARRY, 2014). [42]

A final remark about the correspondence between interpretation and observation relates to context. Reading MEAD with 21st century eyes, one may easily be put off by the long-winded descriptions of tropical landscapes, the structure of Samoan villages, and the organisation of local agriculture and fisheries. Methodologically, there is nothing wrong with the long-windedness: baroque description was accepted in early 20th century anthropology circles. More disturbing, however, is the weak relation with her actual topic: the social position of the young women, including an analysis of their alleged sexual autonomy. This unveils yet another point: there may be correspondence but it is hardly relevant. In fact, one cannot escape the impression (which MEAD's main antagonist FREEMAN surprisingly failed to register) that her evocative descriptions served an entirely different purpose: not so much to embed her analysis in observed details as to project an image of primitiveness for consumption by a Western audience (KING, 2019). MEAD defended this in the preamble to her work by portraying Samoa as a so-called simple case of a society not yet tainted by the forces of modernisation; read: a purer form of social existence compared to America's own culturally diluted version of it. In GOFFMAN's case, conversely, context referred to the institutional environment in which her informants operated, especially that relating to the juridical/judicial machinery, whose detailed

descriptions were relevant because they constituted the empirical core of the self-fulfilling prophecy: the theoretical argument that GOFFMAN sought to develop. [43]

4.3 Rule of thumb #3: Ethnography as social theory development

Ethnographic writing has literary qualities (when practised well), but ethnographers add to that a special ambition, here aptly formulated in the words of veteran anthropologist HANNERZ (2010): to make the world transparent. The GOFFMAN/MEAD controversies suggest that, in order to achieve this ambition, comparison is of the essence. The term comparison is contested in the interpretive sciences, typically raising questions such as: what are the criteria for comparison and with what expected added understanding? I subscribe to the positive idea that scientific progress advances through comparison beyond the studied cases; otherwise, ethnography (and indeed any form of scientific enterprise) becomes bogged down in pointless casuistry. Anthropologists sometimes refer to this colloquially as the *Bongo-Bongo* problem.[18] When one is presenting one's work about some group, network, or society to some audience, invariable there will be someone, usually a senior colleague, who raises his/her voice challenging what one has just said with the claim: things are different with the Bongo-Bongo that I studied! Of course, all cases are different in their empirical detail, but for a fruitful comparison it stands to reason to rise above the level of description, instead looking for patterns (and deviations) on the more advanced level of the social mechanism/force that one sees in action in the studied case. [44]

This leads to the special problem of how ethnography, with its strong descriptive tradition, can contribute to social theory. Addressing this question goes to the heart of yet another one, in the vocabulary of normal social science: what does the ethnographic material represent? In the early development of modern ethnography, roughly in the first three–four decades after its foundation, there was an ideal of making comparisons based on empirical description. MEAD tweaked this in an ideologically motivated fashion to use her Samoan case in order to hold a mirror up to American society. As SHANKMAN wrote: "[MEAD] had written Coming of Age for Americans (...), present[ing] Samoans as potential models for minimizing the problems of American adolescence" (2009, p.135). MEAD skipped over the thorny question of how to compare premodern Samoa with a highly capitalist society characterised by a skewed division of labour fracturing along lines of gender, class, and race with an institutional form of religion functioning as the locus of sexual morality. She presented the comparison as unproblematic, whereas in reality there were fundamental problems ingrained in the comparison that crippled attempts to genuinely generalise from her case. [45]

With their method of constant comparison, pioneered in the 1960s, American methodologists GLASER and STRAUSS (2012 [1967]) offered a solution to the problem that MEAD (and many other contemporaries) had encountered of how to

18 *Bongo-Bongo* is of course not an acceptable term anymore as it can be interpreted as racist or otherwise offensive language.

generalise from ethnographic cases. They posited that comparison was key for scientific progress but on the level of theory, *not* through empirical means. Grounded theory methodology, as this procedure came to be known, is built via the procedure of the open coding of ethnographic material, constituting the raw material for building more abstract categories (sometimes also called axial codes), looking for connections between them with the purpose of inferring patterns that are more general than the machinations of the case in which they are discovered (RAGIN & AMOROSO 2011 [1994]). Often, this goes awry. Case-in-point: world-renowned Dutch-American primatologist DE WAAL (2007 [1981] published a brilliant book about the political practices of chimpanzees based on extensive fieldwork on the chimpanzee colony in Burgers zoo in the Netherlands. Some of his followers, however, projected his interpretations onto the behaviour of politicians in Dutch national politics. This is all well and good until one realises how context impedes serious comparison: a late-modern, capitalist, mass-mediatised society versus a small-scale, illiterate animal society—radically different in terms of their respective social *Eigendynamik*. [46]

Arriving at the point of theoretical comparison, one can see how GOFFMAN was more convincing than MEAD. MEAD considered the numerous and considerable differences between Samoa and the US unproblematic, viewing them as epiphenomenal to the social process of puberty that she studied, apparently believing that what she discovered in her Samoan case could be implemented in another society—social engineering *avant la lettre*. GOFFMAN did not attempt empirical comparison. She could have made passing references to, say, the Latin American *favelas* or the black townships in South Africa under *Apartheid*. Instead, she attempted to tease out the social mechanisms that kept the Afro-American underclass more or less locked up in a police state: the massive presence of police and other law enforcers, including the widely felt fear that they invoked, did not reduce but rather magnified criminal behaviour. The black ghettos kept people trapped in an underground, illegal economy that ultimately resulted in a confrontation with the police, ending with imprisonment—or worse: George FLOYD's death is a tragic case-in-point. [47]

5. Conclusion

With this article, I have tried to contribute to rebutting the assertion that the social sciences in general, and ethnography in particular, are just one opinion among many others—a central tenet of post-truth thinking. I formulated three methodological rules of thumb on the basis of a juxtaposition of two ethnographic controversies: MEAD's "Coming of Age" (1973 [1928]) in Samoa and GOFFMAN's "On the Run" (2014). In summarised form, these rules boil down to a critical reflection about what constitutes scientific facts, how we must report them, and how we can generalise from them. Taken together, they form an important methodological core of the ethnographic enterprise. I analysed this core from an interpretive position: finding out about society based on focused observation and including an understanding of how its members see it. That is not to say that discovering the facts of society is either simple or straightforward. I warned that construing ethnographic observation as social fact without

scrutinising the wider frame of reference in which it is produced undermines claims to ethnographic veracity. From this follows another methodological dead-end that occupied ethnographic thinking in a previous decade: considering not the production of ethnographic facts per se but debating how ethnographic researchers produce "auras of factuality" in their writing (VAN DER PORT, 2017, p.295). Doubting the possibility of facts does nothing but stoke the injurious fire of post-truth thinking. The interpretive position that I advanced in this article instead subscribes to a distinction between facts as they emerge in the context of fieldwork and are reported in a coherent narrative, and the interpretation of those facts, to be subjected to outside scrutiny. Nonetheless, building a common world of facts continues to present a special challenge in a post-truth world where fact and fiction quickly amalgamate into something unsavoury. The need for a truthful ethnography appears to be greater than ever. [48]

Acknowledgements

An earlier, shorter version of this text was central in a debate on fact and fiction in ethnography, published in the Dutch-Flemish academic journal *Kwalon*. I thank Dr. Roy GIGENGACK, Dr. Hans MARKS, Prof. Dr. Toon VAN MEIJL, Dr. Bowen PAULLE and Prof. Dr. Fred WESTER for their responses to this contribution, many of which found their way into this article. I furthermore thank my academic mentors Prof. Dr. Jan Kees VAN DONGE and Dr. Geert DE VRIES for their guidance and intellectual stimulation over the past fifteen years during which the ideas for this article were formed. I also thank the editors of *FQS* as well as the reviewers for their stimulating comments. Finally, I thank my partner Dr. Tessel JONQUIERE for making this article more readable, and Catherine O'DEA for language editing.

References

Agar, Michael (2006). An ethnography by any other name. *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, *7*(4), Art. 36, http://dx.doi.org/10.17169/fqs-7.4.177 [Accessed: September 15, 2020].

Anonymous (2015). *Accusations of Alice Goffman's dishonesty*, https://pastebin.com/BzN4t0VU [Accessed: September 18, 2020].

Athens, Lonnie (2010). Naturalistic inquiry in theory and practice. *Journal of Contemporary Ethnography*, *39*, 87-125.

Atkinson, Paul (2011 [1990]). *The ethnographic imagination: Textual constructions of reality*. London: Routledge.

Barth, Fredrik (2005). From the Torres Straits to the Argonauts, 1898–1922. In Fredrik Barth, Andre Gingrich, Robert Parkin & Sydel Silverman (Eds.), *One discipline, four ways: British, German, French, and American anthropology* (pp.11-21). Chicago, IL: The University of Chicago Press.

Becker, Howard (1963). *Outsiders: Studies in the sociology of deviance*. New York, NY: The Free Press of Glencoe.

Becker, Howard in conversation with Keller, Reiner (2016). Ways of telling about society. *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, *17*(2), Art. 12, http://dx.doi.org/10.17169/fqs-17.2.2607 [Accessed: September 15, 2020]

Beuving, Joost (2017). The anthropologist as jester, anthropology as jest?. *Social Anthropology, 25*(3), 353-356.

Beuving, Joost (2019). Ethnography's future in the age of big data. *Information, Communication, and Society,* https://doi.org/10.1080/1369118X.2019.1602664 [Accessed: May 24, 2020]

Beuving, Joost & de Vries, Geert (2015). *Doing qualitative research: The craft of naturalistic inquiry*. Amsterdam: Amsterdam University Press.

Boas, Franz (1884). The eskimo of Baffin land. *Transactions of the Anthropological Society of Washington* 3, 95-102.

Buss, David (2001). *The dangerous passion: Why jealousy is as necessary as love and sex*. New York, NY: The Free Press.

Clifford, James & Marcus, George (1986). *Writing culture. The poetics and politics of ethnography*. Berkeley, CA: University of California Press.

Crapanzano, Vincent (2004 [1996]). Hermes' dilemma: The masking of subversion in ethnographic description. In Jon McGee & Richard Warms (Eds.), *Anthropological theory: An introductory history* (pp.593-608). Boston, MA: McGraw-Hill.

De Waal, Frans (2007 [1981]). *Chimpanzee politics: Power and sex among apes*. Baltimore, MD: Johns Hopkins University Press.

Douglas, Jack (1973). *The nude beach*. New York, NY: Sage.

Driessen, Henk & Janssen, Willy (2013). The hard work of small talk in ethnographic fieldwork. *Journal of Anthropological Research*, *69*, 249-263.

Elias, Norbert (1998 [1939]). *On civilization, power and knowledge* (ed. by S. Mennell & J. Goudsblom). Chicago, IL: The University of Chicago Press

Fabian, Johannes (1990). Presence and representation. The other and anthropological writing, *Critical Inquiry, 16*(14), 753-772.

Forman, John (2017). *Locking up our own. Crime and punishment in Black America*. New York, NY: Farrar, Strauss and Giroux.

Frazer, James (2016 [1890]). *The golden bough: A study of magic and religion.* Scotts Valley, CA: Createspace Independent Publishing Platform.

Freeman, Derek (1983). *Margaret Mead and Samoa: The making and unmaking of an anthropological myth*. Cambridge, MA: Harvard University Press.

Geertz, Clifford (1993 [1973]). *Interpretation of cultures*. New York, NY: Basic Books.

Geertz, Clifford (2000). The world in pieces: Culture and politics at the end of the century. In *Available light: Anthropological reflections on philosophical topics* (pp. 218-263). Princeton, NJ: Princeton University Press.

Glaser, Barney & Strauss, Anselm (2012 [1967]). *The discovery of grounded theory: Strategies for qualitative research*. Chicago, IL: The University of Chicago Press.

Goffman, Alice (2014). *On the run: Fugitive life in an American city*. Chicago, IL: The University of Chicago Press.

Goffman, Alice (2015). *How we are priming some kids for college and others for prisons*. https://www.ted.com/talks/alice_GOFFMAN_how_we_re_priming_some_kids_for_college_and_others_for_prison [Accessed: September 16, 2020].

Hannerz, Ulf (2010). *Anthropology's world. Life in a twenty-first century discipline*. London: Pluto Press.

Hempenstall, Peter (2017). *Truth's fool: Derek Freeman and the war over cultural anthropology*. Madison, WI: The University of Wisconsin Press.

Kaplan, Abraham (1964). *The conduct of inquiry: Methodology for behavioral science*. New York, NY: Chandler.

King, Charles (2019). *Gods of the upper air: How a circle of renegade anthropologists reinvented race, sex and gender in the twentieth century*. New York, NY: Doubleday.

Kuhn, Thomas (2012 [1962]). *The structure of scientific revolutions*. Chicago, IL: The University of Chicago Press.

Kuper, Abraham (2005). *The reinvention of primitive society: Transformation of a myth.* New York, NY: Routledge.

Kusenbach, Margarethe (2005). Across the Atlantic: Current issues and debates in US ethnography. *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, *6*(3), Art. 47, http://dx.doi.org/10.17169/fqs-6.3.18 [Accessed: September 15, 2020]

Lewis-Kraus, Gideon (2016). The trials of Alice Goffman. *New York Times Magazine*, January 12, https://www.nytimes.com/2016/01/17/magazine/the-trials-of-alice-goffman.html [Accessed: September 17, 2020].

Long, Norman (1968). *Social change and the individual. A study of the social and religious responses to innovation in a Zambian rural community*. Manchester: Manchester University Press.

Lubet, Steven (2015). Ethics on the run. *The New Rambler Review*. https://newramblerreview.com/book-reviews/law/ethics-on-the-run [Accessed: September 17, 2020].

Luhrmann, Tania M. (2020). On finding findings. *Journal of the Royal Anthropological Institute*, *26*(2), 428-432.

Malinowski, Bronisław (1978 [1922)]. *Argonauts of the Western Pacific. An account of native enterprise and adventure in the archipelagoes of Melanesian New Guinea*. New York, NY: Routledge.

Mead, Margaret (1973 [1928]). *Coming of age in Samoa: A psychological study of primitive youth for western civilization*. New York, NY: William Morrow.

Neyfakh, Leon (2015). The ethics of ethnography. *Slate*, June 18, https://slate.com/news-and-politics/2015/06/alice-goffmans-on-the-run-is-the-sociologist-to-blame-for-the-inconsistencies-in-her-book.html [Accessed: September 24, 2020].

Parry, Marc (2014) *Life on the run. A rising star in sociology chronicles the human costs of America's penal system*, Princeton Alumni Weekly, March 19, https://paw.princeton.edu/article/life-run [Accessed: September 24, 2020]

Pollman, Tessel (1990). Margaret Mead's Balinese: The fitting symbol of the American dream. *Indonesia*, *49*, 1-35.

Price, Richard & Price, Sally (2017). *Saamaka dreaming*. Durham, NC: Duke University Press.

Proust, Marcel (1996 [1913]). *Swann's way*. London: Penguin.

Ragin, Charles & Amoroso, Lisa (2011 [1994]). *Constructing social research: The unity and diversity of method*. New York, NY: Sage.

Ryle, Gilbert (2009 [1968]). The thinking of thoughts: What is "le penseur" doing?. In Julia Tanney (Ed.), *Collected papers* (Vol. 2, pp. 494-510). London: Routledge.

Sapir, Edward (1938). Why cultural anthropology needs the psychiatrist. *Psychiatry: Interpersonal and Biological Processes, 64*(1), 2-10.

Seur, Han (1992). The engagement of researcher and local actors in the construction of case studies and research themes. In Norman Long & Anne Long (Eds.), *Battlefields of knowledge: The interlocking of theory and practice in social research and development* (pp.115-143). New York, NY: Routledge.

Shankman, Paul (2009). *The thrashing of Margaret Mead: Anatomy of an anthropological controversy*. Madison, WI: University of Wisconsin Press.

Simmel, Georg (1950). *The sociology of Georg Simmel (*transl. ed. and with an introduction by K.H. Wolff). Glencoe, IL: The Free Press.

Singal, Jesse (2015). The internet accused Alice Goffman of faking details in her study of a black neighborhood: I went to Philadelphia to check. *The Cut*, June 18, https://www.thecut.com/2015/06/i-fact-checked-alice-goffman-with-her-subjects.html [Accessed: September 18, 2020].

Swedberg, Richard (2016). Before theory comes theorizing or how to make social science more interesting. *British Journal of Sociology*, *67*(1), 5-22.

Time (2020). The 25 most influential women of the past century, *Time Magazine*, http://content.time.com/time/specials/packages/completelist/0,29569,2029774,00.html [accessed: September, 24, 2020)

van der Port, Matthijs (2017). The verification of ethnographic data. A response. *Ethnography*, *18*(3), 295-299.

van Maanen, Niels & de Rond, Mark (2017). The making of a classic ethnography: Notes on Alice Goffman's *On the Run*. *Academy of Management Review*, *42*(2), 396-407.

Wacquant, Loic (2009). Urban outcasts: Stigma and division in the black American ghetto and the French urban periphery. *International Journal of Urban and Regional Research*, *17*(3), 366-383.

Weber, Max (1968 [1921/1922]). *Economy and society: An outline of interpretive sociology* (ed. by G. Roth & C. Wittich). Berkeley, CA: University of California Press.

Willis, Paul (2000). *The ethnographic imagination*. London: Polity Press.

Woolf, Virginia (2017 [1928]). *Mrs. Dalloway.* London: Albatross.

Zussman, Robert (2016). Alice's adventures in wonderland: On the run and its critics. *Sociology, 53*, 436-443.

Author

Joost BEUVING is a lecturer and senior researcher in anthropology and development studies. His research interests are in the sociology of everyday economic life and in qualitative research methodology. He conducts ethnographic fieldwork in various places in Africa and Europe. With Dr. Geert DE VRIES, he published "Doing Qualitative Research. The Craft of Naturalistic Inquiry" (2015, Amsterdam: Amsterdam University Press).

Contact:

Joost Beuving

Radboud University Nijmegen
Nijmegen Centre for Social and Cultural Science
Montessorilaan 3, 6500 HE Nijmegen, The Netherlands

E-mail: j.beuving@maw.ru.nl

Citation

Beuving, Joost (2021). Problems of Evidence in Ethnography. A Methodological Reflection on the Goffman/Mead Controversies (With a Proposal for Rules of Thumb) [48 paragraphs]. *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, *22*(1), Art. 1, http://dx.doi.org/10.17169/fqs-22.1.3567.