

‘Unique Adequacy’ in Studies of the Military, Militarism and Militarisation

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ABSTRACT

This paper addresses the relative lack of studies of military phenomena by ethnon-methodology and conversation analysis (EMCA). It focuses in particular on Garfinkel’s unique adequacy requirement of methods – the utility of which is argued still remains - and addresses the perceived (and actual) limitations of a researcher’s absence of first-hand ‘military’ experience may raise. It argues ‘limitations’ can potentially be addressed through reflection upon what constitutes a military phenomenon and what corresponding uniquely adequate familiarity the researcher therefore may have. When issues of correspondence still remain, it is suggested (and illustrated) that creative EMCA methodologies can frequently overcome them through the judicious use of various data collection practices and analysis in light of that assessment.

The ultimate aim of this paper is to suggest ways of opening-up to greater ethnomethodological scrutiny under-researched phenomena of military, militarism and militarisation practice. An important additional aim is to illustrate that methodological attention to ‘unique adequacy’ can usefully be deployed in the research design of non-ethnomethodological formal analytic studies of military phenomena (and indeed non-military phenomena): Critical Military Studies is used as perspicuous example of this.

1.0 INTRODUCTION

In what follows ‘unique adequacy’ (UA) awareness is proposed as a positive, if mundane, research enhancing practice of reflection on the selection of phenomenon for study and ensuing research methods and analysis in light of their relevant ‘members’ knowledge’ and/or ability to obtain it. This paper is not intended as a contribution to the ‘methodological reflexivity’ literature now so abundant in the

epistemological and methodological literature.¹ Since UA is so central to this paper, it will prove useful to start by immediately outlining how it is being interpreted here, even before discussing the issue of ethnomethodological and conversation analysis (EMCA) studies of military phenomena and militarism.

1.1 THE UNIQUE ADEQUACY REQUIREMENT OF METHODS (‘UNIQUE ADEQUACY’)

UA will, as noted, be central to the discussion that follows. Contrary to many ethnomethodologists, I believe UA still has a central role to play in the understanding of what ethnomethodology is, and what ethnomethodological researchers deliberately take into consideration in doing such research.² A fuller debate is for another occasion (Jenkins forthcoming), and here – if controversially for some – it is taken as read that UA is relevant to all ethnomethodological studies, and so will continue on that basis.

So, if the researcher is to understand the production of social action and order from a members’ perspective, Garfinkel sees UA not just as an important and desirable individual and methodological attribute, but as a necessary policy (2002 p175, see also Garfinkel and Wieder 1992). Social order is here seen as locally produced and emergent from on-going interaction and to be explained as such, rather than from the imposition of a formal explanatory theory used to interpret the action and the social order produced (as would be the case with a ‘strong

¹ ‘Methodological reflexivity’ which, as Lynch (2000) has argued is for ethnomethodology (and other disciplines) an unnecessary formalization of ordinary ubiquitous practices. Ethnomethodological reflexivity is for Macbeth (2001: 50) not a methodological technique but a members practice with “...no distinctive professional or methodological attachments”, a “constitutive reflexivity” that “dissolves into the practical achievements of diverse settings, occasions and practices.” (Ibid. 55) Lynch (ibid) provides a - necessarily incomplete - critical typology of modes of analytic reflexivity, while Macbeth contrasts positional and textual reflexivity, and both authors contrast this against ethnomethodological constitutive reflexivity. This does not mean that researchers should not be reflective practitioners in their choice of phenomenon, research methods and, as I argue in this paper, consider their ‘unique adequacy’. My thanks to an anonymous referee for finessing my use of ‘reflexive’ here.

² Michael Lynch (1993) has, read as ‘dismissing’ the significance of UA for ethnomethodology on the basis that it is merely like a ticket that gets one in through the door of ethnomethodology and be discarded there upon. Unfortunately, I think this has been misread as a down-grading of UA when, I would argue, it so key to ethnomethodology that it remains a normative assumption of understanding, if rather confusedly understood and not given deliberate and nuanced consideration it deserves (Jenkins forthcoming). Therefore, to downgrade its importance as an aspect of ethnomethodological practice and understanding for those approaching ethnomethodology for the first time, i.e. those purchasing the ticket via their apprenticeship as students of ethnomethodology, is ‘unhelpful’ and in need of a response (ibid.).

version' of 'militarism'). The imposition of an interpretative framework of meaning, rather than the analysis of the local production of meaning by members, results in a misrepresentation of that action and the order it is producing, via an extant meaning it has for social theory instead; and thus 'ironicising' members' actions (a good explication of this applied to research practice is Sormani 2014). A theory driven approach necessarily passes over the details of the local production of that phenomenon in their own terms by members. So, a top-down application of theory in the research process needs to be avoided, and even 'respecified' as a research topic. However, an accurate understanding of the members' practices in the local production of situated order, i.e. phenomena, needs to be, indeed can only be, understood by those with competencies similar to, or the same as, those of the members themselves. Garfinkel formulated this as 'the unique adequacy requirement of methods'. Alongside this is the correlate that the data collection methods are to be appropriate to the practices of the members of the phenomenon which the researcher was investigating and 'competent' in. Significantly, this implies that a member's competency, i.e. UA, is needed to determine the appropriate data collection methods to be used, i.e. the research methodology.

Dirk vom Lehn (2014) succinctly re-states UA as requiring: "the researchers to fully embed themselves into social activities and acquire the competence and skills of the participants in order to be able to understand and pursue the activities just like the participants themselves." (p106) Key is that UA is grounded in the acquisition of practical familiarity and adequacy as demonstrated through performance *in situ* not by 'biography', i.e. competency is not equitable to individual 'biography' but related to it via competences (the issue of 'biography' will be addressed again below). The 'strong' version of UA, vom Lehn continues: "is based on the assumption that ethnomethodologists [and any other researchers] can make sense of others' activities only when they become members, i.e. participants, in the situation and experience it with their own bodies, just like those who usually work in and experience the situation." (p106) Of course, researchers may already be 'members' with UA.

Thus, the embodied practices of 'members' knowledge' are tied to the selection of methods, and the use of explanatory forms that are emergent from within the phenomena and the researcher's ability to do so with at least 'weak unique adequacy' (Garfinkel 2002), is assumed key. Significantly, 'membership' is not a formal pre-existent category like 'serving soldier', but a mutually accomplished status with others in activity, where 'competency' is performed, displayed, and recognised through the 'performance' of activities in a 'form of life', where UA is on-goingly accomplished. Thus, there is an emphasis on the researcher embodying the knowledge of the members whose practices, as found in naturally occurring situations, they wish to give an account of.

There is little doubt that this reads like a stringent requirement, and in the case of certain specialist activities, e.g. laboratory science, it is. But it is worth pointing

out that not all spheres of social life are so 'exclusive' and 'specialised'. Although ethnomethodology has had a tendency to look at 'professions' (see Anderson and Sharrock 2017 for a critique of this tendency), and their 'rarefied' work practices, to an extent that general sociology with its focus on contemporary culture, gender, race and social class often has not. The point being that not all forms of interesting phenomena are so removed from the shared competencies of social researchers as competent members of society, as that of laboratory science, or indeed kinetic warfare in the armed forces, may be. Additionally, much of the work of laboratory science or soldiering may be constituted by mundane practices of 'ordinary society' of which researchers actually do have UA – this is not to undermine the need for specific UA relating to those professions, but to note not all phenomena are 'specialist', nor is all aspects of competency in specialist activity 'specialised'.

1.2 UNIQUE ADEQUACY: BIOGRAPHY AS COMPETENCE

As noted above, for Garfinkel 'biography' is not enough, it is competence that matters. So, it must be made clear from the start that biography is here treated as a metaphor for the practical skills and knowledge participant members have acquired competences in throughout their life and constitute their varied UA. That UA though should not be viewed as inherently 'theirs', or 'individual' in a simplistic fashion, because these skills and practices are inherently social and largely meaningless outside their use in the situated courses of practical action in which they are contingently emergent. However, we operate in a culture of individualism and of assumed individual agency and therefore we tend to understand 'skills' as individualised in nature rather than social. While I have adhered to that tradition here the non-individualist nature of 'uniquely adequate' practice should be borne in mind as it is what lies behind the 'biography' metaphor and what makes being 'uniquely adequate' not simply due to a formal 'membership' an individual may have. UA as applied to research practice involves reflection towards their 'biography' in terms of practical skills for participation in, or their relevance to, the research phenomenon is what will be suggested as significant here.

The significance, or otherwise, of the researcher's biography and skills is variable, but rarely insignificant for sociological research, especially qualitative research with its relationship to the researcher's ability to engage with participants and understand the observed phenomena. The significance of the researcher's UA is largely conditional on their research project and any situated interaction with others in the emergent practices of data collection and any analysis engaged in. The researcher's biography and their autobiographical self-awareness can, and I would argue, should play a major consideration in all aspects of the research process – and not just for ethnomethodology. Ethnomethodology of course, and as noted above, emphasizes the significance of a researcher's biography in terms of their membership and 'members' knowledge' of the group of practitioners and related

phenomena to be investigated: although knowledge is not meant as a in individual store for facts but more of an ability to engage in social action, i.e. ‘unique adequacy’. This is formulated as the ‘unique adequacy requirement of methods’ and is, I suggest, a key tool to on-goingly reflect upon our research design, data collection and analysis practices, and thus increase the veracity of our research. It should not though be interpreted in such a way as to simply prevent research, but as a way to direct us to better and creative methodologies through its consideration.

1.3 ETHNOMETHODOLOGY AND MILITARY PHENOMENA

Garfinkel, like many of his generation, served in the Second World War: drafted into the Air Force, he designed teaching strategies for anti-tank small arms firing. Yet Garfinkel never took up military phenomena as an ethnomethodological research topic himself and nor have many other ethnomethodologists. For the latter, possibly due to the apparent problems of access to phenomena and perceived lack of commensurate members’ knowledge, i.e. unique adequacy. This, it will be argued, is an absence in need of redress. It is certainly true that most researchers, unlike Garfinkel, will lack any sense of what it is like to be in the military – the vast majority will not have served in their armed forces, and are unlikely to serve in the future. However, I believe this is not the insurmountable obstacle it might initially appear to be. If we take ‘militarism’ in general, instead of ‘the military’ in specific, as our phenomena then we have a starting point within our UA, i.e. our own understandings of the role, organisation and practices of the military as a feature of our societies and cultures, from which to develop studies. It may seem that adopting this approach might lead to ironising the phenomena we then look at, because we will apparently adopt an ‘external’ perspective on the ‘internal’ features of ‘military’ practices and militarism. But, it will be suggested here, that this is better than excluding ourselves from investigating ‘military’ phenomena due to a perceived lack of ‘unique adequacy’. By adopting the ‘inclusionary’ rather than the latter ‘exclusionary’ approach, we will not rule out all ‘military’ phenomena from our investigations due to the potentially unfounded and unnecessary assumption that military personnel are the only ones with UA, i.e. who can claim an understanding of ‘the military’³ and its manifestations in society.

One reason that the exclusionary assumption continues to have traction is anti-military disciplinary history of the social sciences, especially sociology (Joas 2003

³ This point, in many respects, parallels the insistence in early ethnographic work in the field of science and technology studies that scientific practices should not be accorded, a priori, specialised treatment based on a privileging of their epistemic authority. This is not to say science, like military affairs, is not a domain characterised by specialised practices. It is, however, to reject the idea that a researcher needs to be a scientist (or a soldier) to understand those practices. If that were the case, no one could learn to be a scientist or a soldier.

and Joas and Knöbl 2013). Additionally, much social science research, and indeed wider societal discussions and debates, simply presupposes a binary division between civil and military communities: a binary division which is not so much a 'reality' as it is an ideological positioning of the social sciences and indeed many Western democracies. While it is inconsistently held to, the orienting assumption is that the military is somehow separate from society, rather than in and of it, and by adopting this perspective we have been consistently ironising 'the military', and indeed many aspects of 'civil society'. Brown (2008) has highlighted a similar problem in anthropology: "...in so far as anthropology retains its commitment to harness empirical evidence to deconstruct essentialism, such morally tinged certainty about the military "other" demands some investigation of its own." (p450) If, however, we do not make these a priori assumptions, we can instead treat what members do and do not know, and what counts as knowing in this domain, as itself a topic of inquiry.

This is a more useful, and indeed honest, approach because, in several ways, we are all participants in militarism (even if we position ourselves against it) due to our – often unacknowledged – membership of militarised Western democratic societies. We are continually exposed to, and participate in, various 'military phenomena', if not always in the form of the uniformed military service personnel themselves. When we consider our 'membership' in this light, our UA, while not that of say a combat soldier, is nonetheless potentially considerable. Further, by not treating the military as the binary opposite of, and thus radically different to, civil society from the outset it becomes possible to investigate where the actual practices of 'the military' link into, rather than depart from, the generic practices of much of the rest of society and the orders of organisation which constitute it. If this reassessment is applied to situations understandable as 'military' (but in a broad non-ironic sense of 'military'), we can bring our ordinary social competencies, some of which include those of our inherent 'militarism', into the selection and analysis of our research phenomenon. This requires a clear and considered assessment of our capacities, our UA, rather than an unconsidered assumption based upon a normative ideological orientation towards 'the military' derived from sociology and other cultural frameworks. It is against this background that this paper explores how we may approach this task. It does so by looking at some examples from ethnomethodological studies themselves, asking how they may facilitate us to rethink our approach to 'things military', both in ethnomethodology and its cognate disciplines, but also in more orthodox formal social science research. The focus is on the role that unique adequacy may have in facilitating a non-ironicised research agenda.

2.0 UNIQUE ADEQUACY AND ‘MILITARY MEMBERS’

In the cautionary terms of Harold Garfinkel (1967), members of society, our phenomena and collaborators, are not to be treated as ‘judgemental dopes’, i.e., as unreflexive regarding their own internalised norms (see also Garfinkel 2006 p128). Garfinkel was referring to a general problem in sociology and, while great improvement has occurred in many sociological fields with regards to this criticism, I would argue this has not been the case when it comes to sociological representations of military phenomena and military personnel in particular. The majority of studies of the military still rely on quantitative analyses of survey and other similar secondary data resources (Williams et al 2016). This is not to deny their ‘utility’ as research methods, and my colleagues and I have used such data in multi-method research projects (Woodward, Jenkins and Williams 2015), but they remain problematic. By addressing the lacuna of ethnomethodological studies of ‘military phenomena’ – and in doing so respecifying theoretical constructs such as ‘militarism’ as well as the normalised idea of a simple civil-military binary – we can arrive at alternate understandings of these important social phenomena. As Brown (2008) notes this binary was reinforced with the development of the professionalised voluntary armed forces in the West. But may decline with the increasing reliance on Reservists by Western militaries since the turn of the millennium (see Jenkins et al 2018).

Anthropologist Kenneth T. Macleish (2013) in his ethnography “Making war at Fort Hood: Life and uncertainty in a military community”, observes (and for researchers can be said to warn), “Soldiers may be utterly subjected to a system of extreme control and discipline, but their experience is neither reducible to nor extricable from that system” (p25). Macleish also cautions us not to err in the opposite direction: “To describe the official form and order of Army life is not actually to portray that life. But it would also be wrong to think that the regulations, orders, constraints, and earnest straight-laced corporate culture are simply a restrictive veneer beneath which “real” life transpires” (Ibid p27). Macleish promotes a view where military personnel are seen as individuals working in an institution where they are not automatons without agency, but at the same time have to be recognised as working within institutional settings which provide the ‘context’ for their interactions and practices: contexts which they constitute through those actions. While Macleish cautions against focusing on the individual(s) at the exclusion of the formal structures, that both constrain and enable the production of their micro-contexts and interactional practices, he rightly indicates that we need to understand the ‘institutional context’.

It is at this point where, especially in EMCA, the researcher may then decide that, as non-members of the military, they do not have the ‘unique adequacy requirement of methods’ (Garfinkel 2002) to investigate those worlds as competent members. While there is some truth in such an assumption, matters are not as clear

cut as they are often thought to be. Much of the perceived 'uniqueness' of the military is 'over emphasised' (for various reasons) and if investigated, the forms of practice which characterise military organisation would, I suggest, be more readily understandable by 'ordinary members of society' than is currently recognised. This is because the military and its personnel too are more ordinary, rather than exotic and esoteric, than generally depicted. Much of 'military life' is entirely mundane and populated with roles similar to that of the civilian society in which it is both embedded and a part. For example, the UK's Royal Air Force run RAF Brize Norton's military passenger terminal very like a civilian one, it just happens to be staffed by RAF personnel who take on the roles recognisable in civilian airports: and civilians can and do navigate through it successfully. There are many other potential examples where the military modus operandi is similar to the civilian equivalent. Indeed, this is an ever-increasing phenomenon as the military attempts to attract and retain personnel by offering civilian qualifications for its 'military training' and in doing so adopts and develops its practices to conform to the certification requirements – as well for reasons of adopting 'best practice'. As large-scale organisations, the routines and problems of the various branches of the military are much the same as other large-scale organisations including ensuring, for example, that the people who work for them get paid and on a predictable, regular basis. Few would argue it would be impossible for us to understand pay-day (or an air terminal) simply because they were in military organisation context.

Obviously, we should not go too far the other way and assume that 'all' aspects of 'military life' are understandable on exactly the same terms as civilian life. Ethnomethodologically though, the presence of mundane orders of organisation within this 'specialised domain' suggests we should not necessarily be too deterred by any perceived lack of 'members' knowledge' of 'the military', i.e. unique adequacy, either. We must both address those aspects of military affairs we might be said to be already familiar with, and develop methodologies for studies of phenomena where that is not the case: in both cases by considering 'unique adequacy' as a methodological requirement and considering how to assess it in relation to the choice of 'military phenomena' investigated. Practicing this form of understanding ensures that studies of 'the military' free themselves from the ironicising practices of formal sociology, practices I have argued elsewhere (Jenkins et al 2018) are grounded in an awareness of the ideological relationship of modernist sociology to 'things military' (Joas 2003 and Joas and Knöbl 2013) and the culture of the 'military' as 'other' in the perceived binary of a civil-military binary.

I suggest it worth remembering too is that there are a vast number of non-academic studies⁴, frequently personal accounts, using an author's UA, for instance

⁴ There are of, course, a range of academic studies to draw on. In relation to them, it must be remembered that nowhere does Garfinkel suggest that researchers outside ethnomethodology do not have 'unique adequacy' with regards to their chosen phenomena of investigation, only that their data collection techniques

in the genre of military memoirs that we can draw on as ‘resources’, while being aware of inherent limitations (Woodward and Jenkins 2018). Here Privates to Generals, both serving and retired, have grounded their accounts of military life in acquired knowledge of its practice, in and out of combat, as well as post-service life. These accounts, while the subject of sociological and other forms of inquiry (Duncanson 2009 & 2013, Hynes 1998, Harari 2008, Jenkins and Woodward 2014a, 2014b., Woodward and Jenkins 2011, 2018, Ramsey 2011, Dyvik 2016a, 2016b), are not formally within the sociological canon, nor accounts undertaken by ‘trained sociologists’ addressing the sociological literature. This lack of ‘sociological rigor’ may be more of an issue for formal sociology than ethnomethodology however, and the boundaries are not always clear-cut as some do come close to being formal sociological ethnographic accounts, for example Sebastian Junger’s (2010) *War*. Indeed, to reiterate the issues discussed above, but with regards to these accounts, UA does not have to only entail experience of direct military service, as Junger illustrates with regards to ‘combat’, and as studies undertaken by current and former wives and family members of serving and ex-service personnel regarding ‘military lives’ also illustrate (Jervis 2016 and Jenkins, Murphy and Woodward 2016). These other accounts expand the pool of ‘uniquely adequate’ accounts from soldiers to other family members where military family life is the phenomena. Non-military personnel thus also fulfil the UA requirement and thus the study of the military, and especially militarism, can be demonstrated to be not just the province of serving or former military personnel – and this is a key point regarding our own UA. While this is generally accepted, it needs noting as it opens up potential phenomena as well as the UA required to investigate them. However, serious consideration is still required in our choice of phenomena, methodology and capacity to undertake these studies ‘non-ironically’. How this can be done, I suggest, can be informed by looking at some ethnomethodological studies of military phenomena.

Finally, a key point worth emphasizing is that ethnomethodologists usually study a phenomenon having first become a sociologist or cognate discipline member, and phenomena to be researched ‘member’ second. For example, an ethnomethodologist wishing to understand experimental physics will, like Sormani (2014), gain access to the sites of practices, i.e. laboratories, and do an ethnography and potentially also hands-on experiments themselves while nonetheless following their sociological and ethnomethodological training while doing so. Ethnomethodologists can even seek qualification as legitimate practitioners of the subjects they wish to study to give them full membership, and thus ‘strong unique adequacy’. In terms of military training members’ knowledge and gaining

do not usually require them to. Instead their ‘unique adequacy’ is required for the formal analytical frameworks they use, a practice that ends up ironising the original phenomena of investigation: a point which must be constantly reiterated lest it be ignored.

'uniquely adequate' grounding of certain kinds if 'military work' this tends, as in my own case, to be acquired before going on to become a sociologist and social researcher. This partially explains the dearth of their accounts, as the capacity to articulate their membership practices in its own terms often comes after participation has ended and perhaps the opportunity for 'coherent' data collection has passed.

3.0 UNIQUE ADEQUACY AND STUDIES OF MILITARY PHENOMENA

As mentioned at the start of this paper, Garfinkel, the founder of ethnomethodology and the person responsible for formulating the unique adequacy requirement, gained military service experience in the Second World War like most of his generation. As Rawls notes: "He had to train troops to throw explosives into the tracks of imaginary tanks; to keep imaginary tanks from seeing them by firing at imaginary tank ports. This task posed in a new and overt concrete ways the problems of the adequate description of action and accountability that Garfinkel had taken up at North Carolina as theoretical issues." (Rawls 2003 p15) Garfinkel did not take up military themes in his ensuing published research – although his archive shows that he did undertake studies while serving in the military, e.g. The Gulf Port study, and other related studies shortly afterwards. These studies have not been published and were not, as far as I can ascertain, circulated by Garfinkel within the EM community.

Few other ethnomethodologists have undertaken and published studies of military phenomena either,⁵ even in a broadest 'militarism' interpretation of 'military' as discussed above, and those that have done so have tended not to address their UA directly in their methodology. Nevertheless, we can read-off from their studies themselves how they have addressed this issue.

4.0 EMCA STUDIES OF 'MILITARY' PRACTICES

It is one thing to require your students to become 'expert practitioners' of law, science and mathematics, as Garfinkel famously did, and another to say: "go

⁵ John Hockey's ethnographic, but not ethnomethodological, i.e. not looking at the local production of order, *Squaddies: Portrait of a Subculture* (1986), the result of his PhD thesis (supervised by John Hughes) on an operational army unit similar to the one he had served in has long been perhaps the closest to 'unique adequacy' fulfilment that I have been aware of, although Kirke (2009) too comes to mind. A more recent example is Greenwood (2016). Paul Higate's (2015), again non-ethnomethodological, study of private military contractors (PMC) in Afghanistan utilising his previous military status to gain access to undertake a Close Protection course. This was run by, and for, ex-military and paramilitary personnel to become private military contractors and did provide him, a former non-combat military clerk, with a large degree of 'uniquely adequate' experience for the analysis pursued, although he did not become a PMC as part of the study.

become a soldier and fight in a war". Nevertheless, ethnomethodologists have tackled military topics occasionally⁶ and it is informative to look at them in light of UA, and especially where that UA arises not from direct military experience but other practical knowledge related to the military phenomena under investigation.

Harvey Sacks, a co-author of Garfinkel's and founder of ethnomethodological conversation analysis, studied a newspaper item reporting the views of a Navy pilot (Sacks 1992 Vol 1 Lecture 14 p205). Sacks was not in the military, and invokes no such 'members' knowledge' in his analysis. Rather Sacks' analysis is of the membership categorization devices deployed by ordinary newspaper readers of which he is one, when reading the article. No military 'unique adequacy' is invoked other than that which people can obtain from popular media. The UA invoked in the analysis remains within the limits of non-military training and no combat experience.

Alec McHoul (2007) uses Sacks's membership categorization analysis to also look at newspaper accounts of the death by 'friendly fire' of British soldiers by American pilots. Here again the military is a subject matter but the analysis of the reported 'news' and available transcripts and 'generic', rather than specialised UA of military experience 'deployed'. Gibson (2012) has analysed deliberation and decision-making during the Cuban Missile Crisis in the White House using transcripts of recordings made at the time, displaying again an awareness of their own UA built into their methodology, but one which did not require military experience or 'unique adequacy' regarding military strategy and ballistics.

The use of transcripts and various recordings is also key to Lynch and Bogen's (1996) study of the Iran-Contra hearings and the testimony at tribunal of Colonel Oliver North and others. This is a military-judicial phenomena and UA as deployed in the analysis is not of the military *per se*, but of 'evidenced based decision-making' and it stays within those parameters. This does not mean that we do not learn something of military practices and the nature of militarism, but that the analysis is limited by the extent of the researchers' knowledge and the nature of the phenomena that guides the methods of data collection, analysis and explication of findings. The same can be said of McKenzie's (2001) study of a diplomats' media interview account of the first Gulf War, which focuses on the accounting practices of those involved in the larger phenomena of military engagement by the West (See also Boudeau's (2012) study of the Hutton Inquiry into arms sales to Iraq). Thus, military phenomena are investigated without UA specifically derived from military service, but the subject matter and its analysis are chosen in line with the UA capacities of the researcher and thus do not ironicise the phenomena.

⁶ Worth noting is a two-part special issue of *Ethnographic Studies* Volumes 9 (2007) and 10 (2008) entitled *Media, Wars and Identities: Broadcasting Islam, Muslims and the Middle East*, Part 1 and 2 which take an ethnomethodological position.

So far we have broadened out what might constitute military phenomena and how one might adequately fulfil the 'unique adequacy requirement' and in what terms. However, we still have the problem of accessing and analysing military combat, even if we recognise it is only one of many embodied forms of military action and militarisation. For reasons of access mentioned above, combat is not something we have many 'uniquely adequate' accounts of and few soldiers turned sociologists who have been in combat have yet had much to say about it, and this applies to me too. Whatever the reasons for this dearth, recent developments in audio-visual data that captures combat, such as headcams and aircraft flight recorders, provide new forms of data for research. But such audio-visual data in itself does not solve the problem of UA for its analysis, and so research design must reflect this absence and resolve it.

Two ethnomethodological studies are illustrative here in how to attend to this, in both personal fast jet fighter pilot UA would have been the 'strong' option but while not directly available, was nonetheless overcome to some degree. Both studies used the audio-visual data from the pilots of a US friendly-fire attack on British soldiers in the first Gulf War. The first investigations of the in-flight data were by Neville (2009). Neville (2004) had previously studied talk between commercial airline pilots, and I suggest this provided him with a certain degree of members' knowledge, which was supplemented by discussion with military pilots, to give a degree of, if limited, UA. Here we see the research design successfully reflecting the UA of the researcher in the research design and analysis.

However, a second innovative solution to the problem of a lack of sufficient military 'uniquely adequate' grounds for the ethnomethodological interpretation of this data comes from a series of papers where the researchers did not rely upon their own UA (they were neither pilots or military personnel). Instead, aware of their lack of UA their research methods focused on rich, if incomplete, data from the Courts Martial which two pilots had undergone and where the pilots had had to interpret moment by moment the audio-visual data that had been captured on their headcams and flight recorders. (Mair et al 2012, Elsey et al 2016) Thus by using the UA of the pilots themselves the analysts were able, by combining various data, to undertake or use 'UA by proxy', and thereby use and analyse data beyond what the UA requirement would otherwise have allowed them to say, indeed understand, based upon the data without this innovative analysis.

The point I am making is that through understanding their UA requirements, and the limits it imposes, the researcher can design their research methodology to match their competencies. Significant though, is that the parameters of *that* are not just, 'do they have military experience or not?', or even what personal or generic UA be deployed. But additionally, what resources can the methodology access that methodologically compensate for this absence of specialist UA. To build such material into our research we have to have developed robust methodologies, and as I have argued, the basis for doing this is through an honest assessment of

our ‘uniquely adequate’ capacities available to undertake that research, and that these require a deliberative, rather than passive, understanding of what those are. To a large extent this requires us to examine our own ‘militarism’ and what ‘uniquely adequate’ capacities this gives us to undertake research of particular phenomenon and how we do it, rather than dismiss such phenomena *a priori* (or do formal analysis regardless of considerations of our UA, and our lack of it).

What is being suggested is that we are ignoring the phenomena of ‘the military’, ‘militarism’ and ‘war’ (and their respecification). This I believe is partially due to the sociological binary between military and civil society, a cultural tendency to focus on the purely ‘War’ aspect of military activities, with ‘War’ being seen as essentially ‘Combat’, and a mythologizing of the skills and competences of military personnel and their modes of being in the world linked to kinetic warfare. Instead, it is perhaps more useful, as well as realistic, to understand that except on very limited occasions the military are not radically different from many other professions we may investigate, and what actually constitutes military phenomena (which ‘militarism’ believes is embedded in all of our society in various and numerous ways) are present in many forms and many areas of society in the same way that ‘science’ is, i.e. not confined to the laboratory, but pervasive throughout society. Once accepted in this light ‘the military’ and ‘militarism’ – as ‘black boxes’ rather than explanatory concepts – become phenomena that we have more UA available than when restricted to ‘participation in armed combat’: which is an ironicisation of war’s reality in the first place. The implication therefore is, that we deliberately review what it is to be a ‘competent member’ with UA regards ‘the military’, ‘militarism’ and even ‘war’ phenomena, and thus how, in light of such a review, we might investigate them.

5.0 APPLYING UNIQUE ADEQUACY BEYOND ETHNOMETHODOLOGY

While the study of various ‘military’ phenomena relating to war, and especially militarism, does not necessarily require – although it may certainly be useful – military training and participation, that experience does provide a useful way to explore these overt forms of embodied militarism. Having ‘done’, participated in, etc. a phenomenon of study helps reveal the nature of that phenomenon through its embodied activities and members’ knowledge of its practices. I am suggesting we would do well to assess our UA with respect to ‘militarism’ (and anti-militarism), indeed any research phenomena, and whatever our approach.

The origins of this paper come from reflection on my work as a sociologist within the area of the sociology of the military and society: which is not quite the same as ‘military sociology’ which tends to be embedded within military organisations with an aim towards their ‘improvement’, i.e. efficiency. Recent years has seen the emergence of Critical Military Studies (CMS) which, as the name suggests, takes a less ‘embedded’ and more critical approach to the military, but also a much

broader view of 'the military' via an orientation towards militarism in society. CMS is an "approach to military, defence, conflict, and security issues which foregrounds an understanding of military processes and practices as the outcome of social life and political contestation in multiple ways and at a range of scales from the embodied to the global, rather than as given, functional categories beyond interrogation." (Rech et al 2015 p48) These processes and practices are understood as forms of militarism where ideologies "prioritize military capabilities in the resolution of conflicts" (Ibid p48). Militarism is seen to pervade our contemporary societies in various forms to which all members are exposed, and it affects everyone including the social scientists who study it.

The danger with such an approach is its externalism. The phenomenal details of practices can be lost through a concern with militarism theory *a priori*, and hence foundationally, rather than as emergent from our embodied practices in various 'life worlds' (Schutz 1971). However, CMS encompasses many phenomena and methodologies and this includes, for some researchers at least, the militarism we embody through our practices and structures of feeling, as well as through our normalised talk and actions as the way in which militarisation is enacted by individuals and groups (see McSorely 2013). Thus, and usefully in terms of UA, everyone's actions can potentially occasion what may be understood by CMS as embodying 'militarism' and even militarisation, regardless of our explicit marking of it, or concern about it, as members of society (that awareness and concern may indeed be phenomena of investigation themselves).

Militarism used in this fashion would of course be ethnomethodologically problematic for EMCA as a driving force for its investigations and instead would be something that could be subject to an ethnomethodological respecification. However, when one looks to EMCA's corpus of studies for examples of EMCA studies of 'the military' or phenomena that CMS might consider related to 'militarism', it becomes evident that few have been undertaken, and why this is so becomes an interesting question in itself. One reason, I suggest, lies in exactly the problem EMCA might see in CMS studies of 'the military', i.e. the danger of ironising phenomena, not in this case via 'militarism' as an organising theory, but through a lack of members' knowledge of 'the military', i.e. a lack of UA, and its application in the consideration research design methodology

One of the problems with Critical Military Studies remains that researchers and scholars will often interpret phenomena with little or no 'members' knowledge' of said phenomenon, e.g. what it is like to be a soldier. Interestingly, on a number of occasions former military personnel have directly confronted CMS researchers on exactly this point, claiming that they as serving or former military personnel alone have 'unique adequacy' (although not using that term), and hence 'right', to speak authoritatively on military phenomena, especially in relation to the experience of 'War', and 'combat' in particular. Whilst indicative of a real issue, these criticisms were not always accurate as 'militarism' as phenomena of CMS

analysis, as noted above, is usually broader in scope than soldiers experience of combat and its consequences'. CMS research includes various phenomena of 'militarism', not just its manifestation in war and the institutional preparation and execution of it. Nevertheless, the criticism has some justification, and not just in relation to the ironicisation of the practices and experiences of military personnel but perhaps other members of society too.

This was not a suggestion that CMS do only ethnomethodological studies, but rather use UA considerations as a practical 'tool' and/or practice so they can align their competences - as members - with the phenomena they were going to investigate. Thereby choosing suitable methodologies and methods to do their research, and thus minimise the criticisms of their methods and findings from 'members' of the phenomena they investigate. This suggestion is not as outlandish as it may seem, indeed Rooke and Kagioglou (2007) have suggested that UA can be usefully, if differently from here, be used as an 'evaluation tool' for disciplines outside of ethnomethodology. However, so far the UA concept has met with rejection in CMS, and largely because it is seen as exclusionary, i.e. would prevent people doing research on subjects of interest to them where they have no UA. My initial ethnomethodological reaction to this criticism would have been reciprocal blank rejection of their criticism, if it wasn't for the outstanding issue of why are there so few ethnomethodological studies of combat, war, the military and phenomena related to 'militarism' by ethnomethodology and conversation analysis? Could the problem indeed be that 'the unique adequacy requirement of methods'? or at least how it is interpreted regards 'military phenomena' is in practice exclusionary. If so, hopefully this is an issue which this paper has in some way addressed.

6.0 CONCLUSION

Ethnomethodology's emphasis on UA must be seen within the context of a belief that we are all 'practical sociologists', if not necessarily academic ones, and thus we can all claim UA in the constitutive practices of our familiar life-worlds and the settings around which they are organised. The difference is that sociologists leave their familiar life-worlds and pronounce on the life-worlds of others, ironicising those worlds as they are translated via formal sociological theories into the subject matter of the sociological life-world. The 'unique adequacy requirement of methods' is Garfinkel's way of bringing to our attention this methodological, indeed epistemological, problem and it is a policy designed to minimize its impact. The social scientist themselves is not just an institutional dope trained, if not drilled, in research methods which they apply blindly. We too have a life beyond our academic and institutional systems of professional behavioural requirements, which are not separable from, but neither determined by, our training and institutionalization. The research sites of our empirical data collection can thus be seen as the coming together of our embodied institutional and personal

'biographies' in the form of competencies at sites of our data collection whatever the method. These 'biographies' are not just abstract concepts, they are ways of 'being-in-the-world' (Schutz 1971, Heidegger 1962) and practices of 'telling in the world' (personal correspondence with Michael Mair), that are embodied in our practices and interactions, some of which we may want to keep 'back stage' and others that we may want to present 'front stage' (Goffman 1973).

Additionally, the significance, or otherwise, of the researcher's biography is not static and is conditional on the research project and interaction with others in the emergent practices of research and beyond. What this means is that the choice of research phenomena by the researcher can be guided in relation to their own UA, and that therefore its adequate assessment is paramount in the methodological considerations of how they undertake fieldwork and their choice of research methods.

I have been keen to stress that UA regarding military phenomena be recognized as belonging not just to current and former soldiers since we all embody broader aspects of militarism. What might be 'uniquely adequate' with respect to a given research project depends on what phenomena we choose to study and how. Cognisance of this positively normalises rather than pathologises 'the military' and related phenomena, and allows one engagement with encountered individuals and organisations as 'ordinary people', rather than within the ironising binary of civilian/military.

Our understanding of 'military phenomena' has not been facilitated by a tendency of the social sciences, especially sociology, to marginalise one of our major forms of social organization, 'the military'. Sociology is now starting, if still in foundational and theorised forms of militarism and militarisation, to address this lacuna. Nevertheless, it needs to be cautious about how it undertakes this project with its inherited methodologies and research practices. By reflecting on ethnomethodology's concept of the 'unique adequacy requirement of methods' I hope to have both opened to more rigorous appraisal how we select our phenomena, and also how we undertake our research. What is at stake is the robustness of that research, the veracity of its descriptions, and its utility. With potentially serious negative consequences if flawed research findings are then adopted in policy and practice. This does, as a knowledgeable reviewer of this paper has indicated, raise the question of to whom the results of such research would be useful to – military practitioners? citizens of a militarized society? However, this paper is not about determining users of research, but about getting some research undertaken in the first place, by ethnomethodologists, in an area which I have suggested is unnecessarily under-researched by them. Secondly, it is about getting those non-ethnomethodologists who research the area already to build into their research design a consideration of their UA – suggesting that by doing so they may lessen, by judicious choice of phenomenon and methodology, their potential ironisation of the phenomena they aim to represent and others' cause to dismiss it. Whether

“having something useful to say” to members of researched phenomena is the criteria of ‘good’ research methodology I am not so sure. But if we are all ‘militarized’ in some fashion then a ‘better’ understanding how that ‘militarism’ operates in society, and even how the conduct of ‘war’ itself is prosecuted, is surely a benefit to us all.

However, my aims here are less ambitious and I have merely suggested that instead of UA limiting the phenomena we investigate, its consideration can broaden them. As ‘the military’, ‘war’ and ‘militarism’ entails unique adequacies that are arguably embodied in all of us, if in different ways. My hope is that in doing this ethnomethodology (EMCA) can engage in the ‘military turn’ in sociology, and in doing so promote an enabling form of ‘the unique adequacy requirement of methods’, both within and beyond ethnomethodology, and even beyond this substantive topic. Sociology has adopted much of the ethnomethodological critique of formal sociology, UA, I argue, has much to offer too.

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