Wild in the Laboratory: A Discussion of Plans and Situated Actions

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ABSTRACT

Suchman's book Plans and Situated Actions has been influential in HCI (Human Computer Interaction). The book is often discussed with reference to ethnographic fieldwork, sometimes being cited as if it were a field study. However, the book uses examples from a laboratory study and contains criticisms of ethnography. This paper explores how and why Suchman carried out a laboratory study. Based upon this exploration, it argues that social analysis in HCI does not necessitate fieldwork outside the laboratory. More broadly, the paper argues that an appreciation of Plans and Situated Actions can help in moving towards forms of social analysis that span both the laboratory and the world outside. If there is to be a "turn to the wild" in HCI, this should not be a turn away from the laboratory but a turn away from research methods that ignore human practice. This is not to defend laboratory experiments, but lab-based studies that explicate technology in practice.

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1. INTRODUCTION

"Wild sociology" is a term that has been used by Crabtree (2001), Lynch (1993) and O'Neil (1980) to describe forms of social analysis that operate for purposes other than sociology itself. Wild Sociology is, so to say, sociological enquiry in the wild. Wild

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sociology is not a synonym for applied sociology, but sees methods and perspectives from sociology employed for the purposes of another discipline. These methods and perspectives often become enmeshed, integrated and eventually hybridized with the other discipline. One such discipline is HCI (Human Computer Interaction), with ethnomethodology, conversation analysis, distributed cognition, and other forms of enquiry, elucidating, impacting and transforming the ways in which technologies are designed and evaluated (see Dourish 2001, 2011; Randall et al 2007; Rogers 2012). Wild Sociology is not a term in common use, and arguably mischaracterises ethnomethodology and so on as techniques in need of a home, rather than disciplines in their own rights. But wild sociology is a helpful term for the purposes of this paper because it employs the metaphor "the wild" to describe methods that have moved out from Sociology. In HCI this metaphor is increasingly being used to describe research outside the laboratory: terms such as "the turn to the wild", "design in the wild" and "in the wild studies" (e.g. Buxton 2007; Crabtree et al 2012; Rogers 2011) are increasingly used in the context of social analysis and ethnographic approaches to design. The turn to the wild sees the formative design and evaluation of technology moving from the laboratory and into the field. This paper discusses a difference between wild sociology and the turn to the wild: the former is predicated as a move away from mainstream or professional sociology, whereas the latter is predicated as a turn away from laboratory studies. This difference is of interest because, unlike the turn to the wild, wild sociology does not preclude the possibility of laboratory studies; wild sociology is not necessarily the study of "the wild". The paper will argue that the laboratory needs to be rediscovered by social approaches in HCI. This rediscovery will be helpful not just for an uptake of laboratory studies, but in articulating why design and evaluation might stretch beyond the laboratory and into different settings. This paper is not arguing for a return to laboratory science, but for social analysis to span laboratory and field.

This paper revisits Suchman's influential book Plans and Situated Actions (Suchman 1987; 2007), discussing it as an example of wild sociology. In Plans and Situated Actions, methods from sociology are drawn upon but applied within a laboratory environment. The paper will outline Suchman's study and then trace its influence in HCI, concentrating on its subsequent interpretation as an argument for ethnographic fieldwork. A summary of Plans and Situated Actions is given in the following section. This summary pays particular attention to the laboratory aspects of the book and gives reasons why Suchman's methods were applicable within a contrived, laboratory setting. This section will also discuss the influence of Suchman's study on the uptake of

ethnographic methods in HCI. Section three will discuss recent work in HCI that advocates a turn to the wild. The turn to the wild has been influenced by the analytical approaches outlined by Suchman, but also approaches developed by others, particularly Hutchins' (1995) work on Distributed Cognition. In the light of Suchman's book, the paper will argue that Hutchins is arguing against scientific methods commonly associated with laboratory studies of people, and not against the use of laboratories in general. Section four will argue that social analysis (by which I mean social analysis for the purposes of HCI, CSCW and concordant disciplines) needs to loosen its commitment to ethnographic fieldwork and rediscover the laboratory.

2. Plans and Situated Actions as a Laboratory Study

Suchman's book Plans and Situated Actions (1987), published twenty-five years ago, has been highly influential in HCI (Anderson 1997; Crabtree 2003; Crabtree et al 2012; Dourish 2001, 2011; Syzmanski & Whalen 2011; Randall et al 2007; Rogers 2012). The book occupies a central position in most accounts of what Crabtree (2003) and others term the "*turn to the social*", a period in the late 1980s and early to mid 1990s during which methods and perspectives from sociology and anthropology were widely adopted in HCI. Many accord the book great status, for example Rogers (2012) claims Plans and Situated Actions "*took the field by storm and was universally read by all*" with the book having "*a profound effect on how programming, users, and interface design were constructed and researched*" (p.45). Rogers' claims seem exaggerated, HCI is a fragmented field and even if the book was universally read it is clear that many disagreed with it (e.g. Vera & Simon 1993). The book was also by no means the first study to connect social analysis and system design, sociotechnical approaches were already established in information systems research and ethnography had made inroads into HCI and CSCW (see Anderson 1997). But as an output from Xerox PARC (a centre for many key innovations in HCI) the book must have caught the attention of many. With its forceful and compelling arguments, the book also seems to have captured the imagination of many.

In the book, Suchman argues that human-machine interaction is deeply asymmetrical, with humans doing a great deal of work with and around technologies to which the technology has little access, and little opportunity or ability to make sense of. She argues this means that the problem of making usable technologies is not just one of designing self-explanatory technologies, but one of giving support to people's sense-

making practices. Suchman developed her argument with relation to problems encountered by Xerox of making photocopiers that customers would find easy to use. To illustrate her point, Suchman conducted a laboratory study. She used detailed transcripts from these to demonstrate the ways in which people go about making sense of a complex technology. Her contribution was not to offer a specific means to make technologies easier to use, but to reframe the entire issue of interaction.

For her study, Suchman invited four pairs of "*first time users*" (p.114) to be videoed conducting tasks with the photocopier. The sessions each lasted for one and a half to two hours. First time users were invited because "*the troubles encountered by first-time users of a system are valuable in that they disclose work required to understand the system's behavior that, for various reasons, is masked by the proficient user*" (p.114-5). These users included noted cognitive scientists (see Suchman 2011), perhaps as a way of ensuring the participants were intelligent, technically literate people, and likely as a means of driving home the book's attack on cognitive science. The pairs were given a set of tasks to perform, and asked to work together, talking aloud as they went. Pairs were used because "*each makes available to the other what she believes to be going on… [and] she provides that sense to the researcher as well*." (p.115). In a footnote Suchman does mention she carried out about 20 hours of observation of photocopier use in the field, so there was a fieldwork element to her work. She explains these observations confirmed the complaints Xerox had received from users that their machines were too complex too use. She then states fieldwork would not help in understanding those complexities: "*the methodological problem at that point was that I, as an observer, was equally confused … To understand the problem would require an appropriate i.e. videotaped record*" (p110). The videotaped record Suchman required was more than a naturalistic capturing of everyday photocopier use. Through "*sort of uncontrolled experimentation*" (p114), Suchman sought to render photocopier use into an observable activity.

2.1 Ethnomethodology in the Laboratory

The headline argument in Plans and Situated Actions is that a plan does not drive or produce the action of following the plan, but rather it is followed, or reformulated, or worked around, or abandoned in "situated" ways. To follow any plan (be it a route, a laboratory task, a project plan, or the like) requires work beyond that which can be specified in the plan. For example, directions to walk from one building to another will

not (and can never) go to the detail of just how you might walk, or open a door or cross a road, or what to do if you bump into someone on the way, but will give the details deemed useful. Suchman discusses activities such as navigation and skiing, but her attention centres upon Photocopying. Photocopying is perhaps not a typical activity in which we would consider using a plan, and this is really part of the point. The book was not written to advance our understanding of plans but to criticise "*the planning model*" she saw as driving interaction design. According to Suchman, the planning model describes people's behaviour in terms of goals and the cognitive plans formed to achieve these. Suchman argues that this model is wrong, but more importantly that it distracts us from the ways in which people act and interact. The planning model enables an abstract, disembodied, asocial approach to modelling action, where behaviour is only interesting insofar as it betrays underlying processes. It should be noted that the planning model is a straw man term used by Suchman to refer broadly to a collection of approaches in cognitive science. These methods were not necessarily laboratory methods. While Suchman's arguments are applicable to laboratory evaluation methods such as GOMS (Card et al 1983) that were being developed at Xerox PARC and elsewhere at the time, Suchman does not specifically consider these in her book.

Suchman points out that human action and interaction is inevitably and irrevocably "*situated*". Situated action is not a type of action (as the book is often understood as saying, see Randall et al 2001). Rather, through this term, Suchman invokes ethnomethodology and conversation analysis. The term "*situated action*" is really nothing more than a means of introducing an alternative analytical approach; it is a ticket into the ethnomethodological theatre, and can be torn up upon entry. It is no longer relevant to identify anything as situated, once we realise everything is. The term was not coined by Suchman, but can be found throughout Garfinkel's (1967) writings on ethnomethodology. It is by no means a key term for Garfinkel, but was presumably picked up by Suchman because it neatly disassociates the word "action" from the word "plan". Suchman explains that ethnomethodology and conversation analysis reject the view that action is driven by its context, and instead see context as (simultaneously) a driver and an achievement of the action. The relevant next thing to do in any activity is contextually relevant, but what counts as the relevant context is determinable during the course of action. For example in confronting a new technology it may be seen as relevant that you have used something like it before. In trying to use the new technology, you may draw upon your experience, and that experience may, through the course of the activity prove useful, or it may not. Suchman explains:

"[The] conditions of our actions are not simply pre-given and self evident but are themselves constituted through unfolding courses of action and interaction" (p.51)

Suchman is not claiming that people just make things up as they go along, that there is no rationality in people's action. Her argument:

"…is not to say that action is constructed somehow always de novo or in a vacuum. On the contrary, human activity invariably occurs in circumstances that include more and less long-standing, obdurate, and compelling layers of culturally and historically constituted, social and material conditions." (p.51)

The setting in which an activity takes place is likely to be relevant to how an activity such as photocopying unfolds. Blomberg (1988) has, for example, pointed out that the location of a photocopier influences and even presumes who will offer help when there are problems, and that the rhythms of work determine when people have time to play around with features and when they just need to get things done. However, it is not setting that Suchman is interested in or getting at. In fact, the setting (other than the fact a photocopier is available) is not important at all in her analysis. This is a key issue, because Suchman was not asking questions about the laboratory setting in which her study took place, but exploring the methods by which people make sense of a technology (specifically, Suchman explores the "*accountability*" of technology – the means by which it is describable, intelligible, reportable and analysable during the course of use). The laboratory setting clearly and undoubtedly has an effect on the way the study participants act. In particular, the allotted task and the request for them to talk aloud not only influenced, but directly led to what the participants did. However, these artificialities cannot have made everything the participants did artificial. The task she set could not have determined the actions they took in order to fulfil it; according to her own argument, plans do not determine action. The requirement to talk aloud could not have determined what was said and how; as Heritage (2005) points out about talk in institutional settings: *"a person is 95% conversationalist before entering an institutional setting: persons interact using largely the same set of interactional resources in institutional talk as the do in everyday conversation" (p.107).* Ordinary methods of action and interaction are not left outside the laboratory. There may be esoteric things about the laboratory environment, but Suchman is getting at the set of interactional resources that people call on more or less anywhere. Her focus was not on the setting, but the

methods by which any person would reasonably go about a trying to use a complex technology. These ethno-methods are robust enough to be used in the workplace, the university, the museum, the laboratory, or anywhere, and alone or in the presence of others.

Not everyone is comfortable with Suchman's use of a laboratory study. Dourish (2001) puts Suchman's choice to use a laboratory down to the fact her work gives a critique of cognitive methods. Suchman did invite a noted cognitive scientist to participate in her study, but it seems unlikely that the laboratory was chosen purely because it was somehow home turf for cognitive science. The planning model Suchman attacks is not specifically a laboratory enterprise, and if she did wish to attack in this way then it may have been easier to video and analyse an actual laboratory study (for example a contemporary GOMS study). Elsewhere Dourish claims "*laboratory studies are hardly the stuff of ethnomethodology"*, suggesting that the importance of Suchman's work is not in her study but in drawing on "*the ethnomethodological tradition, and introduced it to the HCI community" (*Dourish & Button 1998 *p.402).* I do not agree. Arguably, ethnomethodology has no tradition of laboratory studies, but certainly a range of studies have been carried out, including breeching experiments (Garfinkel 1967) and replications of scientific demonstrations (Bjelic & Lynch 1993, Livingston 2008). Ethnomethodology is not prescriptive about method, an eclecticism of method can be found across the work of Garfinkel and others. Lynch (1993) has, however, criticised Sacks (e.g Sacks et al 1974) and others for moving towards overly laboratory-like methods in the analysis of conversation. It is important to note that, as Lynch (1993) explains, ethnomethodology is not anti-scientific but simply opposes the idea that there can be a social science. One of the key contentions ethnomethodology has with social science is the irony that in running a study, social scientists have to rely on ordinary methods of constituting order, and yet their accounts of social order will erase what they did in favour of theoretical explanation. This is an irony for the social sciences, but is not a critique of science itself and certainly not a critique of the laboratory.

2.2. Plans and Situated Actions as an Ethnography

Plans and Situated Actions has become strongly associated with ethnographic fieldwork. Anderson (1997) sees it as demonstrating the potential for interaction analysis based in ethnographic fieldwork: *"More than any other previous (or subsequent) study, Suchman's work seems to demonstrate the potential which social science analysis based in*

ethnographic fieldwork could have" (p.159). Similarly, Crabtree et al (2009) call for "*ethnography in the style of Suchman*". Rode (2011) goes further, ignoring ethnomethodology and conversation analysis and taking Plans and Situated Actions as a general invitation to any form of ethnographic study: "*[S]ince 1987, when Suchman published her ground breaking book, anthropology has continued to develop, but HCI has been slow to keep pace with these changes*" (p.123).

HCI textbooks usually refer to Plans and Situated Actions as arguing for a focus on real-world situations. Rogers et al (2011) claim Suchman focused on "*real social and physical contexts*", and Dix et al (2003) claim she discusses "*observations of real photocopier use*". In their textbook on Research Methods for HCI, Lazar et al (2010) claim that Plans and Situated Action is "*the most famous example of ethnography in HCI*" (p.222). Lazar et al also equate ethnography with research in the wild, claiming: *"Ethnographic studies are usually conducted "in the wild", in homes, workplaces, educational settings, or other places where the "action" of interest takes place." (224).* Anyone who is not familiar with Plans and Situated Actions is very likely to assume from reading the HCI literature that Suchman's book makes a case for using ethnographic fieldwork for studying human computer interaction.

It is strange that a key ethnographic text for HCI contains a laboratory study, and that this usually goes unmentioned. The reasons why Plans and Situated Actions has been interpreted as an argument for ethnography cannot be found in the book itself. Suchman has a background in ethnography and anthropology, but her book can be read more as a critique of these than a recommendation. Suchman quotes an iconic anthropological account by Thomas Gladwin of Micronesian navigation, not to recommend it but to critique its simplistic contrast between plan driven navigation in the west and situated navigation in Micronesia. Furthermore she criticizes "*traditional*" ethnographic accounts for providing a post-hoc and unverifiable account of action, preferring video analysis. Suchman reports that her own fieldwork left her "*confused*" (p110).

In the following two sections, this paper will discuss the evolution of ideas surrounding Suchman's work. These sections do not present a thorough history of the influence of Suchman's book, the development of Suchman's own interests following publication of the book, or of developments in ethnographic practice in HCI, but seek to explain that it

was for reasons external to Suchman's arguments that Plans and Situated Actions became associated with ethnography and disassociated from the laboratory

2.2.1 The Division of Labour in HCI

Many (e.g. Crabtree 2003, Crabtree et al 2012, Randall et al 2007, Reeves 2011, and Dourish 2001) identify The Computer Researches Out by Grudin (1990) alongside Plans and Situated Actions as a key text for the turn to the social. Grudin's paper summarizes the history of HCI and envisages its on-going development. Grudin likened HCI to a child, going through stages of development. He argued that the discipline is (or *was* in 1990) in its third stage of development and beginning to enter its fourth. The third stage was characterised by its focus on user interfaces and its alliance with cognitive psychology and ergonomics. Grudin envisaged that HCI would, during the 1990s, develop through stages four and five. Stage four, he envisaged, would focus on higher-level cognitive issues in how people interact with computers, extending the interests already present in HCI. This stage would "*rely less on controlled experiments … and more on recording the dialogues and analyzing transcripts*." (p.264). Stage five, he predicted, would see HCI move away from cognitive psychology, to explore the use of computers in social settings. It would be an "*outward step … into the social or work setting*." (p.264). Grudin pointed to ethnographic studies, in-situ contextual design, and participant observation as key approaches for stage five.

Grudin's paper tried to make sense of an already fractured discipline (mentioning, for example, that there were controversies among authors and reviewers for the major HCI conference about what was relevant). The paper was not an attempt to reunify HCI, but suggested that seemingly disparate research efforts can be better understood as a discipline if there is a reconceptualization of the user interface as the "*computer interface*". Similarly to Suchman, Grudin moved HCI from a symmetrical focus on user and computer to one in which the computer sits within the social world. However, Grudin was not trying to open up HCI to social analysis by attacking cognitive science; he emphasised "*we need work at all levels*". Grudin's strategy in rationalising HCI was to create a division of labour. He did this by designating the work setting as the focus for social analysis, whereas the laboratory is where cognitive scientists focus on individuals. Grudin mentioned Suchman in The Computer Reaches Out, but categorised her work as stage five HCI, alongside a number of different approaches including Contextual Design. This may have diffused an explosive situation, but this way Suchman's work was not

treated as a radical reconceptualization of context, but one of a number of ways of addressing settings that lie outside the laboratory.

A decade later, Dourish (2001) and Crabtree (2003) pointed to their own work as embodying the outward step Grudin envisaged. Two decades later Reeves (2011) has claimed that his work on public and group interfaces embodies a further step for HCI. However, Grudin's comments about fourth stage HCI have been largely ignored, and the fifth stage has become treated as a revolution rather than part of an HCI spectrum. In Crabtree's (2003) and Reeves (2011) accounts, HCI appears to arrive at stage five by turning away from stage three; there is no continuity or connection between stages, and stage four is simply missing. Crabtree (2003) describes the turn to the social not as an expansion but a turn away from "*cognitive bias and a misguided alliance to the methods of natural science*" (p.33). Grudin's attempt to repair the continuity of HCI as a discipline failed. The battle lines drawn by Suchman between social and cognitive perspectives were not pacified but redrawn between studies conducted in ordinary settings and those conducted in laboratory settings.

2.2.2 Ethnomethodologically Informed Ethnography

As outlined earlier, Suchman drew on ethnomethodology and conversation analysis. Ethnomethodology (and to some extent, conversation analysis) was later drawn upon by a group of researchers in Lancaster and elsewhere, in a body of work that is sometimes referred to as the Lancaster studies (e.g. Dourish 2006, Randall et al 2001). This work combined ethnographic fieldwork and ethnomethodology, and gave rise to a programme of ethnomethodologically informed ethnography (Crabtree et al 2000). While Suchman's work at Xerox was concerned with usability problems within Xerox, the Lancaster studies were concerned with the software crisis (the problem identified in software engineering that most development projects fail). The Lancaster studies were premised on the idea that if "*social and organisational factors*" (Sommerville et al 1992) could be understood then development projects would be less likely to fail. For example, in a discussion of the value of working with the Lancaster sociologists, Sommerville et al (1992) describe a desire to gain: "*a clear understanding of the workplace and the way in which humans interact with each other in that workplace." (p.343).* This view of the workplace was desirable because the Lancaster studies were not addressing product design, but the development of complex, specialist systems (for example air traffic control systems). The studies did not themselves report back to

specific development projects, but were designed to comment on why many projects fail and provide a method to mitigate this. Ethnographic fieldwork coupled with ethnomethodological analysis became this method, one in which a developer located in one organisation is informed about a place where a system will be implemented. Suchman's studies on the other hand were conducted inside a corporate research and development environment, evaluating technologies under development there. Whereas the Lancaster studies sought to provide a view for developers of complex, professional practices, Suchman's work was addressing ordinary action.

Hughes (2001) and Crabtree et al (2012) have pointed out that although the Lancaster studies began after the publication of Plans and Situated Actions, these studies were not inspired by it. They recount that the air traffic control studies started out as a social study of rule following, and only came to focus on systems design after a chance meeting. Looking further back to the time that Plans and Situated Actions was published, Hughes (the Lancaster Sociologist whose students and assistants would go on to develop ethnomethodologically informed ethnography as a technique for HCI) was drawing upon ethnography and ethnomethodology to focus on entrepreneurship. In the book Working for Profit, Anderson, Hughes and Sharrock (1989) draw upon transcripts, notes, examples of calculation and the like to examine the work of an entrepreneur. As with Suchman's book, Working for Profit begins with a preliminary analysis in which peoples actions are agued not to be a product of context, but (re)producers of context. It contrasts typification of the economic actor with the actor's point of view and a broader ecology of activities. Unlike Suchman's book however, Anderson et al describe an extended period they spent with one of these actors, gathering examples, sketching working environments and collecting working documents. Many of the themes and methods that would constitute the Lancaster studies echo this book far more than they do Plans and Situated Actions.

The Lancaster studies therefore had a much more organisational focus than Suchman's work; they were workplace studies as opposed to interaction analysis. The Lancaster studies do not have their roots in Plans and Situated Actions, but within enthnomethodological studies of work. Whereas Suchman was concerned with a specific, relatively short-term activity with a product designed to be used by any adult, the Lancaster studies were concerned with specialist practices that are tied heavily with the place in which they occur. The differences are interesting but the most important thing to note is that ethnography was not adopted in the Lancaster studies as a

consequence of Suchman's work. Rather, the Lancaster Studies became the lens through which Plans and Situated Actions has come to be understood. To return to Grudin's (1990) paper and his designation of the workplace as the territory of social analysis, while Plans and Situated Actions did not fit with this framework, the Lancaster studies did. Therefore, Grudin's division of labour for HCI could serve people taking social approaches to analysis by legitimising their place in the discipline. That ethnomethodology was applicable in the laboratory was only an academic argument and of little consequence when compared to the opportunity for sociologists to find a professional role through providing and practicing ethnographic methods. This professional role never quite matched what the Lancaster Sociologists were really doing, but it gave them a niche, a topic for textbooks, courses and so on.

3. The Turn to the Wild as a Turn Away from the Laboratory

The metaphor "the wild" is seeing increasing use in HCI, for example Buxton (2007) talks of "*design in the wild*", Crabtree et al (2012) "*work in the wild*", and Rogers (2011) "*in the wild studies*". While wild sociology is in the wild in the sense that it has left its home discipline, design in the wild does not involve the dispatching of researchers and methods to other disciplines but sees design research moving out of its home in the laboratory and into the wider world. Design in the wild seeks to evaluate technology in use outside the laboratory during the course of its development. A trouble in this approach is that the traditional evaluation methods used within HCI are in many ways unworkable in the wild and unable to ascertain the kinds of finding many researchers would like (indeed a motivation for design in the wild is that design and evaluation often misses important phenomena). In order to develop methods appropriate for designing in the wild, HCI has adopted and adapted forms of sociological enquiry. Much of this involves building upon theory and perspectives introduced to HCI during the period Crabtree (2003) and others term "*the turn to the social*". This paper argues, however, that the lens on ethnomethodology (and social analysis in general) provided by Grudin (1990) and the Lancaster Studies is now problematic; in particular, this paper argues that the wild for wild sociology is not equivalent to the wild for design in the wild.

Crabtree et al characterise the study of work in the wild as: *"studying through fieldwork the day-to-day business of a setting and the ordinary activities that articulate it" (p185).* They explain "*Studying work in the wild requires of the fieldworker that they unpack a setting's work by orienting to and focusing on practical action and practical reasoning to*

identify empirical topics" (p187). The perspective of Suchman and ethnomethodology is clear here, but with an additional commitment to fieldwork and the study of work in settings. Crabtree et al (2012) are not using "*wild*" and "*setting*" as synonyms, but use the term "*work in the wild*" as a means of contrasting work-as-practiced with work as it is specified or theorised. They also do not claim that setting will be an important feature of the practical action and reasoning observed, but the implication seems to be that these should be studies in the places where they naturally occur rather than in the laboratory. Although the challenge is no longer to report back from workplaces, for Crabtree et al the pairing of interaction analysis and fieldwork remains.

Whereas Crabtree et al (2012) view the wild as the opposite to specifications or theories of work, most others see the wild as the opposite to the laboratory. For example Reeves (2011) states:

``There are various challenges posed by collecting and analysing data that is drawn from interaction 'in the wild'. When compared to controlled, lab-based environments, there are a host of initial access and consent issues, as well as practicalities such as setting up video cameras, following participants in distributed settings, making sense of conduct, and so on.'' (p35-36)

Firstly, these challenges (except perhaps that of following people in distributed settings) are likely to be true of any study, laboratory or otherwise. But more seriously we see the wild being disassociated from the laboratory on the grounds that laboratories are "*controlled … environments*". This distinction between the wild and the laboratory is more pronounced in the work of Rogers et al (2007; 2011), and Buxton (2007). Rogers et al (2007), for example, see in the wild studies as providing a "*contextual backdrop*" (p.346) for thinking through strengths and problems of a technology in development. They argue:

"In-situ studies (also known as 'in the wild' studies) are beginning to appear that evaluate the situated design experience of Ubicomp, resulting in understandings of how novel pervasive technologies are appropriated in real world settings. These are quite different from the results of lab-based studies" (p.336)

Here, Rogers et al are bringing together "*the wild*", "*real world settings*" and "*situated*" as if these are, if not synonymous, a collection of things that necessitate each other. From

this collection is excluded the laboratory. The term "*lab-based studies*" is presumably meant to refer to scientific and quantitative methods of evaluation, but becomes a sweeping dismissal of any laboratory work. Buxton (2007) similarly sees the wild as anywhere but the laboratory. Reeves, Rogers et al and Buxton seem to be bringing together two things: (1) technologies need to be evaluated in situ, and (2) a perspective that human activity is situated and embodied. In the wild studies and wild sociology are treated as a pair. I believe these are brought together because the laboratory environments that in the wild studies seek to move beyond are seen as equivalent to those rejected by Hutchins in Cognition in the Wild (1995). For many, Cognition in the wild is an equally important text to the turn to the social as Plans and Situated Actions (see e.g. Helander 1997). Buxton certainly points directly to the importance of this book, explaining:

"Hutchins refers to … situated activities as "in the wild" in order to distinguish their real world embodiment from some abstract laboratory manifestation that is as idealized as it is unrealistic." (p.37).

This paper has concentrated on a laboratory study by Suchman, and consequently has focused on the ethnomethodological approach taken by Suchman. Below it will argue that Hutchins (1995) also does not exclude the possibility of laboratory studies of the kind undertaken by Suchman.

3.1 Cognition in the Wild

In Cognition in the Wild, Hutchins (1995) does not address HCI, but fundamental questions about human behaviour and cognition. He proposes a view of "*socially distributed cognition*" (p xiii), arguing against studying people in "*the laboratory, where cognition is studied in captivity*" (p xiv) and in favour of studying "*the everyday world where human cognition adapts to its everyday surroundings*" (p xiv). Hutchins' point is that cognition is inextricable from a complex socio-cultural world. While many ethnomethodologists do not agree with Hutchins (e.g. Sharrock and Button 2011), Suchman (1987; 2007) herself has noted similarities of interest and vision.

Hutchins' area is cognitive anthropology. His complaint is that this area had too readily taken up ideas and methods from cognitive science. He believes that cognitive anthropology should attempt to rethink the idea of cognition, rather than simply act as

an extension of cognitive science. He argues cognitive anthropology should use ethnographic fieldwork in order to do this. His argument is not that fieldwork is essential to the study of cognition, but that it is an important first step for rethinking it. He speculates about the possibility of his work eventually inspiring new forms of laboratory study. He also references Suchman (1987) as an example of the style of research he envisages. Hutchins does not argue that cognition is a consequence of its surroundings; his analysis looks not just at setting, but extends into the history, culture and physicality of practice.

Hutchins uses an example of a naval vessel encountering problems when entering port, and goes on to look more broadly at navigation as it is practiced. The situation Hutchins describes is not wild to the vessel's captain or crew, just to a researcher trying to conduct the study. Arguably the sea is a wild place, but the example Hutchins gives is of a highly trained, disciplined crew, working cooperatively in uncertain and dangerous circumstances. In Cognition in the Wild, the purpose of the metaphor "the wild" does not seem to be in evoking human activities in the everyday world, but in distinguishing Hutchins' interests from the laboratory-based studies that were, at the time the book was published, dominant within his particular field. Hutchins does say that the wild is a metaphor that evokes *"a sense of ecology of thinking in which human cognition interacts with an environment rich in organizing resources" (p XIV)* but generally the metaphor does not feature significantly in his analysis. The metaphor provides a way in the opening and summative sections of the book to refer broadly to what Hutchins believes cognitive science and cognitive anthropology have missed (the ways in which people live and work) and distinguishing these concerns from the predominant ways these fields were operating (treating cognition as something isolatable from embodied, social action). Hutchins does not claim there is such a thing as wild-cognition, or that cognition is fundamentally different depending upon whether it is done in one place or another, but that cognition is an irrevocably situated activity. It seems the wild is a useful, but ultimately disposable metaphor; it does not describe the phenomena he is interested in, but sets the stage for his own analysis of situated action.

Similarly to Suchman, Hutchins points out that all human practice is situated within a range of physical, social, cultural, and historical conditions. But rather than turn away from cognition as Suchman did, he recasts the concept along anthropological lines. So while there are similarities of interest, Suchman and Hutchins head in different directions. Whereas Hutchins distances himself from cognitive science by contrasting

his work with that done in the laboratory, Suchman makes no such move. Both are opposed to cognitive science, but Suchman finds no need to distinguish herself from the laboratory itself. Suchman's study should not be seen as of the kind criticised by Hutchins. Hutchins is not arguing that situated action or distributed cognition does not exist in the laboratory, but rather that scientific methods associated with laboratory studies simply cannot see these; people do not think differently in the laboratory, but rather thinking is seen differently.

4. Wild Sociology in the Laboratory

Problems arise when wild sociology (be it ethnomethodology, distributed cognition, or other approaches to social analysis in HCI) is seen as sociology *of* the wild. The wild is becoming a term in HCI for the world outside the laboratory. This use of the metaphor "the wild" is consistent with the way Hutchins (1995) uses it, but for Hutchins it is a disposable metaphor. Hutchins does not theorise the wild, but uses the term to distance himself from cognitive science. Crabtree et al (2012) use the metaphor in an alternative way. They do not see work in the wild as *anything* that takes place outside the laboratory, but as work as it is practiced (as opposed to work as it is specified or theorised). However Crabtree et al approach the study of work in the wild with a priori commitment to fieldwork methods and make no attempt to disassociate setting and the wild. As HCI moves into a post-ethnographic phase, it seems timely to articulate the difference between naturalistic analysis and the analysis of work in its natural setting.

This paper is not arguing that design in the wild and its variants are a wrong move for HCI; I do not argue that HCI should return to purely laboratory-based research. Rather, in the context of design in the wild, it is important that social analysis comes to terms with the laboratory. Firstly, we need to be clear what is being argued against in order to be able to articulate what is in favour of. Secondly, it may be beneficial to work towards design methods that span evaluation in both the laboratory and the field.

4.1 What is Wrong with Laboratory Studies?

Laboratory studies are often talked of dismissively in social analysis papers. Often this is in the form of social analysis being 'not a laboratory study'. An example from Viller and Sommerville's (2000) paper on ethnographically informed analysis is:

"Ethnographic accounts are based upon detailed descriptions of human activity, resulting from prolonged periods as a "participant observer'' in the work setting. In taking this approach, ethnography avoids the problems associated with the artificial nature of laboratory-based study, and produces accounts that are readily understood by the workers being studied." (p.172)

The detailed, readily understandable, descriptions of human activity that Viller and Sommerville advocate are of the type introduced to HCI by Suchman. They also discuss the study of work over prolonged periods, but this in itself does not explain their criticism of the laboratory. It is certainly difficult to conduct a laboratory study over a prolonged period (and, for that matter, to conduct one in a distributed setting) but this is not the reason given for their dismissal of laboratory studies. Rather the problems of the laboratory are put down by Viller and Sommerville to their "*artificial nature*", as if any human action that occurs in a laboratory is itself artificial. Similar language is evident across the ethnographic literature. In their history of ethnographic research at Xerox, Szymanski and Whalen (2011) explain that researchers there were committed to understanding the fundamentally socio-cultural organization of human reasoning and action, but moreover that they were:

"committed to naturalistic observation of that action - to leaving the highly controlled environment of the laboratory so that what humans did and how they did it could be studied in real-world habitats and settings, under ordinary, everyday conditions." (p.5)

Szymanski and Whalen emphasize the importance of naturalism and real-world habitats and setting, contrasting this with highly controlled laboratory environments. Whether they consider all laboratory environments to be highly controlled remains ambiguous. This kind of ambiguity is of no consequence where fieldwork is required to study the relevant practices, but where these practices are transferable to a laboratory setting it stands to be misinterpreted. Szymanski and Whalen surely are not arguing against the use of naturalistic methods in the laboratory, as was done by Suchman. Crabtree et al (2012) are much more specific with their language:

"In a design context ethnography requires that you immerse yourself in real world settings of relevance to your design task. You can't do it from your office chair, or by phone, or by post, or by administering a questionnaire, or by doing an experiment in a lab."

There is an important difference between Crabtree et al's argument and the others. Crabtree does not single out the laboratory, does not emphasise the idea there is a "*real world*" as opposed to an "*artificial*" laboratory, but talks specifically of "*an experiment in a lab*". Others, including Szymanski and Whalen, and Viller and Sommerville, are much more bluntly dismissive of laboratory studies. The consequence of these blunt dismissals is that laboratory settings rather than experiments are viewed as unable to engage with the situated nature of action. Instead of working through whether a study needs to probe features of a specific setting or be carried out over a long period and/or over a distributed area the choice between laboratory study and fieldwork is too readily reduced to a choice between artificial and natural.

4.2 Working Between the Laboratory and the Field

Grudin's (1990) five-stage model of HCI was introduced earlier. Grudin characterised stage four HCI as concentrating on higher-level cognitive issues and as using methods such as simulation and talk-aloud protocols. I noted that Grudin's concept of stage four HCI has been largely ignored by the social literature in HCI, which sees stage five (in situ studies) as a turn away from stage three (laboratory studies in the tradition of cognitive science). However, this is not to say stage four never happened, but that the social literature in HCI tends to have an out-dated notion of the laboratory. The type of work Grudin saw as stage four, now constitutes a major part of laboratory work undertaken in HCI. It is worth taking stock of this.

In HCI, many organisations and research groups continue to have a dedicated laboratory space for conducting research studies. But the portability of computers and recording equipment mean that any meeting room or space will often do. The increasing interest in mobile or outdoor technology also means that laboratory studies can no longer be assumed to be just those studies that actually take place in a purpose built laboratory. Two common forms of study in HCI are usability testing and technology trials. Usability testing is an activity that commonly takes place in a laboratory, but only to the extent that the laboratory will feature the necessary equipment and software, sometimes a video camera, and will often be free from interruption. Usability tests are often structured, often feature people following set tasks (although can increasingly be automated), and often are used to gauge things like intuitiveness, accessibility, speed and/or error rates. In technology trials, a new system is made available to a group of

users, perhaps for a few hours, or perhaps for an extended period. Here the interests of investigators extend beyond technical feasibility to exploring uses that systems are (or are not) put to. Brown et al (2011) explain that trials go under a range of different names, including field experiments, deployments, evaluations, field studies, and technical probes. Trials therefore do not necessarily take place in a laboratory, and if they do this may be in a "living laboratory". In some cases field trials extend well beyond the laboratory, for example the village of Wray in the UK has been used as a living laboratory over a period of several years (e.g. Taylor & Cheverst 2012). Such trials often retain an experimental attitude, if not in the design of the study, then in the demeanour of the participants themselves (Tolmie & Crabtree 2008). From this perspective, the distinction between laboratory and field is blurred. Any clear methodological choice for the researcher between naturalness and control seems to disappear, and instead the concern becomes one of balancing these.

Another common form of study in HCI could be called the simulation. Simulations are more concerned with human practice than with a specific technology. There may be a specific technology involved in a simulation, but the purpose of the study will not be to inform that specific technology but to draw wider points or generalisations. Sometimes a simulation may involve no technology at all, for example some may be paper and pen exercises. Simulations may set out to discover, for example, how people reason, how and why people make errors, how they collaborate, or how they follow instructions. Simulations tend to be laboratory based, and often go hand in hand with video recording. Suchman's study in Plans and Situated Actions could be categorized as a simulation rather than a field trial. The purpose of doing a simulation, as it was for Suchman, is often to find a way of rendering an existing practice in such a way that it can be videoed, and often in such a way that videos can be compared. Often in simulations care is taken to use members of the group being simulated (for example simulation studies of programmers will prefer to use programmers rather than students, see e.g. Baker et al 2012). The artificialities of a simulation do shape the activities of the participants. Elsewhere (Rooksby 2013), I have examined a simulation study and argued that the artificialities are not abstract influences or controls but serve as a resource for participants in accounting for and organising practice.

These are not purely the kinds of "*experiment in a lab*" dismissed by Crabtree et al (2012), and certainly no longer rely on what Suchman would call the planning model in order to describe action. However, these get caught up in the sweeping dismissals of

laboratory studies, as if everything done (or imagined to be done) in the laboratory is some version of cognitive science. Suchman's book Plans and Situated Action contains a study of this kind, but drew on ethnomethodology rather than cognitivist ideas. It is these kinds of study that I suggest ought to be discovered by social analysis. These are not the kinds of study rejected in the sociology literature, but modes of working that have barely been engaged with at all. As the commitment to describing professional practices in the workplace shifts towards the evaluation of technologies that can be used by more or less anyone in more or less any place, social analysis is increasingly brushing up with these forms of study. Rather than dismissing these, social analysis might seek to provide support for existing methods of evaluation, to clarify for example how laboratory constraints shape an evaluation and consider how these might matter. Alternatively social analysis might come to encompass new forms of laboratory study; studies that do not seek to emulate or extend cognitive science, but laboratory studies in the style of Suchman.

5. Conclusion

This paper has discussed a range of material associated with "the turn to the wild" in HCI. The key points it has made are as follows:

The metaphor "the wild" has several uses, and these should not be confused

"The wild" is a metaphor used in HCI to 1) to refer to any place outside the laboratory, and 2) to refer to the situated ways in which people actually act or work (as opposed to process models and similar outlines of procedure). A use of the metaphor in sociology has also been noted: wild sociology. Wild sociology, I have argued, does not require point 1 above. "The wild" is a metaphor that can be used in various ways to contrast one thing and another, and as such there is a danger that theorising it will lead to confusions. The dangers for HCI of confusing points 1 and 2 are that social analysis will become seen as applicable only to the world beyond the laboratory, and that the field and the laboratory will become distinguished as being real and artificial. The work of Suchman (1987), Hutchins (1995) and others should not be seen as supplying a theory of the world outside the laboratory, but critiques of cognitive science and other disciplines for ignoring the ways people act and work. Suchman uses a laboratory study, and Hutchins uses fieldwork; both explore situated action.

Social analysis does not necessitate fieldwork outside the laboratory

Social analysis does not require activities to be studied in particular "natural" settings but rather orients to the ways in which people act and interact in any setting. This is not to argue that setting is irrelevant; setting may be relevant in what people do, for example participants in a laboratory study may orient to features of the laboratory setting, and this should not be ignored. But it should not be assumed in social analysis that any activity that is researcher provoked or constrained somehow is not a valid thing to study. I am not arguing that field studies are unnecessary, but that they are unessential. Fieldwork continues to be necessary for addressing the design of many kinds of systems (for example electronic health records and air traffic control systems), but has never been a requirement for social analysis itself. When fieldwork is necessary, this is not the result of some requirement to see systems in a "natural" setting, but because it is impractical to study them in the laboratory. The word "natural" has been put in quotation marks in this paragraph because it seems as problematic to think of any setting as natural as it is the laboratory as unnatural.

The challenge for social analysis is how to engage with the laboratory

It does not seem practical to expect that all design and evaluation can take place in the field, and it seems unlikely that importing or developing new forms of ethnography will supply solutions for contemporary problems in design and evaluation. Rather, social analysis needs to become flexible enough to approach and meaningfully analyse interaction in places that are sensible according to the kind of technology in focus and its stage in development. The laboratory is often treated as inadequate in HCI under the auspices of a natural/unnatural distinction. However, laboratories are part of the real world. Suchman has demonstrated the point that what goes on in them turns upon the same interactional resources that people call upon anywhere. Engaging with the lab will therefore require the same real-world treatment and require the garnering of empirical insights and materials as with fieldwork. Such engagement is likely to support design and evaluation more readily than forms of social analysis that rely on ethnographic fieldwork alone.

The enduring relevance of Plans and Situated Actions

I believe Suchman's book Plans and Situated Actions (1987) holds continuing relevance for HCI. Instead of seeing this book through the lens of ethnography, it is timely to rediscover it as a laboratory study. HCI is moving into a post ethnographic phase, where attention is shifting away from the development of specialist, information technology for

workplaces, and towards the development of products. Returning to Plans and Situated Actions will not supply answers to the question of how to design and evaluate these products, but will help in abandoning an a-priori commitment to fieldwork and moving to methods that span laboratory and field.

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