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AUTHOR Parry, Odette; And Others
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ABSTRACT

Key aspects of the academic socialization of doctoral students in Britain are described by comparing and contrasting supervisors of Ph.D. candidates in a natural science and a social science discipline. The role of the supervisor in the production of academic elites is highlighted in the two very different academic research traditions. A total of 49 interviews were conducted in Social Anthropology, 24 of which were with Ph.D. students, and 25 of which were with Ph.D. supervisors. In departments of biochemistry, 8 interviews were conducted with heads of laboratories, and 28 were conducted with doctoral students. Myths that have developed as a result of inadequate information on British doctoral socialization in these sciences have suggested that the ways of doing doctoral research in these disciplines are quite different. In fact, these myths are substantiated by the fact that the structure of research in anthropology revolves around the student-supervisor relationship, but in biochemistry, it appears guided to a greater extent by the principle of teamwork. The role of the supervisor, however, is more similar than would appear at first glance, because the social anthropology supervisor is less directive or important day-to-day than first appearances suggest. (Contains 27 references.) (SLD)

# DISCIPLINARY ELITES AND QUALITATIVE RESEARCH

Odette Parry, Sara Delamont and Paul Atkinson



## Introduction

We describe key aspects of the academic socialization of doctoral students in Britain[1] by comparing and contrasting PhD supervision in a natural science and a social science discipline. We highlight the role of supervisor in the reproduction of academic elites in two quite different disciplinary research contexts and within contrasting academic research traditions. The paper examines the nature and production of disciplinary knowledge in academic socialization. It also focuses on the relevance of qualitative investigation to an understanding of social reproduction in the academy.

For the purposes of this paper we have elected to examine Social Anthropology and Biochemistry. There are several reasons informing this choice of disciplines. The first is that both social anthropology and biochemistry are 'single' or 'primary' disciplines, characterised by a discrete subject area. They are defined by theories and methods which are identified as intrinsic to their respective academic traditions. The second reason for our choice is that these two disciplines are traditionally characterized by quite different methods of enquiry. The third reason is that whereas in the case of social anthropology the research method (ethnography) is that used by the research team during the course of the research, in the case of biochemistry this is clearly not the case.

Although the paper draws on data from two disciplines it is informed from a much larger data set which was the product of two national research projects looking at doctoral study in Britain. The first research focused on PhD work in the Social Sciences and was carried out between January 1990 and January 1992. Here we looked at the processes of PhD research and the transmission of knowledge. The data facilitated a comparison of the socialization of different types of PhD students, in contrasting institutions, on selected areas of study within the social sciences. During the course of this research we carried out a total of 187 ethnographic interviews with PhD students and supervisors in five social science disciplines: Human Geography, Social Anthropology, Town Planning, Area Studies and Development Studies.

The second study, carried out between February 1992 and January 1994, examined the socialization of science doctoral students. Again we were interested in the social contexts in which socialization takes place and the production of knowledge at PhD level in selected science disciplines. This research was similarly realised through detailed case studies of PhD socialization. Data were collected primarily by ethnographic interviews with students, post-doctoral researchers, supervisors and other key members of participating departments (particularly technical support staff) with the addition of some observation of students at work. Subject areas included in the study

were Biochemistry, Physical Geography and Artificial Intelligence. In this project we interviewed 107 PhD students, post-doctoral researchers, supervisors and technical staff in a total of eight different institutions. Observation was carried out in one department for each of the three disciplines in question. This observation comprised watching PhD students at work, both in laboratories and at work stations, attending postgraduate seminar meetings and observing supervisor-led discussion groups for doctoral students.

Both pieces of research sought to bring together perspectives from different sociological traditions, drawing on classic work in occupational and professional socialization and the broadly structuralist perspective on academic and educational knowledge derived from Bourdieu (1988), Bourdieu and Passeron (1977, 1979), Harker and Wilkes (1990), Robbins (1991). These theoretical interests have been informed by our previous research and foreshadowed by earlier publications (Atkinson 1981,1983,1984, Atkinson and Delamont 1985; Parry 1988, 1990 1992, 1993).

## Accessing Educational Elites

Individuals who control access to research settings are aptly described as *gatekeepers* as it is with in their power to allow or withhold permission for researchers to enter (Lofland 1971). Gatekeepers are motivated by the interests of the institution and its members to discern whether or not the researcher is out to hurt or harm them (Hammersley and Atkinson 1983). It is not surprising therefore that access is often a complex and protracted sequence of negotiations, in which the researcher may not always be privy to past events and experiences which have informed the disposition of those from whom they seek approval (Barton and Lazarsfeld 1969).

Where researchers are privy to past events affecting access negotiations, by virtue of their association and familiarity with the respondent group, the vigilance of gatekeepers may be increased. Concern may be heightened when familiarity is such that the institution and it's members feel unable to project the image of their organization which is routinely accepted as a lay portrayal of it's activities and events. Anxiety may be further exacerbated if the researcher intends to use the same methods of investigation favoured by the institution and it's members in the execution of their routine activities.

It was anticipated that research by academics of academics would be susceptible to a number of problems which would not necessarily be encountered in other research settings. We were therefore not surprised to find the respondent group most anxious about the proposed research, were social science academic staff in the departments/institutions which we approached. The research team were further aware that the research coincided with a period during which members of certain disciplines and departments were particularly sensitive to scrutiny. At the time of the research the Economic and Social Research Council in Britain were revising the structure of doctoral study. In an attempt to raise submission

rates for social science PhDs the Research Council instituted a sanctions policy which discriminated against institutions with low rates of submission. Institutions not meeting the required submission levels were sanctioned and did not qualify for Research Council studentships. At the time of our research every department which we visited had been, was currently or was about to be so sanctioned by the Research Council. Because of this the negotiation for access was, in some cases, delicate and protracted. Many of the academic staff were - understandably - nervous about the Research Council (who were funding the research), and its scrutiny of their research training heightened awareness about research training in general. All of these factors added to the sensitivity of the research.

PhD students at the selected sites were consulted about the research and the decision to participate, on an individual basis, was theirs. The concerns of this group largely revolved around their supervisory arrangements; Phd students were understandably wary of upsetting their supervisors at such a critical stage of their academic career.

While every department we approached agreed to grant us access, and while academic colleagues talked to us freely, we were trusted not to reveal their identity to the Research Council, their academic peers or to senior management within their own institutions. We have been careful to preserve the confidentiality of all our respondents by taking the following precautions: deliberately fudging the precise numbers of departments visited in any given discipline (as in this paper); falsifying inessential details about people's research when quoted for illustrative purposes; using reporting strategies which avoid unduly precise characterization of particular departments; and disguising individual identity.

The exact number of departments we visited remains, for these reasons, unspecified. However we are prepared to say that for each of the disciplines we studied we selected at least two departments as our main sites but we also interviewed supervisors and PhD students in other institutions.

**Doctoral Research in Britain**

The Winfield Report (1987) highlighted a dearth of good data on the way in which PhD work is carried out across and between different disciplines. Delamont (1987) and Ashmore, Myers and Potter (1992) have also drawn attention to a lack of ethnographic work focusing on socialization into science. The absence of data has encouraged the growth of 'myths' surrounding the way in which different disciplines carry out doctoral research. During the course of our research several of the respondents (science and social science PhD students and supervisors) evoked these 'myths' when they compared ways of doing doctoral work in the social sciences with the natural sciences. Whereas the social science Phd was seen typically as the product of a one to one (plus) relationship between student and supervisor(s), the science PhD was seen as the product of a laboratory based research group or stable, based on the concept

"team work" (Becher 1988). In the following sections we consider the roles of supervisor within the two 'ideal types' of doctoral research as they emerged from the ethnographic interviews carried out with supervisors and doctoral students in social anthropology and biochemistry.

## Social Anthropology

In Social Anthropology we carried out a total of forty-nine interviews, twenty-four of which were with PhD students in various stages of their research. The remaining twenty-five interviews were carried out with PhD supervisors (a percentage of whom were Heads of Departments) and other senior academic staff.

The organization of doctoral work within social anthropology was found to revolve around supervisor (or panel of supervisors) and the doctoral student to a large extent. The significance of this relationship in part stems from the importance attached to regional specialization within anthropology. Doctoral students selected or were allocated to supervisors according to the region of their intended study. The identification of regional expertise was seen as a crucial stage in the PhD application procedure.

The regional specialisation of social anthropologists formed an important part of their academic identity. All twenty-four students were already clear about the area of the world where they would work even if they had not yet done their fieldwork. The overall pattern of anthropological field work also appeared very traditional: most of the British students were heading for third world countries, while overseas students researched their own societies, not Britain.

The relationship between supervisor and anthropology PhD student was the pivot around which doctoral study was structured. There are a number of explanations for this most of which stem from our observance that anthropology doctoral students lack a peer group found to a greater extent in other disciplines. First there are relatively few departments of social anthropology in Britain, and these departments accommodate small numbers of doctoral students. Second, few departments offer undergraduate degrees in social anthropology hence only seven out of our twenty four anthropology respondents graduated in anthropology, the rest arrived in the discipline through either anthropology MSc's or conversion courses to the subject. Third, the majority of anthropology PhD's spend a substantial period of time in the field and those who register for their degrees simultaneously rarely leave for the field, or arrive back, at the same time. Because of this it is difficult to sustain peer relationships throughout the course of their study. Furthermore, whereas other social science disciplines often provide methods course work for their new PhD students this was not found to be the case in social anthropology. Therefore there is little opportunity for doctoral students to meet together regularly in a formally structured context.

*Fieldwork*

Given that anthropology doctoral students appeared relatively isolated from their peers we were interested in the extent to which students relied upon their supervisors for research support and supervision in their day to day research activities. However we found that the 'traditional' paradigm of anthropological work was highly evident in that supervisors tended to describe PhD work as a 'rite de passage', to be experienced uniquely by the doctoral student. At the heart of this experience lay 'anthropological fieldwork'.

Anthropological fieldwork formed the root or essence of academic identity for members. Many of our anthropology respondents, like this post graduate, cited fieldwork as the key feature distinguishing anthropology from other social science disciplines;

> I think the most important thing to ask people (is) why anthropology is different from other subjects and what they think is special about it. Because it does present special problems of which as a PhD student, fieldwork stands as the central difference with other subjects.

Anthropological work is by tradition highly empirical. This means that members who haven't completed empirical PhDs or carried out substantial fieldwork tend to become marginalized in the discipline. So for example a supervisor suggested to us that non-empirical PhDs could not really;

> ...exist in anthropology because it's very very exceptional not to have fieldwork as part of a PhD.

In some cases this can cause problems for staff and postgraduates not carrying out substantial piece of fieldwork or those who are completing non empirical or theoretical PhDs. These individuals can and often did find themselves marginalized within the discipline. The importance of fieldwork to anthropological careers is crucial and one reason for this is that the successful fieldwork experience is seen as a union ticket to disciplinary membership. The product of fieldwork, and particularly in doctoral study, was expressed by members as cultural capital, to be exploited throughout the anthropological career;

> It's much more about acquiring this body of cultural capital than it is about acquiring an intellectual tool kit.(Supervisor)

Fieldwork lies at the heart of anthropological work and informs the essence of disciplinary identity for it's members. Central to this identity is the concept of anthropological understanding, which is reached through the fieldwork experience. This is not however a process which readily lends itself to explication, but relies on tacit and implicit processes which themselves inform the character of anthropological work. In the account given below, an anthropology

PhD student describes the way in which anthropology differs from other social science disciplines in the focus of its essential interests;

> If you take a thing like pig husbandry you would think it was a very straight forward thing. As an agriculturist or an economist you will think there is such a thing as pighusbandry, and we do it in a certain way and they do it in a different way. I'm going to find out how they do it and that's it. And I think as an anthropologist you have to do this, but you have to go one step further in trying to understand why they do it in the way they do it. Their rationale behind it and what kind of model of husbandry they have. And this is not done by any other discipline even if they work with the same issues underground. the anthropological understanding goes beyond that.

The way in which anthropological understanding is reached does not readily lend itself to explanation. Respondents' accounts were couched in vocabularies based on tacit understandings rather than any explicit criteria. Explanations were embedded in the discovery processes, which as an anthropology PhD student explains are both personal and implicit;

> As an anthropologist I have my focus basically on things which are intangible...how people generate ideas and communicate them.

In doctoral research we found methods were determined by the fieldwork experience and not predetermined prior to that experience. Students, like this post graduate anthropologist described how reflexivity is an important aspect of doing anthropological fieldwork;

> ...If one takes the image of the handyman, you go out with a pile of techniques in your mental suitcase, you don't know which one you're going to be able to use. You've no idea of the kind of situation.

The notion of anthropological understanding is related to issues of cultural difference which means that research techniques are seen as necessarily contextually dependent. The anthropology PhD student below, describes how the techniques which he/she employed during the course of her own field work were;

> ..mainly determined by constraints in the field, because when you go in and choose one subject as an anthropologist, you don't always find that it is relevant to the people among whom you're living, and you don't always call the tune as part of your methodology. You pick up on things which are important there.

A similar view is expressed below by an anthropology PhD student, who contrasts anthropological fieldwork with that in another social science discipline and the implications for research training in anthropology;

> The tenor of anthropology compared to sociology is that you go and find out things without knowing what questions to ask...and that's why in a sense there's an argument for not having much training before the fieldwork.

We found a shared understanding among the respondents that anthropological methods did not readily lend themselves to formal instruction because their principles defied translation into teaching formula. The two accounts below are provided by anthropology supervisors;

> The participant observation is not, I would say, a research method which can be taught in the classroom and applied in the field, whereas statistical methods can be taught in the classroom and applied in the field...But of course participant observation is hardly a method. I think it's the **sine qua non**. It's something you can only learn by doing it.

> ... all this business of training I think is largely spurious, it's something that's learnt by the experience of doing it, it's rather like teaching music, you can't teach people to play without a piano, it's only by playing that they can learn, and I think fieldwork is like that.

This traditional view of fieldwork was shared by many of our informants;

> ...field work was something you did and you couldn't explain it any more than you could explain how you keep your balance on a bicycle...and something of the same anomaly seems to me to pertain to the notion of teaching participant observation, teaching how to be perceptive, teaching how not to put your foot in it socially, teaching how to be subtle. Because all these notions are culturally specific, you see.

The very nature or essence of anthropological fieldwork was translated to us in terms of tacit and personal experiences;

> The debate over the fieldwork experience, the way knowledge is constructed out of observation, interaction between the informant and the investigator, the general reflexivity of the process is very much the stuff of the training before going into the field. ... there remains a certain mystique about (it),. Yes, in order to do the **rite de passage** properly you've got to do it by yourself.

Not only the research but also in evaluation, anthropology was described by its members as a personal and subjective experience;

> I think it's a discipline that rests very much on opinion and interpretation and very little on established methodologies. So there aren't any criteria for judging anyone, except what you personally feel about their work.

and specifically relating to the evaluation of PhD work;

> ...in other words we don't have a lot of criteria we're looking for, that exists on a conscious level. But in practice we end up with implicit or unconscious ideas that we're looking for.

To reiterate, whether abroad or at home, ideas about cultural difference were implicit in anthropological methods. Cultural difference implied elements of both strangeness and unpredictability which in turn informed methods of application. Methods were described as contextually dependent and choice of research techniques was therefore determined by constraints in the field rather than being imposed from outside.

Because of the unpredictable element in anthropological fieldwork, predetermined research strategies were felt to hinder rather than assist data collection. Good students were those who maintained an open and flexible approach to data collection and good research projects were those which developed in the field through the experiences of the fieldworker.

Implicit in the belief that research strategies can not be anticipated was the idea that fieldwork necessitated employing skills outside of the accepted range of research techniques. These skills were described in terms of the personal qualities of the researcher such as empathy and reflexivity.

The unpredictable and highly personal element of qualitative data collection meant fieldwork was celebrated as a highly subjective experience which did not lend itself easily to objective evaluation. At one extreme, in anthropology, data collection period was seen by many respondents as a rite de passage and as such to be experienced uniquely by the practitioner as an 'ordeal by fire'. Although this traditional attitude towards anthropological fieldwork differed between departments, we did find it extended to home PhD students working in Britain. During the fieldwork duration these students tended to keep away from the academy and had limited (if any) contact with supervisors.However we did find that the anthropological 'ordeal by fire' approach to fieldwork characterised departments which were most traditionally academic. Where anthropology was more development oriented then data collection was not treated as a personal experience to the same extent. For example PhD anthropology students working in development were more likely to receive supervisory visits in the field and in some instances work as part of a regional specialist team as opposed to, in isolation.

**Biochemistry**

We carried out a total of thirty-seven interviews in departments of biochemistry. Of these thirty-seven, eight were interviews with the heads of laboratories who supervised biochemistry PhD students in their labs. The remaining twenty-nine interviews were with biochemistry doctoral students and biochemistry post-doctoral researchers. The ratio of supervisor to student among our respondents was much less than in anthropology because in this research we focussed upon research laboratories in which only one or two research directors were officially responsible for the supervision of PhD students, post-doctoral researchers and technical personnel. It was common therefore for several post-graduates in the laboratory to have the same supervisor who in turn may be supervising up to ten doctoral students at different stages of their research.

The way in which PhD study in the laboratory was organised differed substantially from arrangements characterising social anthropology. In the laboratory, research structure revolved around one supervisor or research director with doctoral students and post-doctoral researchers working in his/her area on topics that were to some extent related. In this context specific sets of research related patterns emerged which informed the 'habitus' of the group. This model was offered to us in contrast to the 'traditional' model of PhD supervision in the social sciences;

> The difference between us and social science is that we tend to do PhDs through team work. (Supervisor)

Because supervisors or research directors tended to have several PhD students at any one time they took a back stage role in regard to practical day to day supervision of students;

> The biggest satisfaction I got out of last year was not the research papers, but the fact that there was eight people in a room working in an area that they wouldn't have been otherwise. And I think my leverage, what I contribute is not sitting there and doing research myself but creating the possibilities for other people to do it and trying to shape the direction of what gets done. So really it's more the business of leading science and making things happen(AI Supervisor)

The supervisor's main role as research director means that assistance with everyday problems concerning postgraduate research are resolved else where; and hence the research group/laboratory culture is one which is supportive to members;

> They (PhD students) realise quite quickly that its much quicker to ask somebody than try and find out themselves. Though when

> they first start they often feel they have to find everything out themselves. Of course nobody minds if they ask...people tend to know what everyone else is doing and has done and you might well say ' I should go and ask so and so about that. (Biochemistry Post Doctoral Researcher)

Where group members were working on different research problems there were still overlaps in the materials and techniques which they used, and less experienced members on the group relied upon the more experienced members;

> If things keep going wrong then usually someone who gets it right will sort of go through the experiment one day with you (Biochemistry Doctoral Student)

Given the numbers of doctoral students per supervisor it would not be feasible for supervisors to take full responsibility for training graduate students. In the following account a post-doctoral researcher explains how a doctoral student in the laboratory made out;

> In these initial months his main source of help was (a post-doctoral researcher). He works at the next bench. He was very keen and interested in what the student was working on. The thing about biochemistry is that often nothing works then the only thing you can possibly do to come to terms with that is to grit your teeth and carry on. He didn't see his supervisor very often during that time because the supervisor had a lot of paper and administrative work to do and anyway the post-doc's advice was very good. He's working in the same area. The student is developing a new technique but the post-doc was the person to ask because he had so much experience in the general area. He went to his supervisor for ideas and whenever unexpected results and then the supervisor would suggest new things and directions in which he should go.

The type of PhD organisation described above is only possible where certain conditions prevail. The two crucial features are group size and continuity of research. Only where there are sufficient numbers of postgraduates (at different stages of research) and post doctoral researchers can the team or group model of supervision operate. Also the group or team structure depends upon a continuity of funding which allows for several individuals (students and post-doctoral researchers) working in the same area both simultaneously and in succession. Under these conditions, topics or projects can logically follow on from each other with new PhD students developing the work of previous students. In this way a pedagogic continuity operates as skills and equipment are handed down through the research group. Using a 'stand analogy' of scientific research developed by Hacking (1992) we can see how the interests of group members are mutually intertwined in a linear process through which the work of individuals is shaped and developed.

Within the laboratory doctoral research revolved around mutual support and sharing of materials, skills and equipment. An important feature of this model is a continuity of practice in that skills, equipment and topics are passed down through the ranks of post-doctoral researchers and research students.

Unlike social anthropology therefore the research structure for PhD work in biochemistry revolves around the laboratory team which provides both support and supervision on a day to day basis for the research student.

*Scientific Knowledge*

Doctoral research in biochemistry is grounded in a set of pre-established ideas, materials and skills which inform the content of scientific knowledge. This pre-established knowledge includes background theory and ideas, the tools and apparatus which are used for modification of those ideas and the modes of analysis through which results are interpreted. In one sense doctoral research in biochemistry can be seen as having a highly material context through which scientific outcomes are realised. The material context for doctoral students is partially a function of earlier research from which current projects are developed. In the following account a doctoral student explains how his research fits into this context;

> (A previous PhD student) was funded to do his research on this enzyme. There was a person working on the enzyme before him. The first person tried to purify the enzyme and came quite close. Then a year later the PhD student started and actually purified the enzyme and started working on it. Now I shall be taking that work a little bit further. (Doctoral Student)

Another source of pre-established knowledge upon which doctoral students rely is the published findings of previous research. Latour and Woolgar (1979) argue that publication is an essential component of a process of scientific reification whereby tacit skills or material equipment (often the product of grant funding themselves) become the acceptable tools of other laboratories. In this way laboratory production activity can be maintained and expanded.

PhD work in Biochemistry differs from anthropology in that it entails the identification of goals which can be realistically achieved within given time spans. Contrary to the idea that Scientific PhD students actively pursue an open ended and long term commitment to scientific discovery our data concurs with that of Knorr-Centina (1981:59) in that they do not commit themselves to a journey of unknown destination. Rather;

> ..they choose a known destination at which it seems likely they will arrive not only on time, but ahead of anyone else.

Doctoral study in biochemistry in this sense is the antithesis of anthropological research as the latter is designed to be developed in the field through the experiences of the fieldworker. Anthropological methods are contextually dependent and techniques are determined by constraints in the field rather than being imposed from outside.

In contrast most of our Biochemistry supervisors had been responsible for setting up PhD research, attracting funding, and inviting applicants. Although many of our PhD respondents claimed to have a reasonable amount of freedom in their everyday work, ie. in following their own leads, ultimately most were aware of having a fairly tight research brief from the outset. Accounts suggested that in biochemistry the PhD is constructed to meet two important criteria. Namely, PhD students need to produce results and they need to produce these results within a specified time period. Although there is some ambiguity (we were constantly reminded that even no results or negative results could be written up as PhD material) generally the supervisors we talked to preferred results. In pursuit of results the course of a PhD students research could drastically change;

> Where experiments, or an experiment is not working my attitude is don't flog a dead horse. So change the obvious things and if that doesn't work either we switch to another sort of experiment. For example if th y have to purify enzymes, they may not be able to do it. They need a second chance to get results. And they need some results. (Biochemistry Supervisor)

*Lab Work*

Before even entering the research laboratory, many students of biochemistry have experienced science as pedagogically stable. These experiences lead to expectations that goals of laboratory experiments are realisable and that outcomes are certain. However, as noted by Delamont and Atkinson (1985), and Collins (1985:35), experiments which are carried out as a routine component of education and training address questions to which the answers are already known and are constructed to produce only successful conclusions. The post-graduate students we interviewed had completed practical (laboratory based) projects in their final year of undergraduate study, but as a biochemistry supervisor confirmed,

> In their undergraduate training they get very little undergraduate experience, in their final year project they get a feel and that's all. When you're selecting students you do it purely on academic ability and some people are useless in practical terms and then you really suffer. Fortunately I haven't had any like that. It takes a long time to train them. At undergraduate level the experiments are designed to work, that's why they're chosen. Someone once said if you took every thing that worked in the lab over the course of a year it would be two weeks work in the lab.

Many postgraduates come to doctoral research poorly prepared because the differences between undergraduate and graduate science are largely unanticipated. Whereas at undergraduate level students expect their experiments to work, at postgraduate level they can never be certain. The biochemistry postgraduates acknowledged a lack of preparation for postgraduate work, and despondency and sometimes panic when their experiments consistently failed. The following description of initiation into PhD work is provided by a biochemistry doctoral student and is representative of the accounts provided other postgraduates;

> The first thing I had to do was make an RNA and I kept failing. It took me three months before it started working. The thing about biochemistry is that often nothing works and the only thing you can possibly do to come to terms with that is to grit your teeth and carry on trying. It came as a big shock to me that nothing worked.

The realisation that the outcomes of laboratory work are by no means certain, accompanies a growing concern among post-graduates that there is nothing predictable about doctoral study and there is no guarantee that PhD requirements will be met;

> It's that its suddenly for real. You're not playing any more but that it's completely open ended and there is no guarantee that its going to work. It's three years of your life and it could easily go down the toilet. (Biochemistry Student)

Biochemistry doctoral students experienced frustration because initially "you can't get something to work" and "you can get to your wits end trying to get something to work". We also found that just because an experiment had worked once, there was guarantee that it would work at any time in the future;

> When you run a test you might do it once and it works. You do it 4 or 5 times more and it doesn't work. (Biochemistry post-graduate)

Although one explanation for this was the sheer volume of variables involved, "for a single experiment there may be 6 or 7 variables, for a complicated one there could be hundreds", this does not explain why some experiments work first time and others don't;

> Sometimes you'll do something for the first time without any rhyme or reason it will work, and other times things that should work won't.

Therefore whilst biochemistry doctoral students work to a more tightly structured research topic or problem, the nature of scientific research means that

their day to day research experience is an unpredictable as that of anthropology students;

> I start off between eight and eight-thirty in the morning and stay according to how much I've got to do. Usually I stay all day and sometimes much longer. You can't plan very much what you're going to do ahead because you may start an experiment and then things could go very wrong. So I couldn't plan over the next week, because my plans will change according to how successful my experiments are. Also if I have a positive result to an experiment I may change my mind as to what I'll do next. That would mean I'd have to alter my plans.

The capricious nature of scientific knowledge has implications for the transferral of knowledge in that the transmission is seldom straight forward. Previous research (Collins 1985:56) suggests the flow of scientific information travels best where there is personal contact with an accomplished practitioner and where it is already tried and tested. It is capricious in that similar relationships between teacher and learner may or may not result in the transfer of knowledge.

One aspect of tacit work described by our biochemistry respondents was 'bench work'and is explained below by a biochemistry post doctoral researcher;

> You get a feel for working at the bench and you get a real feeling for it. We deal with tiny amounts and you need confidence in dealing with that. You have to overcome being tentative and it's a well known saying 'being good at the bench'. People who are good at the bench take a protocol and fiddle around with it and they will get it working. Other people are really cack-handed. It's a knack and it's virtually impossible to teach. Some people who are good at the bench seem to do many things wrong and yet their experiments work. And some appear to do it perfectly and it never works. There was this one person here known as the cowboy, but his stuff always worked. (Biochemistry Post Doctoral Researcher)

The way in which our biochemistry respondents described the tacit nature of the skills involved in carrying out laboratory work in some senses therefore bore remarkabe similarity to the accounts we received from anthropologists about the nature of ethnographic research.

**Discussion**

In this paper we have considered doctoral research in two contrasting disciplines. Myths which have developed as a result of inadequate information on British doctoral socialization in the social and natural sciences suggest that the ways of doing doctoral research in these respective disciplines are quite different from each other. Indeed much of our data substantiates these myths in

that the structure of research work in anthropology revolves around the student supervisor relationship whereas in biochemistry it appears to be guided to a greater extent by the principle of team work.

Within these two contrasting models, the paper has looked at the role of supervisor. Initially it appears the role of supervisor differs in the two contexts, with the supervisor adopting a far more prominent role for the every day research work of post graduates in social anthropology. On closer inspection we find this misleading as the social anthropology supervisor is far less directive or important in the day to day management of research than first appearances would suggest. Because the transition from neophyte to anthropologist is achieved via rite de passage or 'ordeal by fire', the doctoral student acquires the necessary disciplinary prerequisites through the unique experience of doing anthropological fieldwork. That fieldwork is contextually dependent and unpredictable means that anthropological methods and techniques are not seen by members of the discipline as teachable skills. Rather they are described as tacit, implicit and uniquely personal, to be intuited by researcher in situ.

A version of the tacit knowledge which characterises anthropological work was also apparent in biochemistry, where doctoral students must grasp the intangible and capricious nature of scientific knowledge. Collins, referring to the performance of skills without being able to articulate how they are achieved, argues that 'tacit' knowledge (Polanyi 1958:67) is a crucial component of scientific work . This leads Collins to propose two models of learning. The first relies on formal instructions (algorithimical model), and the second upon social skills (enculturational model) Whereas the algorithmical model views knowledge as the sort of information that a computer needs to perform to expectations, the enculturational model is based on a set of social skills.

Complementary concepts have been developed by Jamous and Peloille (1970) to characterise different definitions of academic and professional knowledge. Technical knowledge is defined in terms rendering it amenable to documentation, prescription and explicit formulation. Conversely, indeterminate knowledge is defined in terms defying translation into techniques, skills and formulae. Whereas technical knowledge is believed to lend itself to formal instruction, aspects of indeterminate knowledge are felt to defy translation into explicit transmission. Indeterminate knowledge is held to be 'caught' rather than 'taught', transmitted via personal experience rather than by systematic instruction.

In the same way that anthropologists described the skills necessary to their work Collins (1985) describes scientific work which cannot be prescribed by written instructions. It is interesting that in both anthropology and biochemistry the analogy of riding a bicycle was to explain the 'indeterminacy' of the skills required to carry out their respective work.

From our data it is apparent that disciplinary work is characterised by different levels of both indeterminacy and technicality in same way in which both

elements are represented in different forms of occupational work (Atkinson, Reid and Sheldrake 1977). In a previous publication we have suggested that whereas technicality indexes quantitative research methods, indeterminacy indexes qualitative research work (Parry, Atkinson and Delamont 1993). Given the level of indeterminacy we found in both social anthropology and biochemistry doctoral research we should not be surprised therefore to find a qualitative approach highly suited to the needs of the present research.

Prior to carrying out the fieldwork which has provided the data presented in this paper the research team were sensitive to issues which may arise from the experience of researching academic colleagues. We were particularly sensitive to our usage of ethnographic interviewing for the anthropologists among our respondents. Indeed many of our anthropology respondents commented with both irony and humour on our choice of anthropology as a subject for the research and initially the researcher, found it somewhat unnerving to interview members of a discipline which itself relies so heavily upon participant observation.

However, as it transpired our concerns were largely groundless. Our own disciplinary background presents participant observation as a teachable and learnable skill which, within sociology, is routinely taught at both undergraduate and postgraduate level, quite removed from the anthropological ethnography described here.

Furthermore because of the element of 'tacit knowledge' informing the accounts of our biochemistry respondents, we found qualitative methods which were employed during our research both appropriate and acceptable to the members of the biochemistry departments which we visited.

## Notes

(1) An Explanatory Note for American Readers

In the United Kingdom the PhD is a research degree. There are not necessarily any taught courses, and no qualifying examinations. The student chooses a topic and conducts research on it. In Humanities and Social Science the student is often working alone for three years or more, producing a text of around 100,000 words. In science, the student is most often based in a laboratory amoung others, but is still solely responsible for the production of a text. Students have one (or two) supervisor(s) and the supervisor provides the academic training and guidance. That relationship is central to the 'success' or failure of the graduate student.

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