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Narrative stability in interview accounts

Paul Atkinson, School of Social Sciences, Cardiff University

Cathy Sampson, School of Social Sciences, Cardiff University

Address for correspondence:

Professor Paul Atkinson

Glamorgan Building

King Edward VII Avenue

Cardiff CF10 3WTWales, UK

AtkinsonPA@Cardiff.ac.uk

Abstract

We identify and discuss the phenomenon of narrative stability, in the context of current methodological literature on interviewing. It derives from two independent studies, undertaken fifteen years apart, of members of the same genetics research group who were interviewed by different researchers. The first ('Discovery') interviews were collected very soon after the breakthrough was first published. The second ('Legacy') interviews were based on informants looking back at those events. Some strikingly similar narrative episodes across those accounts suggest strong narrative stability. In the course of interviews informants reproduce biographical stories that are well sedimented. Not all interview materials, therefore, should be thought of exclusively in terms of co-production between interviewer and informant.

Keywords: interviews; accounts; narratives; narrative stability

Introduction

Interviews are among the most widely used forms of data collection in the social sciences. Their proper treatment and the status of the data so derived have been matters of contention for a number of years. Here we use a rare opportunity to make a methodological assessment of some features of interview-derived accounts; more substantive analyses derived from those data sets have been published elsewhere (Atkinson, Batchelor and Parsons 1997, 1998; Sampson and Atkinson 2011, 2013). The data-sets consist of two series of interviews, conducted some fifteen years apart, derived from the same scientific research group. The first

data set derives from 1992. The research group in question had very recently been responsible for a major discovery in the field of genetic medicine. The breakthrough was the location and nature of the gene responsible for Myotonic Dystrophy DM), published simultaneously in three papers by several research groups that had engaged in an international process of collaboration and competition (Harper et al. 1992; Harley et al. 1992; Brook et al., 1992). The first set of interviews were conducted shortly after the discovery was announced, and the sociological study documented the trajectory of that discovery before and after that dramatic event; it also traced the immediate consequences of that event for the members of the research group. The second set of interviews were gathered by Sampson in 2007 in a study of the effects and implications of genetic diagnosis of DM over a longer time-frame, including the periods before and after the successful cloning of the gene and the publications announcing that discovery. Part of that study was based on interviews with the same research-group members, who were again asked about that particular genetic discovery. For ease of reference we call the original interviews the Discovery accounts, and the subsequent interviews the Legacy accounts.

The fact that members of the same research group had been studied from a similar social-science perspective fifteen years apart provides an unusual methodological opportunity. Clearly, it allows us to compare the accounts, derived from the same informants, about the same events. Here we do so for a particular analytic reason. But we do not do so in order to examine the veracity of those accounts. We do not assume, nor do we claim, that multiple interviewing – however separated in time the interviews may be – provides a check on the truth of the reported events, or of the reliability of the informants' accounts. None of the discussion in the rest of this paper is about the scientists' veracity. It is, not therefore, an exercise in fact-checking. Rather, our focus is on the relative *stability* of those accounts over time, and hence on the nature of accounts and narratives as phenomena in their own right. The interviews were conducted quite independently, by two quite different researchers (both female).

Interviewing

The nature and role of interviewing in contemporary social research is, in some respects, contested. On the one hand, there is a radical scepticism that, at the extreme, would imply little or no useful role for interviewing. At best, interviews could be analysed only as social encounters in their own right, and the talk so generated of no referential value. Analysis could

therefore only be performed on ‘interview talk’. This radical critique is embedded in a wider criticism of a reliance on interviews more generally, including the pervasive ‘interview society’ that is founded on an implicit faith in the revelatory power of the personal interview; see Silverman (2017) for a robust re-statement of that position. At the other end of the spectrum there is a celebration of the interview as a means towards privileged access to the interviewee’s private self. The interview is thus promoted as the method of choice for the exploration of ‘selves’ and the articulation of ‘voices’.

A great deal of the contemporary literature on interviewing stresses an approach that departs from previous (alleged) emphases on asymmetry and detachment. Contemporary approaches to interviewing, including so-called postmodern interviewing (cf Gubrium and Holstein 2003), are likely to stress some or all of the following aspects of the interview, in terms that contrast with previous approaches. The postmodern interview is an active one. That is, the interview is not a transparent medium for the elicitation of information. Rather, the respondent is engaged in constructing versions of reality in the course of the interview itself: ‘Constructed as active, the subject behind the respondent not only holds the details of a life’s experience, but, in the very process of offering them up to the interviewer, constructively shapes the information’ (Gubrium and Holstein 2003: 32). In this version of interviewing, therefore, the encounter and its contents are seen as *co-constructed*. This is in turn related to an emancipatory agenda – of giving ‘voice’ to respondents, and allowing their ‘stories’ to be heard. In other words, it is not just the respondent who is active in the interview. Interviewer and interviewee are understood in terms of a dialogical relationship: ‘If interviews are interpretively active, meaning-making occasions, interview data are unavoidably collaborative ...’ (Gubrium and Holstein 2003: 68). Fontana (2003: 52-53) likewise articulates a view of the interview as a collaborative venture. A recent example of this general perspective is evident in Hampshire, Iqbal, Blell and Simpson (2014), arguing for an emphasis on the interactive joint accomplishment of the encounter between the interviewee and the interviewer, a consequent blurring of identities between the ‘self’ and ‘the other’, and the co-production of understanding. The interview is characterised as an ‘extraordinary encounter’ that is celebrated in terms of its ‘authenticity’ (Smith, Staples and Rapport 2015).

But some caution must be exercised. We still have too few empirical studies that examine the nature of interviews themselves. We do have a substantial body of methodological examinations of the interview as an encounter, and the nature of interview talk: see for

instance Rapley (2001); Baker (2004); Potter and Hepburn (2012). But here we address a rather different analytic issue. That is, the stability of accounts. It is an empirical matter as to whether a respondent is creatively and collaboratively engaged in reality-construction within the specific frame of any given interview. There are, after all, other possibilities. Any researcher who has undertaken interviews is potentially aware that accounts – especially accounts of past events – may give the appearance of well-rehearsed stories. They may be elicited in the interview, but are not exactly improvised in that specific context. After all, in a non-technical sense, we are probably all aware of the kind of stories that we and others can trot out when the opportunity presents itself. Of course, those narratives may be subject to minor emendations: over time they may gain a little in the telling, or they may be edited down (cutting a long story short). But the fact remains that they are not necessarily co-produced with the interviewer (or any other interlocutor). The teller of such stories is not self-evidently negotiating her or his construction of reality with the interviewer. Many of us have a repertoire of personal stories that we can reproduce without any great intervention from another, save for their inviting (or at least permitting) the telling, and providing an audience. On occasions the production of such accounts may be seen as evidence of inauthentic speech by an informant. This is especially the case when informants are probably used to telling ‘the same’ story for multiple audiences: it is in the nature of the ‘interview society’ (Atkinson and Silverman 1997). Comfort’s intellectual biography of the geneticist Barbara McClintock strongly suggests that she had well-established narratives of her life and work, that constituted a sort of personal mythology (Comfort 2001), and they became incorporated into the public mythology surrounding her.

This is not to imply that there may be no interpretative work involved, or that no interaction takes place during the interview encounter. Apart from any other consideration, the respondent must establish the appropriateness of the story in response to a given elicitation. In retrieving a given story, the respondent is – to that extent – an ‘active’ participant. (Indeed, it is hard to see how any response could take place in the absence of some degree of active engagement.) Our mundane observations on narrative stability may, however, serve as a corrective to over-enthusiastic endorsements of ‘active’ or ‘postmodern’ perspectives on interviews and interviewing (Holstein and Gubrium 2003). We repeat: the precise nature of interview-derived accounts is an empirical, analytic matter. It should not be based primarily on normative views of interviewing or the wider nature of research relationships (Atkinson and Delamont 2006).

These are, then, empirical matters. The interview and the data it yields are not a single phenomenon. There are undoubtedly many forms of the encounter, and many kinds of account embedded within interview accounts. Our proposal here is not to reinstate detached, impersonal interviewing as the benchmark for authenticity or validity. We question *any* prescriptive view that celebrates the interview *per se*. The point is to examine actual occurrences of interview talk in order to test the extent to which any characterisation of interviewing captures the full range of possibilities. Our own data are derived from interviews with the research group members, and it is not our intention to undermine the nature of interviewing. We need to inspect interview data not just in terms of what they might reveal about the inner selves and personal experiences of respondents, but also in terms of their *forms* and *functions*. A proper analysis of interview data will demonstrate *how* narratives and accounts are constructed (Atkinson 1997).

We examine some aspects of the Discovery Accounts and the Legacy Accounts. That is, narratives of the scientific discovery collected from the same research group members, separated by a period of fifteen years. In particular we examine the continuities between the two sets of accounts. We display features suggesting that episodes may be *sedimented* among respondents' stock of stories. We emphasise that this exercise has absolutely nothing to do with an evaluation of the respondents' veracity or accuracy of recall. Contrasts and similarities in the accounts over time tell us about the relative stability of *some* narratives. They do *not* reflect on issues of validity or reliability in our treatment of them here (though there may be other contexts in which consistency of recall is an analytic issue in its own right). Self-evidently, the two sets of interviews are not simply separated in time. There are biographical and scientific issues that arose in the intervening years. The second round of interviews certainly did not recapitulate the first set.

Methods

Both of the studies were explicitly focused on the research group's discovery of the DM gene. The first ('Discovery') study took place in the immediate aftermath of the publication of the discovery. The interviews were conducted by Claire Batchelor, who was employed as a research associate. Those interviews were conducted in the scientists' place of work, an Institute of Medical Genetics. The second ('Legacy') study took place in the context of a retrospective study, concerned with the impact and significance of the genetics. Those interviews were conducted by Cathy Sampson in the conduct of doctoral research. Members

of the original research group were by then employed in a number of different institutions, including the Institute of Medical Genetics. The interviews were conducted in the scientists' current place of work. Both sets of interviews were enacted by female researchers. Both studies were supervised by Paul Atkinson. Both sets of interviews were open-ended, exploratory in nature, guided by a check-list of topics rather than a set interview schedule.. To that extent, therefore, the two sets of interviews are comparable in terms of the characteristics of the interviewers, the purposes of the studies, the circumstances and conduct of the interviews, and the nature of the interviewing itself.

The Discovery interviews had been transcribed and analysed at the time of their initial collection, and analyses of the scientists' discovery accounts were published (Author Y et al). Sampson conducted the Legacy interviews fifteen years after the Discovery interviews. She was able to contact and interview the core members of the original research team. They were – with their given pseudonyms - the senior clinician who led the research ('Prof'), a senior scientist ('Andrew'), two scientists who had been postdocs in the research team ('Pauline' and 'Tom'), and a laboratory technician ('Kay'). All references to other actors have also been allocated pseudonyms. Consequently, we now have paired interviews with five of the scientists.

Sampson initially conducted her own research on the Legacy without reading the Discovery accounts. This was a deliberate methodological decision: we were careful to ensure that there should be no unconscious 'contamination' between the two research projects, and hence on the analysis of the interview data. Once the analysis of the Legacy interviews had been undertaken, and key papers published, we then turned to the Discovery accounts in order to make systematic comparisons between them. Sampson conducted a secondary analysis of the original Discovery accounts. The secondary analysis began with preliminary readings of the transcripts, and the interviews were then coded using Atlas/ti software to manage the data. These codings were conducted independently of the original researchers, and constitute a genuinely secondary analysis. Although Atkinson was involved in both projects – as Principal Investigator on the first project, and doctoral supervisor on the second - he did not intervene in the secondary analysis, beyond making the transcribed interviews available for the exercise. In this paper we are not concerned primarily with the thematically coded content of the paired interviews. Rather, our purpose here is methodological. As we have indicated, the convergent nature of the two research projects, and the conduct of interviews with

members of the same research team gives us the unusual opportunity to make direct comparisons, across time, of the narratives. Since our interest here is not in the fine-grained discursive level of the interview talk, the extracts have been lightly edited in the interests of readability.

Our argument, illustrated with extracts from the two data-sets, is straightforward. Embedded in the accounts produced by some of our informants there are episodes that are narrated in almost exactly the same terms despite the passage of time between the two studies. The accounts display what we are referring to as narrative stability. This in turn illustrates a more general point: that it is necessary to examine data derived from interviews in terms of their form and function, in addition to their referential content. These examples of stability across time are suggestive of broader issues concerning methodological reflections on the interview in qualitative research.

Narrative stability

We continue our discussion through two egregiously transparent examples. In each case we present the ‘discovery’ account followed by the ‘legacy’ account for each informant. Obviously we are not claiming that each interview pair is identical in all respects from start to finish. But there are embedded narrative ‘nuggets’ that are noticeably similar. The first is taken from the interviews with the research-team leader, a senior clinician-researcher (‘Prof’). We may call this the ‘stroke of luck’ story. As will be seen, it is embedded in a lengthy response in which the clinician-researcher undertakes to give a chronological account of aspects of the discovery process. The long account is prompted by the following question:

I: So could the gene have been found earlier?

And the segment that we focus on was produced after the following prompt:

I: You mentioned in the previous summer the work on 59a and in the last year...

This refers to a particular chromosome region that the research team included in their search for the Myotonic Dystrophy gene.

Discovery account

P. It's very difficult, the whole thing forms a continuum and I'm the only person around who's seen it from the beginning. It might be worth giving you a bit of a chronology, from the beginning, as I see it and see how it compares with the papers because it's quite a remarkable chronology and quite a bit of history too. How it started, the first publication of relevance, I think it was 1955 was Moore's

study. There he was looking at blood groups, and he found linkage between two blood groups, this was the first linkage more or less, and he actually studied a series of diseases with samples from Copenhagen and various studies, and one of them was a study of Myotonic Dystrophy which had been done in 1948 and he had some samples from that and he looked at the blood groups...and he found a hint of linkage. Now that sat there, and nothing further was done until about 69 to 70 and that was when I was working in America and several studies then, I took this up and so did Roger ...who had picked up Moore's original hint of linkage and was explaining it further, with some Glasgow and London families so the upshot was of that was that in 1971 to 72 both studies showed that indeed the original hint of linkage was correct and there was linkage between myotonic dystrophy and these two blood groups, so that was the first example of linkage in man and the only trouble there was linking it with a chromosome because they didn't know which. So it then stayed put for the best part of another ten years until I think it was 1982, now this was how the collaboration got going. We'd been working on Duchenne and we'd collaborated with the ... group in London our groups had found linkage and ... the X chromosome with Duchenne, and so we thought why not do it with an autosomal disease. Obviously my interest with Myotonic, that seemed a really good chance and also seemed a good opportunity to get something set up at this end. Just at that moment we had a stroke of luck. This American oil millionaire with Myotonic Dystrophy sent a colleague of his who was good scientist around to find out who was working on Myotonic Dystrophy because he'd like to fund some research. I got a letter and got together with John Richardson and thought 'Let's put in a proposal to try and localise the DM gene'. The trouble was we didn't know what chromosome it was on, we thought it might have been chromosome 4 but it wasn't very certain and so we wrote this proposal, we were just about to send it out and then we went to the International Human Genetics meeting in Israel. Somebody presented evidence that another marker was on chromosome 19. The significance of this marker had in the meantime also been linked up with Myotonic Dystrophy. So that put it was quite amusing because what happened was we rushed back crossed out chromosome 4 on the grant application and put 19 instead just in time, rescued it from the post and sent it off and they funded it. They regarded it as a speculative one, I gather. But that meant that we could get our lab set up here, it funded

Andrew's post although that was taken over by the university, and gave us our molecular group here got off the ground. So that was 1982 to 1983, so now we knew what chromosome it was on, so then it became possible to use molecular techniques like libraries of sequences on chromosome 19 and looking at other genes on chromosome 19 and that's how things got going. Andrew produced probes from his library which showed linkage, and then there was the Ap2 probe which was the first close linkage and from then on it got closer and closer, so that lead gradually into the phase when they could use physical mapping techniques and all the other things, because during this time techniques had been improving so every year that passed, not only was the linkage closer but there were techniques of handling larger blocks of DNA which didn't exist before and it was during that time also the Dutch groups started getting involved, that was about 1986 or so, and the Canadian group as well. The Duke group had been involved for a very long time with all kinds of things and they were also approached by this American oil millionaire. They were actually funded to do a completely different project, when they heard what we were doing they jumped on the same thing, I think its fair to say but that was how things lead into it. So that shows you how the chronology evolved from what you might call the classical into the specifically molecular era. And of course all along we had been in the situation of having exceptionally good families which served as a foundation, together with the molecular.

Legacy account

I: So then you came over to Wales. And did you actively set up something here then on the basis of what you'd learned?

P; Yes. Well, I finished my Myotonic Dystrophy thesis, delivered it to – no, collected it from the binders because it had to be sent to Oxford as an Oxford thesis. Collected it just the day before flying back and wrote a couple of papers. And so – and hadn't really had – there were quite a lot more things which I wrote then when I got back. But my initial feeling was that when I arrived in Cardiff with the brief to set up medical genetics, I thought, well, this isn't going to be common enough really to be able to much useful continuing research in Britain.

And it wasn't really until I actually met [child with DM] who had just been diagnosed at the age of about 18 months or 2 by [paediatric neurologist]. I had been asked to give a talk on Myotonic Dystrophy and I'd shown one or two pictures of children with it. And she came up to me afterwards and said, "I'm sure I have a patient with this who I've labelled as cerebral palsy." And sure enough, he and the family had Myotonic Dystrophy. So that set me thinking, 'Well this perhaps isn't that rare'.

And the first thing I did really after that was to extend the childhood Myotonic Dystrophy survey. I wrote round to all paediatricians in Britain, basically asking, "Have you ever seen a patient with this?" And then ended up again going round the whole country, as far north as Aberdeen, visiting these families at home. And I ended up with a series of – I think it was seventy something, congenital patients. Which really again provided a big database. Because quite apart from the children of course there was all the families. And so then I wrote that up, two papers in (inaudible) of disease in childhood on congenital or childhood Myotonic Dystrophy in Britain.

[Extract omitted]

Until – I suppose it was around 1980 or so when the DNA polymorphisms came in. And this was when a huge change started which we were right in at the beginning of. Because John Richardson in London had started working on mapping genes with DNA markers. And he started off with cystic fibrosis and – which we weren't involved in. And then he heard of me I think because of our very large Huntington's [Disease] families. But by that stage the Boston people were already working on Huntington's so we didn't pursue that. What we started off on was Duchenne. Because there were markers on the x chromosome, the very first ones. And I went up and spent quite a lot of time up learning the techniques up in London. And then we put things together with us providing the families and the linkage analysis.

And then initially the DNA analysis was done the London end and then moved down to Cardiff. And we mapped the Duchenne gene on the x and it was at that point that John Richardson was very keen to look at something on a different chromosome. And as I said the first thought was Huntington's but the Boston

people were already underway with that. And so we thought well best collaborate with them, not try and do it ourselves. And then I thought, well, why not Myotonic Dystrophy? And the trouble was at the time we started, we didn't know which chromosome it was on.

And then we had our stroke of luck which I think you will know about. Which was this very nice guy called [...], who's an oil millionaire in Denver, developed Myotonic Dystrophy and had several – not congenital, but quite badly affected – children. All kinds of behavioural and other problems who'd been misdiagnosed as – I'm not quite sure what. And eventually they found these children all had Myotonic Dystrophy. And I think it was only then they realised he had. And they were very devastated by this. And he asked a colleague in Denver, "Why don't you try and find people who are working on this" – because he was a very wealthy person, "and fund some projects."

And by then I had, I think – I had just written the first edition of my Myotonic Dystrophy book. And so I was one of the people written to and that was more or less the time we were just starting thinking of applying DNA markers. So then, like I said, the real problem was we didn't know which chromosome things were on?. And that was a big problem because with Duchenne you knew it was on the x so you just had one chromosome and you could look at markers on that chromosome. Well, if you didn't know which of all the other chromosome it was on it was absolutely needle in a haystack and it didn't sound terribly strong.

So we put in this application for this Denver fund. And we put it in that we – there was some hint, and it was a terribly weak hint, that the gene might be on – I forget what chromosome, because one of the markers linked to Myotonic Dystrophy had been suggested to be on that chromosome. It wasn't terribly convincing. Anyway we put that in. Then I went to the International Human Genetics Congress which was in Israel and there a paper was presented on putting a marker, a gene, on chromosome 19. And that gene we already knew was linked to Myotonic Dystrophy. So suddenly I realised, heavens, this gene is on chromosome 19, which wasn't the one that we put in our application.

And so then I remember coming back home, rushing in and saying to the girls in the office, "Has that application gone off?" And they said, "Oh we're terribly

sorry, we've been awfully busy and people being off sick, it hasn't actually gone off." So I heaved a big sigh of relief. And I remember we got the paper out and we went over it in Tippex and we Tippexed out whatever chromosome number we'd originally put in and let it dry and typed in 19 over the top. And that was then received much stronger and then they funded it. And I gather that in the end they funded two projects. They funded one safe one and one speculative one. Ours was the speculative one.

Now there are two 'events' intertwined in this account that we wish to concentrate on. That is, the last-minute re-writing of the research proposal – crossing out one chromosome and substituting another – and the influence of the American millionaire. Now in and of itself, this story – in its two versions - is perhaps unremarkable. But it was, for this senior member of the team, a key event in the long-term development of the research project and the evolution of the research team. It is striking that in recounting the genealogy of the team's genetic discovery the senior informant reproduces the 'Millionaire Story' in such very similar terms to two different interviewers, in separate studies, separated by fifteen years, and on both occasions unprompted. There are of course some differences in detail: the millionaire's children are mentioned only in one version, for instance. It is reasonable to infer that, as leader of the research group responsible for the major discovery, this senior clinician had in fact produced this version of events on more than one occasion before and after the first 'discovery' interviews. The story also has the quality of being tellable as well as memorable, in that it deals with unforeseen events, and recounts a turning-point in the collective career of the research group and in the unfolding story of how the genetic discovery was made. What is also clear is that these 'luck' stories are embedded in a much longer account that formulates the long-term work that had gone into the research. The 'lucky' events that are recounted contrast with and complement the narrative of long collaborative work.

Our second example also relates to a very specific event in that discovery story. It derives from paired interviews with someone who was a postdoctoral scientist in the laboratory at the time of the genetic research and the consequent discovery. Recounted by one of the scientists, a postdoctoral researcher at the time of the original research, it is a 'moment of discovery' story. The informant, a female senior scientist, is describing a complex series of exchanges between her research group and others. At the point we enter into the interview, the informant

is describing relations with other researchers, and the preparation of papers for scientific journals.

Discovery Account

I. He saw your paper before?

H. They'd written it, but they changed it a little bit when they saw what we put in and also there was one thing Tom and I weren't happy about. Tom had managed to get hold of a copy of the Fragile X *Cell* paper, which wasn't out till the beginning of December, so it was out after we'd submitted our paper. And he faxed a copy through to me to look at, to help me write the myotonic paper, so it was just luck that he was able to get hold of a copy. It was accepted for publication, we only really got it a week before it was actually published, but it just meant we could be a bit more, it gave me a ... of what exactly they'd done with the Fragile X and what we could do with the myotonic. It helped to write the paper in a way. He said not to show it to anybody, and I didn't, but I said do you think I should show it to Andrew: 'Oh yes, show it to Andrew and tell him he's not to show it to anyone else'. So I did, and lo and behold he faxed a copy of it to . Don't ask me why. So that meant that we'd lost an advantage in things we could say in the paper. And Nigel [London] wouldn't have done the same to us. It was just to make Andrew because Andrew hadn't got it from Tom and he had been told, he'd been asked not to do it, and he didn't even tell me he'd done it, I found out by chance, by some comment that was made.

I. Did Andrew ever say anything about it afterwards?

H. Not really, Andrew won't discuss these things and Tom and I were saying, 'Whose side is he on, if you're meant to be a team?' Unless Nigel's given him information that Tom and I didn't know about. I know that's happened before, that Andrew's been given information and he hasn't passed it on to me because it gives him an advantage. So Tom and I were just flabbergasted at that stage, because I was so stressed out because Andrew hadn't helped do any of the lab work. It was Kay and I, and I'd been doing sixteen-hour days, seven days a week doing it. And it's no fun staying here late on your own, particularly in the winter, it's horrible. He hadn't done any of the lab work, he didn't do any for the first lot at all. Kay and I did it all and to go and do that as well. Apart from that it was

quite exciting. When I first saw the autorad, when I did that one week experiment, and got the autorad out, my hands were shaking, because I was so excited by it. And I just couldn't believe we'd seen it. If I hadn't known about Nigel's, I can't imagine how I'd feel. I would have been totally, I probably wouldn't have believed what I'd seen, that I'd made a silly mistake. I'm glad I knew that they'd done it because I would probably have been so over excited at seeing it.

Legacy Account

I: What was the day-to-day stuff like as the gene discovery came closer? What was it like in the lab?

H: Well, it was quite tense, in one sense. Certainly when we were getting towards the closing stages.... I was working ridiculous hours particularly – sixteen hours a day – and I would often phone up ... and say, before I went home, because he [Tom] was in Boston which was five or six hours behind and I'd say right I've found X,Y,Z, you know...what have you got...and so he'd do the next bit in that kind of way. Particularly at the very end...we were just like a conveyor belt, just getting the data. And that was a bit mindless in a way. I can remember – there was one odd bit and we had this feeling there might be a triplet repeat involved and ... on the morning that he [Andrew] went up to London I had developed the autorad which showed that that was the case, we'd actually got something and they [London group] had found something as well, ...I can remember showing ... just to make sure I didn't imagine it and my hands were shaking when I was showing him [junior scientist] and it was just – After all that work and all that effort, to realise you've actually got something.

The story of the autorad was a key episode in the discovery narrative. Members of the research team were visiting another laboratory. Like all the laboratories in the international network, there was an ambivalent relationship of collaboration-and-competition. Sight of an autorad at the laboratory provided a valuable indication that both research teams were working on the same lines, and were converging on the same result. The visual evidence of the autorad provided confirmatory evidence that our informants' team lacked at that time. On the basis of their sight of the autorad, they were able to return to their lab and repeat the

procedure. Their own autorad, as a result of their work back in the laboratory, was strong, concrete evidence for the gene ‘discovery’. Subsequently, the precise timing of the events and the use made of the information was contentious. Once more, the recounted events were of significance in the unfolding discovery stories. However, it is noticeable that even details, such as the informant’s shaking hand, are vividly preserved between the two interviews.

Now it would be quite wrong even to suggest that these two pairs of interview extracts are identical. It is, however, noticeable that, embedded in much longer narratives are very similar accounts across a considerable time gap. They seem to be what we might call *narrative kernels*: episodes that are sedimented in narrative terms. Indeed, reflection would lead one to suggest that many episodes, especially those that have particular biographical significance, would be of this sort. They represent extended versions of narrative formulae that a teller can produce on multiple occasions. It is worth noting that the ‘Discovery’ accounts were collected shortly after the original discovery itself was published, and in some cases had already taken narrative shape in the short intervening period.

Scientists’ discovery accounts hold a particular place in the methodological canon on accounts and accounting devices. Gilbert and Mulkay (1984) analysed scientists’ narratives in terms of their characteristic accounting devices. They identified two registers that co-exist: the empiricist repertoire (that expresses the impersonal inexorability of scientific progress) and the contingent repertoire (that expresses circumstances, luck and personal characteristics). Those two repertoires are reconciled on the basis of ‘the truth will out’ device. The personal and the empiricist are also reconciled through appeals to hard work and being in the right place at the right time. The narrative segments that we focus on here can be characterised in similar terms, and can be glossed in terms of ‘as chance would have it’, or, as we have suggested here, ‘stroke of luck’ accounts. They are components of many narratives of success: performers’ accounts of their successful careers often contain graphic accounts of ‘luck’ or a ‘lucky break’ that is embedded in a narrative context of hard work and dogged perseverance (Sampson and Atkinson 2011). They are exemplars of a genre of ‘serendipity’ accounting. They are to be found embedded in accounts of hard work and perseverance.

As here, those accounts, those narrative fragments, can take on a discursive life of their own, that can be embedded in wider autobiographical accounts. To that extent, they resemble ‘formula’ stories. Those are identified by Loseke (2012) as culturally shared formats that are

available to speakers in the course of personal interviews. They embody culturally recognisable and appropriate versions of events, motives and feelings. In the case of narrative stability, we have episodes that are the personal equivalent of formula stories – components of a narrative idiolect. Discrete episodes, articulated as graphic stories, can thus be deployed in longer autobiographical accounts. We can surmise (in the absence of multiple interviews with the same informants) that such episodes become well-rehearsed, and can be produced, in similar forms, on multiple occasions of telling. These episodic narratives, we suggest, can be thought of in terms of sedimenting experience into narrative form.

Discussion

We return to our main analytic point: the stability of some accounts over time. The fact that *some* narrative episodes display such stability should alert us to the danger of over-emphasising the uniqueness and spontaneity of interviews in general. Some aspects may be, some clearly are not. The general nature and purpose of interviewing has been a topic of methodological debate and controversy (Hammersley 2017). While the extreme ‘radical critique’ might seem to preclude the use of interviewing altogether, that is not a necessary consequence. Rather, we need to be attentive, methodologically speaking, to a number of issues.

We are not prevented from examining interview accounts. And we do not need to ditch notions of interviews’ referential value. Interview accounts are obviously *about* something. They normally report what the speaker holds to be the case. They are also rhetorical enactments that construct the events they report. They reflect the individual memories of the speaker, but they draw on the collective tropes that are shared among a given speech community. Hence we return to our earlier argument in this paper. We have not looked at the data-sets in order to check the veracity of our informants’ accounts concerning their scientific work. The extent to which accounts display similarity over time is by no means a measure of their veracity. It certainly gives no clue as to the strength of any ‘underlying’ sentiments. Equally, of course, differences between accounts over time are not evidence of dissimulation. We should not expect all accounts to remain unchanged. The context in which the interview is granted, and the discursive context within which any particular episode or person is reported, as well as changing priorities over time, all contribute to such variation. The stability of accounts reflect the fact that many informants have well-established, repeated versions of events. This may be particularly the case when the topic in question represents a

significant turning-point in the reported biography. Everyday social actors are likely to tell such events repeatedly. And in the course of repeated telling, the account becomes crystallised. Such accounts, therefore, may be more-or-less fixed episodes that are inserted (as it were) in the occasioned enactment of the interview encounter. They may be analogous to the stock-in-trade of the oral poet, or indeed of the professional comic. Each has a hoard of episodes, anecdotes, characters and epithets that remain relatively unchanged even when composition is apparently improvised.

Consequently, accounts derived from interviews challenge us to engage in sustained narrative analysis (cf. Riessman 2008). We need to pay close attention to *what* stories, narratives or accounts are told, and *how* they are accomplished. Here we have identified just one distinctive aspect of interview accounts. We suggest that the stable, sedimented narrative episodes we have described have a distinctive character. In the course of the scientists' biographical work, these episodes stand out. They describe key turning-points in the unfolding stories of scientific work. As stories of luck and serendipity, they perform accounting work. They are examples of the contingent repertoire of scientific accounting, that express the personal – even affective- aspects of discovery, while being embedded in broader accounts of the unfolding discovery process. They are, in narrative terms, turning-points. They are analogous to the 'epiphanies' of life-history accounts (cf Denzin 1989). As we have suggested, stability is one noticeable feature of such narrative episodes. It is functional, in that such vivid episodes of serendipity can be deployed on multiple tellings of the events. They sediment the memorability and newsworthiness of the reported events, while constructing them *as* events in the past – recent or more distant. These issues reflect wider topics in life-history interviewing and oral testimony (cf Portelli 1991). Sedimented narrative episodes provide resources for the enactment of memory, and they inscribe what may be counted as 'memorable' Here memory is regarded as a narrative accomplishment, not as a mental process. Testimony is not evaluated in terms of its veracity, but in terms of its discursive formation of recounted experience (Plummer 2001).

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Notes on contributors

Paul Atkinson is Emeritus Professor of Sociology in the School of Social Sciences, Cardiff University. His research interests are ethnographic research methods, the sociology of opera and music, and the ethnography of craft knowledge.

Dr Catherine Sampson is a Research Associate in the School of Social Sciences, Cardiff University, where she previously worked as a qualitative researcher at the Marie Curie Palliative Care Research Centre. Her particular areas of interest are the mental health of young children and young people, palliative care, and narrative analysis.