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What oral historians and historians of science can learn from each other

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Abstract. This paper is concerned with the use of interviews with scientists by members of two disciplinary communities: oral historians and historians of science. It examines the disparity between the way in which historians of science approach autobiographies and biographies of scientists on the one hand, and the way in which they approach interviews with scientists on the other. It also examines the tension in the work of oral historians between a long-standing ambition to record forms of past experience and more recent concerns with narrative and personal 'composure'. Drawing on extended life story interviews with scientists, recorded by National Life Stories at the British Library between 2011 and 2016, it points to two ways in which the communities might learn from each other. First, engagement with certain theoretical innovations in the discipline of oral history from the 1980s might encourage historians of science to extend their already well-developed critical analysis of written autobiography and biography to interviews with scientists. Second, the keen interest of historians of science in using interviews to reconstruct details of past events and experience might encourage oral historians to continue to value this use of oral history even after their theoretical turn.

Introduction

The fields of oral history and the history of science have not interacted in any great depth. Historians of science have used oral history in their work and reflected on that use but they have not taken much note of methodological and theoretical debates in oral history, captured in journals such as *Oral History* and *Oral History Review*.[1] Oral historians have, in the main, operated with no particular interest in the work of historians of science, including work using interviews with scientists. This paper argues that historians of science and oral historians can learn from each other. In particular, (a) methodological and theoretical developments in oral history should encourage historians of science to

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1 Charles Weiner, 'Oral history of science: a mushrooming cloud?', *Journal of American History* (1988) 75(2), pp. 548–559; Soraya de Chadarevian, 'Using interviews to write the history of science', in Thomas Söderqvist (ed.), *The Historiography of Contemporary Science and Technology*, Reading: Harwood, 1997, pp. 51–70; Ronald E. Doel, 'Oral history of American science: a forty-year review', *History of Science* (2003) 41, pp. 349–378; Lillian Hoddeson, 'The conflict of memories and documents: dilemmas and pragmatics of oral history', in Ronald E. Doel and Thomas Söderqvist (eds.), *The Historiography of Contemporary Science, Technology and Medicine*, London: Routledge, 2006, pp. 187–200.

extend their own nuanced critical analysis of science autobiography and biography to interviews with scientists, and (b) the use of interviews by historians of science should inspire oral historians as they begin to re-emphasize the extent to which interview narratives – though subject to multiple personal and discursive influences – capture aspects of past presence, action, occurrence and experience.

Interviews in the history of science

Historians of science have a very well-developed, highly sophisticated literature dealing with science biography and autobiography. In this literature, the analyses are very attentive to ways in which writers of science biography (and makers of science biopics) draw, more or less knowingly, on a range of narrative forms and conventions. Mott Greene, for example, argues that in addition to the 'genre convention' of biographical 'consistency', writers of science biography tend to favour subjects in which it is possible to include a period of difficulty ('stories of blind alleys of investigation, failed experiments, inexplicable results … criticism by superiors or colleagues') and the involvement of a 'magical agent' in the path to eventual success: 'in a scientific biography, this agent … might be something discovered in a book, or an object discovered in the world, in the course of an exploration or experiment. The object may be given to the hero, or found by chance'.[2] Others writing, or writing about, science biography have engaged in detailed debates on the extent to which biographical writing should emphasize individual agency (psychological or existential) or instead emphasize the way in which individual lives expose social, economic and cultural contexts in which science is practised.[3] Work on science autobiography is equally nuanced, illuminating what Dorinda Outram calls 'pressures' acting on the writing, including prevailing models of the self and familiar narrative forms.[4] In the case of European biologists, Pnina Abir-Am concentrates on the way in which their autobiographies draw on fairy-tale narratives:

> The autobiographies of [Francis] Crick, [François] Jacob, and [Rita] Levi-Montalcini reflect the European model of a strong Cinderella who depends on mentorship and patronage in illustrious institutions. All three recall vividly their formative experiences as scientific *misérables*, late and modest starters … eventually rescued from topical and institutional dead ends by the action

2 Mott T. Greene, 'Writing scientific biography', *Journal of the History of Biology* (2007) 40(4), pp. 727–758, 742.

3 Mary Terrall, 'Biography as cultural history of science', *Isis* (2006) 97(2), pp. 306–313, 308; Mary Jo Nye, 'Scientific biography: history of science by another means?', *Isis* (2006) 97(2), pp. 322–329, 323–324; Geoffrey Cantor, 'Boyling over: a commentary on the preceding papers', *BJHS* (1993) 32(3), pp. 315–324, 316; Michael Shortland and Richard Yeo (eds.), *Telling Lives in Science: Essays on Scientific Biography*, Cambridge: Cambridge University Press, 1996, p. 13; Thomas Söderqvist, 'Existential projects and existential choice in science: science biography as an edifying genre', in Shortland and Yeo, op. cit., pp. 47–51.

4 Dorinda Outram, 'Lifepaths: autobiography, science and the French Revolution', in Shortland and Yeo, op. cit. (3), pp. 85–102; Michael Shermer, 'Darwin, Freud, and the myth of the hero in science', *Science Communication* (1990) 11(3), pp. 280–301; Erica L. Fraser, 'Masculinity in the personal narratives of Soviet nuclear physicists', *Aspasia* (2014) 8, pp. 45–63; Dominique Pestre, 'Narratives, imaginaries, anecdotes, and the moral of the story: on three physicists' autobiographies', *Isis* (1996) 87(4), pp. 695–700.

> of mentors-turned-colleagues … the protégés … invariably though vaguely attribute their unexpected 'luck' to [these] benevolent personalities.[5]

Historians of science have not extended this critical analysis to interviews with scientists. Instead, there is an expectation that in talking to an interviewer, scientists provide rather straightforward access to their lives and experiences. Charles Weiner states, for example, that 'oral history interviews can tell us how scientists got that way … interviewers can probe scientists' family backgrounds; the origins of their interest in their subjects' and they can 'document how scientists actually spend their time'.[6] He goes on,

> We can also learn how they practise their craft, how, why, for whom and for what rewards they work, and how they feel about it. In short, oral history can help us learn more about what it means to 'do science' than is revealed in public or private written record.[7]

Spencer Weart and David DeVorkin write of the American Institute of Physics's interviews with astronomers and astrophysicists that 'the personal feelings and philosophies, the educational experiences, and the attitudes towards the public of people who have been restructuring modern astronomy and cosmology are usually readily accessible in interviews', and they imply that accounts of careers are relatively unproblematic: 'a person might not remember why he wrote a particular term in an equation, but he rarely forgot how he got his first job'.[8] And Nicholas Russell sees oral-history interviews or 'directed autobiographies' as ways of getting to 'the scientific inner life' and 'insight into the "behind the scenes" processes of science'.[9]

This somewhat direct or straightforward conception of interviews is found elsewhere in the history and sociology of science. Steven Shapin regards his 'interviews and conversations' with scientists as a way of reaching 'internal' features of a certain field of recent science, distinct from 'beliefs about science circulating in the general culture' that are 'external'.[10] He writes, 'I wanted to retrieve from the frontlines of present-day technoscientific knowledge-making something of what it *feels like* to those trying to make a career, to make knowledge, and to make sense of the … institutional worlds they inhabit'.[11] The interviews, he suggests, record 'concrete realities' that can be held up to – compared with – discourse:

> Certain *stories* about the 'essential natures of science,' the 'essential nature of the scientist,' the 'essential nature' of such institutions as the university and industry are … compared to concrete

5 Pnina G. Abir-Am, 'Nobelesse oblige: lives of molecular biologists', *Isis* (1991) 82(2), pp. 326–343, 330, 343.

6 Weiner, op. cit. (1), p. 549.

7 Weiner, op. cit. (1), p. 548. Discussing a particular example of 'life history interviews with ten women' in science and engineering he writes that these 'document the women's formative years; the development of their interests in science' (p. 556).

8 Spencer R. Weart and David H. DeVorkin, 'Interviews as sources for history of modern astrophysics', *Isis* (1981) 72(3), pp. 471–477, 471, 472.

9 Nicholas Russell, 'Towards a history of biology in the twentieth century: directed autobiographies as historical sources, *BJHS* (1988) 21(1), pp. 77–89, 86, 84.

10 Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation*, London: The University of Chicago Press, 2008, pp. 17, 2.

11 Shapin, op. cit. (10), p. 17, original emphasis.

> realities and found to be problematic, even as I take these stories seriously as consequential cultural tropes. (Here, as elsewhere, the assumption that the historian must choose between 'rhetoric' and 'reality' should be rejected, while it still remains sensible and important to ask *which* stories – tropes and rhetorical specifications – hold up best when juxtaposed to the patterns of quotidian institutional life.)[12]

In another example, Andrew Pickering's model of the 'mangle of practice' depends upon the assumption that it is possible to use Peter Galison's account of the development of the bubble chamber by Donald Glaser – which is partly based on Galison's interviews with Glaser – as providing straightforward access to Glaser's experience, in particular his goals and his experience of the affordances and resistances of particles and instruments.[13] Pickering writes, 'there exists an excellent account of the history of the bubble chamber published by Peter Galison (1985) on which I can draw to establish my central points'.[14] He draws on it as raw material – an 'excellent' record of Glaser's goals and experience – to propose a theory of agency in the production of scientific knowledge:

> The most obvious source of agency in my historical narrative is human: I found it necessary to refer several times to Glaser's plans and goals in order to make sense of the story. But my frequent references to the resistances that Glaser encountered … should make it clear that he, as a human agent, was not in control of history … in his attempts to go beyond the cloud chamber: in his practice, these resistances appeared as if by *chance* – they *just happened*.[15]

There is no sense that what Pickering calls the 'temporal emergence' of resistances, 'appearing as if by chance', could have, for Glaser or for Galison, a narrative function or appeal. Like Shapin, Pickering uses (though in this case at one stage removed) interviews as if they provided a more or less direct record of experience.

Finally, Joseph Hermanowicz's work on the 'satisfaction' of physicists, based on interviews, is attentive to the ways in which three different 'social worlds' of academic science in North America (three kinds of university that he labels 'elite', 'pluralist' and 'communitarian') induce different 'general ways of constructing and narrating a career', so that the scientists in each understand who they are 'over time and with others who are also presenting versions of a socially-shared script'.[16] Nevertheless, like Shapin, Hermanowicz seems confident that interviews collect 'details of "what life is like" on the inside' of science in each of these 'social worlds', including 'the personal, undisclosed rather than publicly professed side of work'.[17] What interviewees say is taken to be direct

12 Shapin, op. cit. (10), p. 18, original emphasis.

13 Peter Galison, 'Bubble chambers and the experimental workplace', in Peter Achinstein and Owen Hannaway (eds.), *Observation, Experiment, and Hypothesis in Modern Physical Science*, Cambridge, MA: MIT Press, 1985, pp. 309–373.

14 Andrew Pickering, 'The mangle of practice: agency and emergence in the sociology of science', *American Journal of Sociology* (1993) 99(3), pp. 559–589, 568 n. 8.

15 Pickering, op. cit. (14), pp. 574–576, original emphasis.

16 Joseph C. Hermanowicz, 'Scientists and satisfaction', *Social Studies of Science* (2003) 33(1), pp. 45–73, 68.

17 Hermanowicz, op. cit. (16), pp. 47–48.

evidence of their 'experience of the career', of 'how the academic life is subjectively experienced', including 'levels of meaning they assign to work'.[18]

Interviews in oral history

I suggest that historians of science should extend their critical analysis of biographical and autobiographical texts to interviews with scientists. One way to do this would be to engage more deeply with oral history, in particular with aspects of oral-history theory. Since the 1980s oral historians – themselves learning from analysts in fields such as anthropology, cultural studies, linguistics and psychology – have developed and practised forms of analysis in which the responses of interviewees are understood as intricately formed, drawing on all kinds of resources circulating in 'culture' (from narrative forms to specific features of popular history), spoken with certain audiences in mind, and serving particular psychological needs. This approach to interviews is often reflected on by oral historians as a positive transition to a new state or even 'paradigm'.[19] Penny Summerfield, for example, writes that before this shift,

> American and British oral historians identified the events or historical phenomena in which they were interested, and sought witnesses to those things. They were preoccupied with the accuracy of the information that interviewees provided and the reliability of memory. The shift that has occurred over the decades is towards greater interest in the narratives people compose about the past and the ways in which memory is socially, culturally and psychically constructed.[20]

It is not possible to do justice here to the full range of influences involved since the 1980s in – to use Lynn Abrams's words – 'transforming oral history through theory', but two stand out.[21] One is linguist Charlotte Linde's work on transcripts of interviews about career choice, in which she shows that interviewees seek to claim forms of coherence over time, and across different parts of their lives, often relying on specific 'coherence systems'.[22] A second is cultural historian Graham Dawson's concept of 'composure' – a cultural reading of Kleinian psychoanalysis in which individuals are understood to experience psychological composure (a positive state) when they are successful in composing personal narratives that are in accordance with prevailing discourses:

> storytelling … 'composes' a subjective orientation of the self within the social relations of its world, enabling it to be imaginatively entered into and inhabited. The story that is actually told is always the one preferred amongst other possible versions, and involves a striving, not only for a formally satisfying narrative or a coherent version of events, but also for a version

18 Hermanowicz, op. cit. (16), pp. 47, 69, 48.

19 Alistair Thomson, 'Four paradigm transformations in oral history', *Oral History Review* (2007) 34(1), pp. 49–71.

20 Penny Summerfield, 'Oral history as an autobiographical practice', *Miranda* (2016) 12(3), pp. 1–12, 1.

21 Lynn Abrams, 'Transforming oral history through theory', in Paul Thompson with Joanna Bornat (eds.), *The Voice of the Past: Oral History*, 4th edn, Oxford: Oxford University Press, 2000, pp. 132–139.

22 Charlotte Linde, *Life Stories: The Creation of Coherence*, Oxford: Oxford University Press, 1993; Linde, 'Explanatory systems in oral life stories', in Dorothy Holland and Naomi Quinn (eds.), *Cultural Models in Language and Thought*, Cambridge: Cambridge University Press, 1987, pp. 343–366.

> of the self that can be lived with in relative psychic comfort – for, that is, subjective composure.[23]

Abrams observes that 'this theory of composure' is 'now widely used within oral history analysis'.[24] A view of interviews as narratives drawing on a range of discursive resources, subject to psychological and social pressures, clearly resonates with the literature on biography and autobiography in the history of science, discussed above. Indeed, Summerfield characterizes oral history's theoretical turn as the realization of 'oral history as an autobiographical practice'.[25]

In the next two sections, I show that oral-history interviews with scientists – recorded between 2011 and 2016 for An Oral History of British Science at the British Library – are readily understood as narratives possessing all of the complexity claimed by historians of science for biographies and autobiographies. I focus on two aspects of these interviews in particular: accounts of becoming and continuing as scientists, and accounts of successful scientific work. In order to stay very close to the material, it has been necessary to be very selective; I discuss extracts from a very small proportion of a collection of over one hundred interviews. However, the examples I highlight are exceptional only in that they provide especially clear examples of the features of the interview material that I wish to emphasize; they are not exceptional in having these features. In other words, the arguments I make are supported by other interviews in the collection, the majority of which can be accessed online in full.[26]

Accounts of becoming scientists

Elsewhere I have shown that scientists tend to tell stories of their childhoods that claim a correspondence between early interests and their scientific career.[27] We might say, in line with aspects of oral-history theory sketched above, that such stories offer narrative and personal coherence (career choice explained by childhood interests) and that they offer psychological composure through alignment with certain prevailing assumptions about scientists' childhoods. In some cases, interviewees state that not being able to tell such stories produces a feeling of what Summerfield calls 'discomposure'.[28] Consider the following from the interview with biochemist Charlotte Armah:

> Interviewer: How was your interest in chemistry [at school] expressed … in the sorts of things that you did when you weren't at school?
>
> Yeah, you see I wish I could say that I had a little chemistry set at home and I would tinker with it but, no, I think the only kind of chemistry I did at home was just baking really … I can't say

23 Graham Dawson, *Soldier Heroes: British Adventure, Empire and the Imagining of Masculinities*, Abingdon: Routledge, 1994, p. 22.

24 Abrams, op. cit. (21), p. 135. Abrams does briefly note criticisms of composure theory in oral history in this chapter and in her book: Lynn Abrams, *Oral History Theory*, London: Routledge, 2010, p. 100.

25 Summerfield, op. cit. (20), pp. 1–12.

26 See https://sounds.bl.uk/Oral-history/Science.

27 Paul Merchant, 'Scientists' childhoods', *Oral History* (2013) 41(1), pp. 63–72.

28 Penny Summerfield, 'Culture and composure: creating narratives of the gendered self in oral history interviews', *Cultural and Social History* (2004) 1, pp. 65–93, 69–70.

> that I had like a whole lab, you know, I built a lab in my bedroom and I would kind of nick things: bits of, you know, bleach and perfume and see what would … No, I'm afraid not … I just did other stuff that other teenage girls would do: sing into my hairbrush, watch *Top of the Pops*, make clothes, other stuff.[29]

Asked why she wishes she could tell such stories of childhood chemistry, she says,

> I just feel that it would be more, a more interesting story if from the age of four I was really interested in chemistry – that I begged my parents for a chemistry set, but it wasn't like that, you know, I feel as if it might be a better story, but it's not my story I'm afraid.[30]

She is clearly aware of the elements of the standard story and expresses feelings of some discontent that she is unable to produce them in her own case.

While Armah states that she is simply not in a position to tell the standard story of scientific career prefigured in childhood, others attempt to tell such stories even when the material for the telling isn't especially rich. For example, oceanographer David Cartwright uses the very first minutes of his interview to attempt to claim a connection between his career in oceanography and a coastal childhood, as follows:

> Interviewer: Would you start by telling me please where and when you were born?

> Yes, I was born in … 1926 … in … the Stoke Newington district of London. But I left that – I was only kept there for about two and a half years and then my parents moved down to run a hotel … in Worthing, Sussex … And that's of course not irrelevant because we were right on the seafront and I had a lot of experience of the seashore and the tide goes out quite a long way and so one gets familiar with that, that sort of behaviour.[31]

His deployment, later in the interview, of another narrative form – a version of the Cinderella narrative identified by Abir-Am in the case of scientists' autobiographies – is more successful. He explains that in 1951 he started his career in the Royal Naval Scientific Service (RNSS) in the Admiralty's Department of Naval Construction, where he felt much underused.[32] His transfer from here to Group W (Waves) – where he feels he realized himself as a scientist – was secured by the patronage of a senior scientific 'saviour', the group's head, George Deacon: 'Dr Deacon … was a saviour of many people, people who seemed to have nothing – nothing useful they could do [in RNSS]. Although under his guidance they did extremely well'.[33]

Periods of difficulty followed by rescue are strongly present, too, in Lewis Wolpert's account of becoming an embryologist. First he was saved by a friend who helped him move from soil mechanics to cell biology:

> I wasn't happy doing soil mechanics and … a friend of mine in South Africa, Wilfred Stein, he knew that … And I got a letter from him: he said I've just been reading in the paper about

29 Charlotte Armah interviewed 2014, British Library catalogue reference: C1379/107 Track 2 [25:24–26:22].

30 Charlotte Armah, C1379/107 Track 2 [26:28–26:45].

31 David Cartwright interviewed 2011, British Library catalogue reference: C1379/50 Track 1 [00:06–01:24].

32 David Cartwright, C1379/50 Track 2 [02:34–02:42].

33 David Cartwright, C1379/50 Track 3 [1:45:11–1:45:34].

> scientists looking at the mechanical properties of cells when they divide; I think that's what you should do. And he was coming to King's College in London and I went to the professor where he was going and I said, is there any chance that I could do this? He said yes, and he arranged for me to do a PhD on the mechanics of cell division.[34]

Wolpert says that the letter from Wilfred Stein was 'amazing' and 'I've thanked him many, many times for changing my life'.[35] Having entered biology in this way, Wolpert found himself, he tells us, in another difficult patch, to the extent that he might not have persevered with the work for which he is now best known in science – the 'French flag' model of pattern formation in embryology – if he had not been saved by a chance meeting in the sea:

> I was at a marine station in America called Woods Hole … and I gave a Friday night lecture on my new ideas … At the end of the lecture just no one spoke to me and nor would anyone speak to me the next day and I asked a friend of mine what's going on and they said, 'who in the hell do you think you are?' And they just hated it. But I was in the sea the next morning – you'll never guess who I bumped into: Sydney Brenner. Now Sydney was one of my heroes and is the only genius I know and I was very depressed of course about this and I told Sydney – and Sydney talks about finding me crying in the water – and he said, 'pay no attention, Lewis, we like your ideas, and just ignore them completely'. He totally saved me. He said both he and Francis Crick … like your ideas; please don't give them up. That saved me completely. And then I wrote a paper which was pretty widely quoted and it was alright.[36]

Wolpert's story is perhaps influenced by his own experience of conducting a biographical interview with Brenner, who tells a similar story from a different position: 'This idea of Lewis's was very novel, but he didn't get much interest in it. I can remember finding him in Woods Hole in the late 1960s very depressed, saying that he'd given these lectures and no one paid any attention to him.'[37] One set of events allows the two men to cover biographical ground in a way that achieves a good 'fit', in Greene's sense, with certain expected features of science biography.[38] Narrative and personal composure are afforded by the closeness of this 'fit'.

Accounts of successful scientific work

Like accounts of career beginnings, the accounts of scientific work in life-story oral histories are readily understood as narratives that draw on a range of discursive content, including those story forms and genre conventions identified by historians of science in biography and autobiography. In this section I briefly consider four accounts of scientific work in An Oral History of British Science. In the first two, the interviewees are able to achieve composure because their accounts contain what Greene identifies as standard features of good science biography: struggle and eventual success, afforded by a 'magical agent'. In the other two accounts, the shape of what happened maps more awkwardly onto this model, producing signs of discomposure.

34 Lewis Wolpert, interviewed 2015, British Library catalogue reference: C1672/06 Track 1 [21:02–21:46].
35 Lewis Wolpert, C1672/06 Track 3 [25:06–25:12].
36 Lewis Wolpert, C1672/06 Track 1 [1:04:53–1:06:04].
37 Errol C. Friedberg and Eleanor Lawrence (eds.), *Sydney Brenner: A Life in Science. As told to Lewis Wolpert*, London: BioMed Central, 2001, pp. 134–135.
38 Greene, op. cit. (2), pp. 750–751.

On the evening of 9 June 1966, John Houghton was involved in a balloon-flight trial of a remote-sensing instrument that he and others had made and which they wanted to be among those included on NASA's Nimbus 4 mission. NASA's deadline for submissions, which had to include details of a successful trial, was the following day:

> We filled it [the first of three balloons], it just got full, just about, it was just lifting the weight, it was almost ready for action when all of a sudden there was a great noise, 'swish', the whole balloon parted from its neck, and disappeared into the sky … So we thought we have three balloons, that's all. So balloon number two comes out, we do the same thing with balloon number two, get to the same point, and 'swiff', off it goes again. So then there was balloon number three, and we thought, well … there's something wrong … So we connected it up differently … filled the balloon number three, and off it went, and we got the measurements back, and we sent them off to NASA.[39]

Houghton's interpretation is that God used narrative suspense to show that he was helping with balloon three:

> The remarkable balloon flight is something which … I see as God helping me to do something … but also announcing to me that he was helping me by the way in which that particular event occurred. If that makes sense to you?
>
> Interviewer: How did he announce that he was helping you? …
>
> By the fact that we were right on the edge … the third one had to work whatever and it did and without that I … wouldn't be here today I don't suppose in the sort of way I am, and it's one of those, you know, absolutely pivotal points.[40]

Notice that Houghton says, 'if that makes sense to you'. While the faith in a form of interventionist Christian God may well be surprising for the interviewer, Houghton can be reasonably confident that the shape of the story is familiar.

The events of one evening provide Houghton with the opportunity to tell a story containing what Greene identifies as desirable elements in narratives of scientific achievement. For Mike Baillie, similar features are spread over years (the late 1960s to the mid-1980s) in an account of his work on the development of a 'long' dendrochronology for Ireland. He takes care to stress that the task was physically and practically difficult and regarded by certain senior scientists as unlikely to succeed:

> From a standing start, we wanted oaks of all periods … the target was something like 6,000 years … And, we basically went off collecting … thousands of samples that all had to be numbered and stored somewhere … It's important to realise that back in those days the, the major figures in palaeoecology in both Britain and Ireland had basically said, tree rings won't work … in Ireland.[41]

They were first urged on by the gift of oak itself:

39 John Houghton interviewed 2011, British Library catalogue reference: C1379/45 Track 4 [1:36:44–1:38:27].

40 John Houghton, C1379/45 Track 6 [17:55–18:55].

41 Mike Baillie interviewed 2012, British Library catalogue reference: C1379/85 Track 2 [47:22–53:05].

> If you're going to build a chronology, you want … trees that live for hundreds of years, you want the rings to be clear, you want the species not to miss rings, and not to duplicate rings … Put that suite together and oak is the dream timber … You almost have to look over your shoulder and say, who designed oak.[42]

What is Baillie looking over his shoulder for, if not the 'magical agent' in Greene's set of conventions? The story continues because – in spite of this initial, uncanny help – Baillie and his team were repeatedly thwarted, as they worked through the 1960s and 1970s, by the absence of oaks for certain key periods: 'what we found was that we were merely reproducing the same chronologies and the same gaps were still there'.[43] The closing of one of the gaps, in particular, affords the telling of a story that Baillie has confidence in, in part because it has worked well with other audiences.[44]

> And then luck intervened … The story is written up and has been repeated often … I was going to a conference in Durham around 1981 and … I was sitting on a train looking out the window … when we suddenly passed several fields, and sitting in one of the fields was a large heap of bog oaks. And I consciously thought, well no one will ever find those, because they were in the middle of absolutely nowhere. And sometimes it's a mistake to challenge the universe with thoughts like that, because, a few seconds later we passed a large blue signpost which said, 'A689 A1(M)', which of course is the best grid reference you could possibly have.[45]

Using the reference he found the site with 'this heap of beautiful bog oaks, long-lived, regularly grown', closing the gap (the others were closed by 1986).[46] Here, at this beautiful heap, comes the end to a period of scientific work that lends itself to narrative containing a strong mix of elements of desirable science biography, very readily delivered by the speaker.

If we can understand Houghton and Baillie's accounts of successful scientific work as affording psychological composure due to the way in which these accounts match up with what Greene calls the 'genre conventions' of scientific biography, then we might expect to detect discomposure in interviews with scientists whose accounts do not quite contain what is required (their stories do not, in Greene's terms, 'fit').[47] There is some evidence that, in An Oral History of British Science, this is what we find. Consider, for example, Nicholas Humphrey's account of the way in which his discovery, in the late 1960s, of 'blindsight' in monkeys was aided by the presence,[48] in the University of Cambridge's Psychological Laboratory, of an especially helpful monkey (named Helen). He begins,

> I think I was just incredibly lucky to meet this one monkey. The other monkeys … I did do some of the work with them, but they were stroppy, difficult, aggressive. Helen was like a Buddha,

42 Mike Baillie, C1379/85 Track 2 [55:17–56:32].
43 Mike Baillie, C1379/85 Track 4 [17:51–17:59].
44 Mike Baillie, *A Slice through Time: Dendrochronology and Precision Dating*, London: Batsford, 1995.
45 Mike Baillie, C1379/85 Track 4 [20:50–22:01].
46 Mike Baillie, C1379/85 Track 4 [22:25–22:31].
47 Greene, op. cit. (2), pp. 750–751.
48 Blindsight is a form of visual competence that persists after damage to or removal of the visual striate cortex. Nicholas Humphrey, 'Vision in a monkey without striate cortex: a case study', *Perception* (1974) 3, pp. 241–255.

> she was so calm … She was incredibly diligent. She would let me test her for hours on end sometimes, she never gave up. She gave the most unbelievably reproducible results … I couldn't have done it, I think, with any other animal.[49]

He goes on to recall counterfactual reflection with senior colleague Larry Weizcranz (who went on to discover blindsight in humans):

> I talked to Larry about this, whether anyone else would have done the same work. He doesn't think it was ever likely to happen. We would have discovered that monkeys have blindsight presumably, and that humans do, but via a different route, and there wouldn't have been the same, I think, intellectual excitement about the discovery.[50]

But while other scientists can tell stories of the arrival of a magical agent only after a significant struggle, Humphrey seems to worry that, in his case, luck arrived too soon or too readily: 'But, I mean, you know, I don't take credit for the good fortune of having worked with Helen'.[51] Similarly, glaciologist Stanley Evans seems uncomfortable with the ease with which a technique for the radio echo sounding (RES) of glaciers was confirmed by the sites at which early testing took place.

> We flew down the Gilman Glacier [Ellesmere Island, Canada, in 1966]. I had a steady echo … and I saw the depth decreasing, just exactly as you would expect, as we flew down the glacier to the snout and I saw it peter out on the snout and then we just got the rocky ground below … And we didn't realise then that we happened – by going to Ellesmere Island – we must have accidentally gone to the easiest place to get the most dramatic results we could have happened to go, because it was plenty deep enough to be impressive, you know, a kilometre or so, big enough and cold enough to get – we got continuous records there. We must have been by chance in the easiest place in the world to try out radio echo sounding.[52]

Next, the airborne RES equipment was taken to Antarctica with US funding and aircraft. Speaking of its use here, Evans suggests that like the Gilman Glacier in Canada, Antarctica made it 'easy' in a way that – in the telling – makes him feel uncomfortable:

> I think all the continental ice sheet was – I don't like to say it was easy but it was [laughs]. And the ice shelf was also a relatively easy target because it's smooth and uniform … that bit warmer … a few hundred metres thick, not a few kilometres thick, and highly reflecting where you meet the seawater.[53]

If conventional, circulating narratives of science require struggle before resolution experienced as luck, then luck arriving too soon prompts a feeling, in the storyteller, of falling short. For Evans, radio echo sounding worked too well too soon, presenting some discomposure (albeit rather mild and marked by laughter) in the retelling.

The examples examined above suggest that historians of science should extend their nuanced analysis of scientific biography and autobiography to the analysis of interviews with scientists and that this would not be difficult to do. We turn next to consider why

49 Nicholas Humphrey interviewed 2016, British Library catalogue reference: C1672/21 Track 5 [00:32–01:57].

50 Nicholas Humphrey, C1672/21 Track 5 [02:07–02:26].

51 Nicholas Humphrey, C1672/21 Track 5 [14:11].

52 Stanley Evans interviewed 2011, British Library catalogue reference: C1672/51 Track 6 [36:22–50:11].

53 Stanley Evans, C1672/51 Track 8 [1:03:42–1:04:33].

they have not already done so, and what oral historians can learn from the way in which historians of science have tended to value interviews with scientists.

What oral historians can learn from historians of science

Historians of science have tended to value interviews with scientists for what they tell us about aspects of past action and experience, rather than for what they tell us about narrative and personal composure. Soraya de Chadarevian expresses this focus very vividly in her comments on her experience of an oral-history training course at the British Library:

> The … oral history course was quite explicitly directed at the reconstruction of 'life stories'. This is not the aim of 'directed' or 'research interviews' as I, for instance, pursue them. I am not so much interested in the life stories of my interviewees as in their participation and interpretation of particular events. Quite obviously, the distinction between these two uses of oral history is not clear-cut, but I do think that some kind of distinction has to be made.[54]

Indeed, in her book *Designs for Life*, which was published subsequently, de Chadarevian uses her own and other interviews with scientists not to further an understanding of how and why scientists give certain accounts of the past and not others, but instead as first-person accounts of events, people, objects and practices in the Laboratory of Molecular Biology in Cambridge over three decades. She writes that she has 'used interviews … mainly as a guide to archival work and to test interpretations' and to 'elicit information from some of the actors'.[55] When interviews are referred to in the book, it is often to support statements of occurrence, for example, 'Crick and Kendrew in particular singled out the Hardy Club as a very useful forum'.[56] Generally prevailing attitudes are also identified through interview: 'Most "professional" crystallographers considered protein crystallography … a hopeless undertaking and a "waste of time", a view that prevailed well into the 1950s'.[57] Individual points of view are also discovered: 'Max [Perutz] … didn't trust it' (a densitometer instrument), as are experiences of experimental work: 'As [John] Kendrew put it … "you could spend an immense time arriving at the final result because the machine would go wrong"'.[58] De Chadarevian is not concerned with why John Kendrew might like to tell her stories of the difficulty of running computer calculations, or of starting work on a technique viewed by others (like dendrochronology in Ireland) as a hopeless undertaking. Similarly, when he tells her that two encounters early in his career – one in the jungle with J.D. Bernal and another at Caltech with Linus Pauling – turned him immediately onto protein crystallography, she is more interested in whether this account is accurate, than in why this is the account that is given:

54 De Chadarevian, op. cit. (1), p. 60.

55 Soraya de Chadarevian, *Designs for Life: Molecular Biology after World War II*, Cambridge: Cambridge University Press, 2002, p.13.

56 De Chadarevian, op. cit. (55), p. 92.

57 De Chadarevian, op. cit. (55), p. 103.

58 De Chadarevian, op. cit. (55), pp. 123, 115.

> While there is no reason to doubt that the two encounters indeed took place, it is likely that they assumed their decisiveness only retrospectively. There is clear evidence that for several months after his return to England Kendrew seriously contemplated … the possibility of remaining with the Scientific Civil Service.[59]

De Chadarevian's focus on what oral histories reveal about past occurrence – what people did and thought and felt in the past – is shared by Ronald Doel, even while he calls for historians of science to use interview collections in new ways. Though he observes that, in the history of science generally, 'Concern with sheer documentation of what happened has been supplanted by heightened curiosity about text and memory itself',[60] his suggestions for the expanded use of archive oral-history collections are not concerned primarily with matters of 'text and memory', but rather with expanding the range of historical contexts and past experiences that interviews can be informative about:

> Interview projects typically involve dozens to hundreds of interviews … They are valuable (and sometimes unique) sources of information about the early lives of scientists, hierarchical relationships between individuals in complex bureaucratic organizations, and the traditionally invisible members of scientific communities … They provide insight into shared professional identities, patterns of funding, political and religious affiliations, and the cultures of research communities.[61]

De Chadarevian and Doel, then, say loudly and clearly that they value oral-history interviews for what they reveal about past action, thought, experience, context and so on. This is their focus – a focus which cannot be described as 'objective' rather than 'subjective', because they are interested in the personal experience of their subjects. Their focus is on past action and experience, rather than on the construction, in the present, of accounts of the past. This interest in the reconstruction or 'recovery' of past individual and social life is, of course, a long-standing interest of oral historians, and did not disappear from the profession as it was transformed by theoretical innovations from the 1980s. Oral historians have continued to value – and sometimes even stress the value of – oral history as the creation and analysis of, in Joanna Bornat's words, 'this rich resource of witness accounts about the past'.[62] Paul Thompson's interest in exploring (with Mary Chamberlain) 'genre and narrative in life stories'[63] in oral history ran alongside other work (with Daniel Bertaux) in which

> we use life stories as evidence of facts (situations, contexts, conducts) along with perceptions and evaluations … the approach … takes interviewees as informants about the various contexts which shaped their life: thus they are used as sources to reveal what happened to the interviewee, how and why it happened, what he/she felt about it, and how he/she reacted to it or

59 De Chadarevian, op. cit. (55), p. 83.

60 Doel, op. cit. (1), p. 350.

61 Doel, op. cit. (1), p. 360.

62 Joanna Bornat, 'A second take: revisiting interviews with a different purpose', *Oral History* (2003) 31(1), pp. 47–53, 47.

63 Mary Chamberlain and Paul Thompson, 'Genre and narrative in life stories', in Chamberlain and Thompson (eds.), *Narrative and Genre*, London: Routledge, 1998, pp. 1–22.

> 'proacted' to realize his/her projects. This orientation thus aims at gathering both factual and interpretative information.[64]

Many other examples could be presented. Nevertheless, it remains the case that studies concerned less with 'factual and interpretative information' and more with how interviewees' narratives are composed have enjoyed a particularly high status in oral history in recent decades. There is some evidence, though, that oral historians – including those most associated with applying and developing 'composure theory' – worry that the value of interviews as providing information about the past has been downplayed. Writing under the title 'Making the most of memories: the empirical and subjective value of oral history' in 1999, Alistair Thomson responded to perceived 'rumblings that perhaps … theoreticism has gone too far, and that the important initial motivations for oral history – to provide empirical evidence about undocumented experience … were being submerged under the weight of poststructuralist and postmodernist theories'.[65] Here he points out that his own study of Australian men who fought in the First World War, though principally valued in oral history for its analysis of interviewee composure and discomposure in relation to forms of popular memory, also has valuable 'empirical' content, useful for understanding 'how and why [particular] young Australians joined the army and went overseas to fight on the European Western Front'.[66] In the case of interviewee Fred Farrell, for example, Thomson explains that 'listening … and reading between the lines of memory' affords a view of 'factors' that, in fact, motivated enlistment, concluding that 'as historians we want empirical evidence about what happened in the past; but we also want to explore how past events have impacted upon individuals and societies, and to understand the subjective meanings of those events for participants, at the time and over the years'.[67] This is a restatement of the purchase of oral-history interviews on 'what happened in the past' and subjective experience 'at the time'.

Writing more recently, Summerfield – whose work on women in the Second World War is, like Thomson's, often cited as an exemplar for others wishing to explore composure in oral history – has argued that the power of recent oral-history theory, developed in the last few decades, should not be allowed to crowd out more long-standing aims for oral history:

> These days … oral history has become more methodologically reflective. Questions now include such issues as how interviewees construct themselves through narratives that arise in dialogue with an interviewer, and how personal experience and public histories interact in

64 Daniel Bertaux and Paul Thompson, *Pathways to Social Class: A Qualitative Approach to Social Mobility*, Oxford: Oxford University Press, 1997, p. 13. Bertaux and Thompson echo the interest of historians of science in the formation of scientists: 'Survey research on social mobility has typically treated families as black boxes, whose inputs are a handful of variables such as father's occupation … Case studies of families allow us to open those black boxes and to see what takes place inside … We can explore the relationship between early socialization and adult occupational success or failure' (p. 19).

65 Alistair Thomson, 'Making the most of memories: the empirical and subjective value of oral history', *Transactions of the Royal Historical Society* (1999) 9, pp. 291–301, 294–295.

66 Thomson, op. cit. (65), p. 296.

67 Thomson, op. cit. (65), p. 301.

> the production of memory stories … However, while this intellectual refocusing is central to any discussion of the present state of oral history, it is important not to overstate it. One approach did not displace the other; one was not wrong and the other right; they coexist alongside other approaches that have longer roots.[68]

As examples of these more deeply rooted approaches she suggests the 'collection of folklore' and other kinds of 'recovery history': 'The oral history movement demanded a reorientation of history, ending the neglect of the ordinary person by insisting that if records did not exist they would have to be created with the help of the new technology of the portable tape recorder'.[69] Like Thomson, Summerfield reinterprets her own work with a stress on what it reveals not just about the construction of personal narrative, but about features of past situation, occurrence, presence and experience. Thus she points out that her oral histories with women in the Home Guard did not only show how certain women struggled to compose themselves and their stories in relation to generally accepted popular histories, they also allowed her to 'revise the historical record'. In particular, 'oral history provides evidence that British women joined the Home Guard, and were taught to use weapons, in spite of the official ban'.[70] She includes quotations from interviews that are taken to be descriptions of past experience ('you did get quite an impact on your shoulder from them' [rifles]) and strikes a similar note to Thomson: 'The value of oral history derives both from evidence of, and information about, what happened to people in the past (which as we have seen is often not recoverable by other means), and also from the way in which interviewees remember and express their experience'.[71]

Oral historians seem to be taking another look at what their interviews capture and what kinds of analysis they afford, re-emphasizing views of, to use Summerfield's words, 'what happened to people in the past'. In doing so, they may find it useful to engage with and contribute to a field, such as the history of science, in which the use of interviews as a documentary source has a high status.

Conclusion

This paper has argued that a closer engagement between oral historians and historians of science might (a) encourage historians of science to extend their own sophisticated analyses of narrative composition, developed in work on biography and autobiography, to interviews with scientists, and (b) prompt oral historians to explore with renewed interest what Thomson calls the 'empirical value' of their interviews. Achieving (a) and (b) might be expected to result in scholarly work of new kinds. One marker of success would be an increase in the number of studies, authored by oral historians or historians of science (or a mixture) in which different ways of focusing on interviews are combined. As Summerfield suggests above, there is no reason why work concerned with forms of

68 Summerfield, op. cit. (20), pp. 1–2.
69 Summerfield, op. cit. (20), p. 2.
70 Summerfield, op. cit. (20), pp. 3–4.
71 Summerfield, op. cit. (20), p. 4.

personal and narrative composure in the act of interviewing may not also 'revise the historical record'. We might look forward to, for example, studies of science as a particular kind of work – conducted in relation to what Jon Agar has called wider 'working worlds' – in which oral history contributes both to recovering details of that work and at the same time to exploring the role of stories about work in processes of personal and narrative composure.[72]

72 Jon Agar, *Science in the Twentieth Century and Beyond*, Cambridge: Polity Press, 2012, pp. 3–5.