

Frederic Bartlett and the Idea of an Historical Psychology

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George Humphrey has described the 'welcome' he received when he took up the first chair in psychology at Oxford 1946.

[The] new professor ... found himself the jetsam of an acrimonious debate as to whether he should exist at all. He heard the lions on the shore sniffing round him, waiting for him to make a wrong move. (Humphrey, 1953, p. 382)

The chair was established to support the new Honours School of Psychology, Philosophy and Physiology (PPP). Yet the main problem was the philosophers. And there were so many of them! As Humphrey put it, Oxford had more philosophers "to the square mile than anywhere else, perhaps with the exception of the Vatican" (1953, p. 382).

R.G. Collingwood was one of the philosophers at Oxford who did not completely dismiss psychology as an impossible science. In fact, as Oxford philosophers go, Collingwood kept himself remarkably well informed about new developments in psychology, including psychoanalysis. He even chaired a committee of the sub-faculty of philosophy in 1928 encouraging the establishment of a psychological laboratory (Connelly & Costall, 2000).

Nevertheless, Collingwood seemed to set prohibitive limits on what the new psychology could properly take on. According to Collingwood, it should restrict itself to the realm of sensation and the irrational, and so, in effect, exclude both history and values. However, as James Connelly and I have pointed out, Collingwood seems also to

have recognized an alternative viable option for psychology (Connelly & Costall, 2000). He set out this alternative in the context of a critique in which he warned psychologists against mistaking the historically situated for human universals. Referring back to what he described as David Hume's "'science of human nature' ... with its strictly empirical methods," he argued that Hume had been mistaken in assuming "that human minds had everywhere and at all times worked like those of eighteenth-century Europeans."

However, Collingwood raised no objection to such an empirical study, as long as it was recognized for what it really was, a study of human nature *in situ* - in Hume's case, "*an historical study of the contemporary European mind*" (Collingwood, 1939, pp. 115–116, emphasis added). Yet, Collingwood never seriously explored the idea of an historical psychology in a positive way, that is, as an alternative direction in which psychology might develop into a *science*, along the lines, say, of Wundt's cultural psychology, or Vygotsky's socio-historical approach to human development.¹

At the time Collingwood was writing, Vygotsky's main work had not been translated into English, and Wundt's international reputation had suffered badly from his emphatic support of the German cause during the First World War. There was, however, an example of

¹ In contrast to Collingwood, Stephen Toulmin has been an enthusiastic proponent of the idea of an historical psychology as developed in the work of Wundt and also Vygotsky (e.g. Toulmin, 1987).

an attempt at an historical approach to psychology that was available much closer to home, though not at Oxford, but at Cambridge.²

When Frederic Bartlett (1886-1969) began to study at Cambridge, shortly before the First World War, the Cambridge psychologists had established close links with anthropology. Several of them had taken part in the anthropological expedition to the Torres Straits in 1898 (Costall, 1999), and two of the main figures, W.H.R. Rivers and C.S. Myers, led a kind of double life as both psychologists and anthropologists. Bartlett's own early research was concerned with what he called "conventionalisation", the way material from one culture is assimilated into the existing schemas of another culture. This interest in culture contact stemmed in part from the commitment within British anthropology to the theory of cultural diffusion. But the effects of culture contact were also an urgent political issue:

Among English anthropologists the term 'culture contact' has been chiefly restricted to the description of the changes caused in native societies by the impact of European civilization. The depopulation of the Pacific, consequent on European

occupation, stimulated field work on the problem of contact ... (Richards, 1939, pp. 290-291)

For many years, Bartlett, like Wundt before him, has mainly been remembered as an experimental psychologist. In more recent years, there has been increasing recognition of Bartlett's project for a socio-historical psychology. Most of the focus has been put upon his still widely cited book, *Remembering*, published in 1932. Yet, by the time this book was written, Bartlett was, as I have argued elsewhere, already on the retreat, opting for a safer *scientific* image of psychology with which to promote his new department. As he himself explained, when he had published *Remembering* he had already moved beyond what he called his "conventionalisation" phase (Bartlett 1958, p. 144). The First World War had interrupted the sequence of his thought, and he then came across Head's individualistic notion of the schema which led him to place a new emphasis upon "constructive imagination and thinking" (p. 148). Interestingly, the subtitle of *Remembering* is *A study in experimental and social psychology*, but the book is hardly a synthesis. Bartlett's treatment of experimental and social psychology is split into two separate halves, an early sign of Bartlett's dissociation of these two interests.

The revisionist accounts of Bartlett, as an early champion of an historical psychology disagree about his eventual commitment to that ideal. The anthropologist, Mary Douglas (1987, p. 81) claimed that Bartlett simply dropped the idea, and she portrayed Bartlett as a victim of his institutional setting. Yet, the institutional setting of early Cambridge psychology was not pre-ordained, and Bartlett, as an astute academic politician, must have played some part in the transfer of his department from the faculty of Moral

² Frederic Bartlett became the first professor of psychology at Cambridge in 1931, and eventually the most powerful figure within British psychology. As such, he was in the historically unique position of being able to fix the professorial appointments throughout Britain for many years, and, in this way, also engaged in another form of promotion, that of a kind of mindless, introverted, experimental psychology that persists to the present-day. In fact, this kind of psychology has received a new lease of life where the same old experiments are being conducted all over again, but now connected to impressively expensive brain-imaging equipment.

Sciences to Natural Sciences. In any case, he seems to have been pleased with the outcome. As he recalled, “the Psychological Laboratory was ... placed where it properly belonged, with *Physiology, Biochemistry and Pathology* . . . Very nearly all the subsequent changes have grown out of this one.” (Bartlett, 1937, p. 108; emphasis added).

In contrast, Derek Edwards and David Middleton (1987) portray Bartlett as a hero-figure resolutely championing a socio-historical approach to psychology throughout his career.

In fact, Bartlett’s eventual attitude to socio-historical psychology could best be described as NIMD-ism: let’s have a radical historical psychology, but Not In My Department. He never appointed any social psychologists to his department. He even discouraged social approaches in relation to applied research. David Duncan, who was a member along with Bartlett of the Scientific Advisory Committee of the National Institute of Industrial Psychology, has described Bartlett as a “cantankerous reactionary ... [who] definitely had the boundaries of psychology as an applied science very tightly drawn, *for others at any rate*” (Duncan, 1999; emphasis added). Duncan’s qualification - “for others at any rate” - is telling. Bartlett never forgot his commitment to this alternative historical conception of psychology. As he himself put it, it remained his “leading interest” (Bartlett, 1957, p. 72).

There was a conflict between Bartlett’s efforts to present Cambridge psychology as a ‘hard’, natural science, and his “leading interest” in social psychology. Sometimes, this led to absurd situations. After the Second World War, Bartlett obtained research money from the Rockefeller Foundation to help Terence Lee complete his research on people’s sense of urban neighbourhood. The

research was related to the Government’s post-war re-housing initiative, and Bartlett was keen on the work because of its wider policy implications. But, locally, he was pushing the line about psychology being a hard science, and Lee’s research did not exactly fit in:

. . . halfway through my Ph.D, round about 1951, Bartlett was making a big play (he spent a lot of time on university politics, where he was very influential) to shake off the image of “Moral Sciences”. . . . He called me in one day and explained, apologetically, that my particular dissertation project was a bit of an embarrassment to him in this campaign. It could hardly have been more quantitative (I was the first to adopt Discriminant Function Analysis from Physical Anthropology to Social Psychology!) but it was not experimental — it was observational. This was the main source of the problem. Anyway, he asked if I would be so kind as to seek affiliation to a more congruent Department and he made several suggestions, including Anthropology and Human Ecology. I followed up the latter and received a warm welcome. (Lee, 1995)

In fact, although Lee was no longer officially a member of Bartlett’s department, nothing changed. He continued to remain in the department with Bartlett, and successfully completed his thesis in 1954.

In the end, Bartlett came to regret the official version of psychology he had so carefully promoted, that of an essentially experimental science, and a highly introverted one at that. Beate Hermelin told me that when she was a young student and newly arrived in England, she met Bartlett at Cambridge at a particularly tedious research meeting (most likely, therefore, one held by the Experimental Psychology Society). She said she was embarrassed that she could hardly conceal her disappointment about the triviality of the research. “Oh yes,”

Bartlett, confided to her, it's all gone wrong. I wish I'd written novels instead" (Hermelin, 1994).

Bartlett set out his idea of an historical psychology in his book, *The psychology of primitive culture*, published in 1923. This book was based on lectures he had given at Bedford College, in London, with the encouragement of W.H.R. Rivers. At the very outset of his book, Bartlett takes on what he calls "the time-honoured question of the relation between sociology and psychology". He refers to Rivers' claim that the sociologist could, and indeed *should*, disregard psychological facts, and "rigorously confine his attention to purely social determination" (Bartlett 1923, p. 25). Bartlett agrees with this position as far as the sociologist is concerned, and he provides plenty of examples of group processes, such as the elaboration of rituals (see pp. 161, 206-8 & 212-3), where there is "no necessary connexion with conscious purpose, and [the result] is, in fact, rarely or never foreseen by any individual" (p. 161). But he insists that the argument does not work both ways. Psychology cannot divorce itself from sociology, for that would tempt us to do exactly what we should avoid - seek explanations which "always ... go back to the individual as he may be pictured to exist outside of any social group" (p. 8).

Bartlett purpose was to challenge the possibility of what he called a "prehistoric psychology":

... the attempt to find the beginning of social customs and institutions in purely individual experience may be essentially a mistaken one. In general terms our problem is to account for a response made by an individual to a given set of circumstances *of which the group itself may always be one*. ... It is only if we interpret individual

to mean pre-social that we can take psychology to be prehistoric. The truth is that there are some individual responses which simply do not occur outside a social group. To look for these outside such a group is to court failure, and leads inevitably to speculation and guess-work. (Bartlett, 1923, pp. 11-13.)

In his 1923 book, Bartlett envisaged social psychology not merely as a supplement to general psychology, but as an inclusive approach to human psychology that was social and historical to the core. In contrast to Collingwood, and indeed most so-called cultural psychologists, including Vygotsky, as far as Bartlett was concerned, there were hardly any aspects of human psychology beyond the reach of history.

Bartlett was promoting an approach to the individual as necessarily nested within a social order. We discover, as it were, the individual - what is individual - *within* the group (pp. 279-280). We start with those conditions that are general to all groups, then consider those processes which are specific to the group, such as particular modes and mechanisms of the transmission and maintenance of culture. Only then, Bartlett insists, may we "legitimately attempt to go further" (if indeed the group is sufficiently open to direct observation) and "try to trace what are the variations of individual attitude in regard to the specialised social responses":

If we can do this successfully for all our problems we have taken the final step in the development of a complete social psychology. For we have now effected a union between that study of the individual as such which is the concern of general psychological investigation, and that account of the psychical conditions of behaviour in the group

which social psychology sets itself to pursue. (Bartlett, 1923, p. 280)

Bartlett also challenged the standard contrast between the primitive and civilized mind. According to Bartlett, the usual contrast was based upon a misleading comparison, "not between the primitive man and the ordinary member of a modern social group, but between the former and the scientific expert at work within his own field" (Bartlett, 1932, p. 284).

So far I have tried to convey the gist of Bartlett's idea of an historical psychology. But why did Bartlett fail to pursue this project? Mary Douglas (1987, p. 81) once nicely observed that "psychologists are institutionally incapable of remembering that humans are social beings. As soon as they know it, they forget it." I now want to identify some of the issues that keep leading psychologists to forget - or in Bartlett's case, *suppress* - the idea of psychology as *history*. I will start with Bartlett's own hang-ups.

Already, in his second book, *Remembering*, Bartlett is framing his commitment to the idea of an historical psychology in a grudging way. When he explains that the group can influence the individual both directly, through the actual presence of other people, and also *indirectly*, through the influence of the group's beliefs, conventions, customs, traditions, and institutions, he seems far from happy with the implications:

... this is, theoretically speaking, rather *troublesome*, because it seems to mean that *everything* in psychology belongs to social psychology, except idiosyncrasies and such forms of reaction as are immediately and dominantly determined by physical stimuli. (Bartlett 1932, p. 243; emphases added.)

One obvious problem, given the Department had moved from Moral to

Natural Sciences, was that an historical conception of psychology hardly conformed to what his colleagues in the faculty regarded as serious science. But Bartlett also had his own methodological misgivings. Indeed, by 1957, he came to the following gloomy conclusion:

To us, as to [Rivers], there seems little chance of developing a sound social psychology unless first the problems of its methodology can be settled. But we have given up the idea that it possible to establish a method on the grounds of its rationality, or indeed any other general ground. ... [There is now] a muddle ... (Bartlett, 1957, pp. 75-6.)

Bartlett is widely celebrated within psychology for encouraging researchers to 'loosen up,' and ensure that their experiments are relevant and stay close to real life. And yet Bartlett became obsessed with the strictures of method in a way I still find puzzling. Very early in his career, Rivers had somehow managed to convince Bartlett that the method of experimental psychology was fundamental to anthropological research, even though, as Bartlett himself admitted, Rivers never actually explained to him exactly how this was supposed to work out! It seems to have been a kind of conversion experience:

I can bring back to mind very clearly the first time I met Rivers. Already he had reduced the large, somewhat military looking moustache which adorns his earlier photographs. He seemed to me tall, spare, sharp-shouldered, full of nervous though quiet energy. He sat on a sofa in a large room untidy with books and papers. A College 'gyp' brought up tea, two or three bits of bread and butter, and some very sawdusty Madeira cake. He ate nothing and drank only milk and water. He told me then, and many times later, that if I wanted to study anthropology, or, I gathered, do anything else that was any good, I must first spend many hours in the psychological

laboratory learning the psycho-physical methods. I did. (Bartlett, 1956, p. 82)

Throughout his career, Bartlett kept returning to this issue, and kept coming up with his own solutions about how experimental psychology was supposed to make its crucial contribution. He eventually settled upon the strange idea that somehow the experimental psychologists, by carefully pursuing what they, at least, regarded as decidedly *non-social* problems, would somehow, in the process of their research, perfect an appropriate method suitable for the anthropologists in the field. In other words, an historical psychology would have to wait until the appropriate methods had been devised for it by an experimental, and essentially individualistic, psychology:

One thing is so immediately obvious that it hardly needs to be said. It is rarely if ever possible to obtain results from controlled laboratory investigations which are immediately valid and important in the field of social psychology. . . . The primary problem for the laboratory psychologist interested in social psychology is, not that he should at present try to investigate admittedly important facts in any wide social setting, but that he should try to devise the most exact methods possible for an investigation of facts which have a definite social application. These methods he must then hand on to the field psychologist, who will use them as faithfully as he can, not hesitating to introduce modifications if necessary, though with as little departure as possible from whatever exactness of control the laboratory has been able to achieve. (Bartlett 1939, pp. 25-26)

There are wider reasons why psychologists shy away from taking history seriously. For those coming from a background in experimental psychology, it has always been tempting to mark out a domain of human psychology that is universal and hence,

to use Bartlett's own term, prehistoric. For them, scientific laws should be universal. It is breathtaking the way that the 'laws' they claim to discover are simply *assumed* to be universal, even when based, as they often are, on the most restricted of 'samples':

When I mention a psychological subject, I mean a subject from a Western, industrialised culture; and not only from a Western industrialised culture, but an American; and not only an American, but a college student. (Jahoda, 1970, p. 13)

However, it is also remarkable the extent to which the so-called 'cultural psychologists', despite their seemingly radical emphasis upon cultural diversity, set their own definite limits on how much people might differ despite their different socio-historical circumstances. Their concern is, presumably, that a socio-historical approach to psychology could go too far, morally and politically. It might challenge the very principle of *psychic unity*. It has become almost standard within cross-cultural psychology to insist - and assume - that it is only the *content*, not the *form*, of human mentality that can differ in different socio-historical circumstances. For example, this *a priori* distinction is maintained in the work of Michael Cole, one of the leading figures in modern cultural psychology. As John Greenwood has noted,

None of Cole's illustrative samples of cultural psychological theory and practice constitute illustrations of historically or culturally local forms of psychological functioning: they are all readily interpretable as *different cultural or historical manifestations of putatively universal forms of psychological functioning*. (Greenwood, 1999, p. 511, emphasis added; see also Tulviste, 1991)

Even Vygotsky set up a dualism between two lines of human development: the biological and cultural. Furthermore,

despite his socio-historical approach to human development, the eventual *outcome* of development is to render the person as *asocial*. According to an influential, neo-Vygotskian metaphor, adults provide the “scaffolding” that supports the child’s development. But it is, of course, in the nature of scaffolding that it will be eventually removed, thus leaving us with a self-standing construction: the autonomous individual posited by standard psychological theory.

In contrast to the ‘competition,’ therefore, Bartlett’s historical psychology would have been truly radical. It would not, as in the case of Wundt, have been restricted to the ‘higher mental functions’, nor committed to the dualisms, current within socio-historical psychology, of form and content, and biology and culture. Yet, in Bartlett’s case, he did not simply put strict limits on the scope of ‘history’ in his version of historical psychology. He suppressed the entire project over the course of his long career.

Bartlett and Collingwood and the idea of an historical psychology

Finally, I want now to return to Collingwood, and address what are, in effect, two versions of the same question:

1. *If* Bartlett had developed his historical psychology, would Collingwood have recognized it as a proper way for psychology to develop?
2. Why did Collingwood not pursue the idea he himself had floated of an historical psychology (Connelly & Costall, 2000)?

In a discussion of Wittgenstein’s objections to psychology, Goldfarb has argued that Wittgenstein’s real complaint was that psychology was *scientistic*, not that it was *scientific*. Unfortunately, by the time of Wittgenstein and Collingwood, this is a distinction that could no longer be easily made. With the rise of scientific naturalism in the nineteenth-century, ‘science’ and ‘scientific objectivity’ had both come to take on quite a new meaning (Turner, 1974; Daston, 2000). The newly professionalized science had become political, challenging the existing authorities, and claiming its own exclusive access to the truth. Science became scientistic. (It is remarkable how the standard texts with such promising titles as “What is this thing called science?” never mention this rather important historical fact.)

Furthermore, even if *among* the sciences, the scientific gains of psychology and sociology are still widely regarded as puny, these disciplines were crucial for fulfilling the claim that science – in other words, *scientists* – had the final word not only regarding how the ‘world’ really works, but also human affairs. Thus, if no more than ‘place holders,’ the creation of departments of psychology and sociology was essential, ideologically, to this whole social transformation.

Collingwood was no fool and must have recognized the strategic role of psychology in the promotion of the new scientistic science. His polemic against psychology was surely at one with Wittgenstein’s

a trenchant attempt to protect and conserve a domain of knowledge and form of understanding from erosion and distortion by the scientific spirit of the age. (Hacker, 2001, p. 42).

Now what could constitute a more profound threat to humanistic understanding than a self-consciously *historical* psychology encroaching into the domain of history? This surely is why Collingwood was so concerned to put such tight boundaries around the scope of modern, scientific, psychology. But this could also explain why Collingwood, the historian, and someone who never missed a chance to have a 'go' at psychologists whenever they overstepped those boundaries, nevertheless did not rant against David Hume's proposal for a 'science of human nature' – even though, as Collingwood insisted, it was inevitably *historical* by its very nature. In Hume's time, science had not become scientistic, and a science of psychology, the big hope of scientism, had not yet been enlisted to its cause.

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Discussion

Wes Sharrock

Could you give me an example of the kind of thing Greenwood was talking about when criticising Cole?

Alan Costall

Take categorisation. Within cognitive psychology, categorisation is seen very much as something universal and fundamental, rather than a specific and situated kind of practice. It is supposed

to be *the* way that any creature could possibly exist in the world. If we did not categorise things we would be in chaos, in a blooming buzzing confusion. So you have plenty of cross-cultural research about how different people categorize things differently, but nevertheless based on the assumption that categorization itself is a universal and fundamental cognitive function. For example, there has been extensive work on cultural differences in colour categorization. [...] The investigators travel to exotic places, come out from the bushes, or wherever, with a case full of colour samples, and on the look out for interesting cultural differences. But [they look] within strict limits. First of all, of course, they take it for granted that everyone understands what a psychology experiment is supposed to be about, and, in this instance, that everyone shares the same abstracted notion of ‘colour.’ The only issue up for grabs, empirically, as far as the researchers are concerned, is how the people being studied draw the boundaries between different colours. So for most cultural psychologists, categorization and the ‘objects’ to which it is applied are largely taken for granted.

Wes Sharrock

Sorry, so what exactly is Greenwood criticising Cole for?

Alan Costall

Well for precisely doing *that*. For not going for broke. In my example, for not considering whether the people they are studying actually share our sense of colour as an abstracted property.³ Can

³ Saunders, B.A.C. (1995). Disinterring basic color terms: A study in the mystique of See Saunders, B.A.C. (1995). Disinterring basic color terms: A study in the mystique cognitivism. *History of the Human Sciences*, 8, 19–38, & Saunders, B.A.C., & Brakel, J. van (1997). Are there nontrivial

we find people who are actually doing things in a radically different way from us, not just doing what we happen to do but *differently*, that is the criticism.

Dave Francis

I don't really know anything about Bartlett, so can you say a bit more about how Bartlett went about creating the scientific psychology at Cambridge and well, why it had to go that way? You seem to be implying that it had to go that way. Is it to do with university politics, the immaturity of a new department, where they must appear to fit in with the methods that were in accord with scientism?

Alan Costall

One of the main sources of experimental psychology was experimental physiology of senses, and the research of people like Helmholtz and even Fechner, so there was an already established model of what an experimental psychology ought to look like. Interestingly, at the beginning of Bartlett's book on remembering there is a penetrating critique of a certain notion of what experiments should look like. He talks about how the psychologist should not "stand in awe" of the "stimulus." In fact when Bartlett did any kind of research on his own behalf, it did not accord at all with the strict ideal of experiment promoted within his own department at Cambridge. Bartlett published a strange book on thinking when he retired and I think it is significant that he published it only after he had retired. It was based on interviews and asking people how they solve problems and all the rest of it. It didn't look a bit like Cambridge experimental psychology.

constraints on color categorization? *Behavioral and Brain Sciences*, 20, 167–228.

Question

So, is it like Wittgenstein when he had to get out, he had to go to an island to get his ideas together for the *Investigations* because he was just strangled by the place?

Alan Costall

It is interesting about Wittgenstein and psychology at Cambridge. Wittgenstein got involved in some of the early work on the psychology of music for example. C. S. Myers, Bartlett's 'patron' at Cambridge, was very much into that sort of stuff. Curiously, despite the later Wittgenstein's view that experimental psychology was terminally confused, I have found no signs in my work of personal tensions between the philosophers and psychologists at Cambridge that there were at Oxford.

But to get back to Bartlett. He was faced with two problems. The first was the extent to which the new psychology should be *identified* with *experimental* psychology. To a large extent this had been decided for him, when Myers established the Laboratory of Experimental Psychology. The second problem was what *form* experimental psychology should take. Kurt Danziger has emphasized the obvious but widely repressed fact that the psychology experiment is an institution. To get people to take part in the exercise they have to understand what they are letting themselves in for. Furthermore, experimental psychology has taken distinctly different forms. It is interesting with Wundt's experimental psychology that it was the subject who was actually the expert, and who typically published the paper. The experimenter was just a technician, presenting things for the expert 'subject' to respond to. There is another kind of model which Danziger describes as developing in France, in relation to hypnotism, etc., where the subjects, themselves, do not know what

is going on. Another kind of model is set up in America where you have students taking part in large groups, aware that they are contributing to science, but again they don't really know what is going on, since such knowledge is supposed to contaminate the results. The curious thing is that Bartlett opted for precisely the 'physicalist' model of experimentation that he so famously criticized in the first chapter of *Remembering*. My view is that with the move of the Department from Moral to Natural Sciences, Bartlett became trapped. Research had to be done, seen to be done, and also to look much like what his colleagues in physiology (with whom the psychologists shared the building), and the research funders, etc. thought it should look like. Bartlett was responsible for many staff without tenure who were funded by short-term research contracts.

Ivan Leudar:

There are several things. One, I am completely puzzled by the comment on Rivers, and that he was actually insisting that only through using experimental methods can you move psychology somewhere. I am puzzled because *Instinct and the Unconscious* for example is actually very interesting, and nowhere there is he using an experimental method - he examines his own practice reflectively. What he does is almost ethno-methodological: he works, he reflects on what he does, he uses his knowledge. It is very interesting how in doing this he reworks nicely the idea of repression [for him it depends on education and training rather than being something in the universal human nature] and he has got something which is very vivid. So, why would a man like that, who obviously knows what he is doing when he is doing it, why would he actually be insisting that psychology has to use experimental methods? I am just

completely puzzled - I do not understand him.

One more thing, you seem to be talking as if the history of psychology was starting in Cambridge at this point, and these people were, at this point, insisting that psychology should be an empirical discipline. It just seems to me that it can't be like that. If you take it back to what I was digging out of Collingwood (quite deliberately in a biased way) the business actually starts in 16th century. [...] So if this is right then you would have had a variety of psychologists, some of them philosophical like in Scotland, some of them actually empirical scientists like those dealing with memories and perceptions. So what account do you give of somebody like Rivers, doing something that he is doing, would Collingwood have to say something about that?

Alan Costall:

Bartlett provides several accounts of his meeting with Rivers. In an appropriately Bartlettian way, the details are not consistent. (In one version it is not even Rivers but one of his fellow researchers who puts the spell upon Bartlett.) You need to remember that Rivers initially established his reputation for his work on the physiology of the senses. One version Bartlett gives of the role of experimentation is that it is a kind of initiation. Anyone who hasn't gone through the discipline of doing experimental psychology is going to be careless. So it is partly a kind of discipline thing, stopping people being 'sloppy'. But there is also the issue of calibration which kept coming up in the context of psychophysics: becoming aware of the characteristics and limitations of the observer. So, one way or another, there is this aspect of initiation. This is how it worked out in the USA. Famous psychologists, like Thorndike, having done their initial

work in experimental psychology, put experimentation aside. This in part was for financial reasons (there simply weren't the jobs in experimental psychology). But the idea was that once they had gone through the training they were now scientifically transformed. I suppose it is like the anthropologists. As PhD students they undergo a punishing time, suffering in the ordeal of field-work. Then they come back home, get an academic position, and spend the rest of their careers taking it easy and publishing. Bartlett (along with his other main mentor, Myers) certainly made these points, but it is also the case that Bartlett also reckoned that experimentation, in some form, would be necessary as the standard method for an historical psychology.

Ivan Leudar

The other thing was really a comment expressing a small disagreement. It is on Vygotsky and Russia when he and Luria were working there. I wouldn't think that there was actually any official presupposition about the *psychological* unity of mankind then. In fact it would be the exact opposite, you would have different classes and these would be quite different mentally, some progressive and some backward, and what Vygotsky would be doing would be actually standing up for an individual. I always thought that, even though he has a theory of socialisation and internalisation, that theory actually reflects the situation in Russia at the time when people are being re-educated, sometimes quite forcefully, but even that re-education in his way is never a situation in which you become completely engulfed by and in the social. Vygotsky is always leaving a niche for the individual to resist and which actually allows one to be natural.

John Pickstone

So to continue then in your spirit, to underline the huge importance of physiology in Cambridge: I think in most places it ranks closely with medicine but at Cambridge it ranks with physics. [...] It has terrific power. [...] part of its selling power is this training thing, which is particularly important for medics [...] There must be clear parallels there.

Alan Costall

Some of the people I have interviewed have said how embarrassing they found it as psychologists even to be sharing the same building with distinguished physiologists, some of them Nobel Prize winners.

John Pickstone

What I am less clear about is the other part of the local context where a guy could go to sociology, or to human ecology which is a very peculiar formation. So if you could just say a little about that and then we will switch and I will ask about Collingwood in Oxford and what sort of local politics there were campaigning against psychology.

Alan Costall

So about Cambridge first: I have found no references to these developments in my work on Bartlett. The first chair in sociology was not established until 1970, the year after Bartlett died, and, according to its first holder, there was not much going on beforehand.⁴ I do not

⁴ "Today ... I find myself with the heavy responsibility of justifying a new chair in a discipline which in this university is still striving for full recognition. ... There are two salient features which distinguish the state of sociology in Cambridge from the situation found in many other major universities. First, the decision to establish a chair in sociology was taken only very recently; and second, there are many members of the

know about human ecology at Cambridge, but I guess this was a post-war development, so Bartlett would be close to retirement.

The Second World War changed the whole picture with all the military research that was going on. Bartlett gets shifted even more into a sort of engineering style of psychology because of the war work, and through the influence of Craik, an amazing student from Scotland who came to work with Bartlett and absolutely charmed him. Craik really gets into this kind of engineering approach to psychology but again I think Bartlett begins to see how it starts to go wrong in the hands of some of his other students. I have letters of Bartlett's where he is extremely negative about the triviality of their work based on mechanical analogies.

Going back to the early days with Bartlett, one of the things worth bearing in mind is that Bartlett gets to Cambridge just before the First World War, and very soon all of the other people go off and in some way or another get involved in the war work. So he is left pretty well on his own and in a very vulnerable position because the department really had not got a very safe base. The laboratory would not have been there in any case unless Myers, whose family were very rich, had not put up the money. It was a funny set-up. After the war, Bartlett was still very much on his own, James Ward had given up any interest in this new psychology, Rivers died just after the war, and then Myers goes away in a huff and sets up the NIIP, the National Institute of

university, junior as well as senior, who do not regard sociology as a proper academic subject." Barnes, John Arundel, 1970, *Sociology in Cambridge*. Inaugural lecture of the Professor of Sociology, 15 October, 1970. Cambridge: Cambridge University Press, p. 1.

Industrial Psychology, so I can understand why Bartlett could feel so vulnerable and so prepared to shape Cambridge psychology to what those surrounding him thought psychology at Cambridge should look like.

John Pickstone

What about Oxford, when Collingwood is making polemics against psychology, to what extent is that appropriate to the local context?

Alan Costall

His targets are not local. They are people like Freud, Lloyd Morgan at Bristol, and Spearman at London. After all, there is not much local going on. There weren't any psychologists in Oxford worth 'going for' until quite a bit later. The thing with Collingwood is that he had no initial misgivings about psychology as long as it behaves itself, and restricts itself to the limited agenda *he* set for it.

James Connelly

Can I just pick up on that? There are two, maybe three points. The first point is - I think Collingwood had a general concern with psychology and its place in explanation or understanding. When you come to the particular Oxford context I think there are at least two events which are important here. I mean one is the proposal to set up a psychological laboratory in Oxford which was earlier than the proposal to set up the course and Collingwood was involved in both because it went to the philosophers to discuss whether or not the psychology laboratory should be set up. Essentially the point was, we should look after it so that they don't overstep their boundaries [...] It wasn't that he was opposed to it so much that it should be kept in its place. And the same with any joint proposal for psychology programmes and so on. The other point worth making [is that] Collingwood went to Haldane's laboratory and participated in and

observed the experiments on the physiology of perception just before Haldane died. I presume he did this before 1936, as Haldane died in 1936. Collingwood refers to this in the lectures on realism that he wrote in that year. As far as Collingwood was concerned, this was very good, interesting experimental science which he was perfectly prepared to accept as being a valid form of knowledge. The problem is when psychology then goes and tries to explain too much or explains what he thinks it categorically cannot explain. So I think you can say very clearly that in this sense he was in favour of the science of psychology but not in favour of its improper extension.

Alan Costall

I think in the end it is the very idea of a science of psychology that he doesn't like. Because it is not only trying to open up a space of naturalistic explanations but also threatening to close down other spaces, namely humanistic understanding.

Mathieu Marion

I wanted to pick up on something that has been said, in reply to what you said. I think he never had any qualms in as much as psychology was committed to the sort of experimental stuff about perception [...] and even in the autobiography I do not think he has any qualms –

Alan Costall

- But it is that issue, about overstepping the boundary. You find him going for people, I mean some of this is very *ad hominen*. Ivan quoted this thing from our paper as it happens, Donagen says, 'every psychologist to whose notice it has been brought has been justly angered by what Collingwood wrote of psychology in his Essay on Metaphysics.' I am probably brought to this inference by thinking about

Wittgenstein's misgivings about the very idea of a psychology in the end.

Mathieu Marion

Okay let's put it that way, there is a very narrow field in which there is no problem with doing this physiological inquiry that is psychology - everything else is confusion [...]

Alan Costall

I am not an expert on Collingwood, and one thing that puzzled me when I was working with James Connelly on our paper on Collingwood, was that Collingwood identifies two issues in relation to putting limits upon the scope of a naturalistic psychology, but they do not seem to be connected together by him. The first is the normative, or criteriological, as not being properly part of psychology, but there is also the other point about the historically situated not being a proper subject for psychology. It seems to me there are plenty of examples where we can talk about people going about their business as being historically situated but which I would not count as criteriological. For example, the way people walk. Mauss talked about this, years ago, in terms of body techniques, people in different cultures walk differently but is that normative?

Mathieu Marion

In your story about Bartlett moving away from social psychology - what was his interaction with the philosophers? Because all through his lifetime the philosophers were saying extremely negative things about psychology [...] and couldn't it be the case that actually having been sort of brainwashed by all these idealist philosophers [...] that in the end he sets up next to physiology and forget about the rest [...] Could there be something like this also involved?

Alan Costall

It is interesting, but I am not aware of any kind of real interactions between Bartlett and the philosophers, and this is curious because the Tripos degree involved both philosophy and psychology. According to the accounts of people who took the course, the teaching was completely compartmentalised. You did your psychology and you did your philosophy, pretty much like a lot of so-called 'joint honours' schemes in other universities.

Alan Collins

There is a story, isn't there, about him going up to Cambridge to do philosophy and he gets to the squash, meets Russell and Moore and decides not to do philosophy.

Wes Sharrock

I was just wondering whether something like this was involved: it is a feature certainly of sociology that the people who have the schemes to make a science always assume that it is someone else's work to deliver it. They are not interested in working it out once they have laid out how it is to be, somebody else can go and do it. I was carried right back to my earliest days here when of course one of the reasons we got involved in ethnomethodology was that we were fed up with what we then called programmatic, which were the endless delivery of new manifestos with no follow through. Not a lot has changed since then. A small but nice example of the discontinuity between the top of the organisation and the underlings might have been here because as we understood in the 60's when the Manchester Psychology Department was under Cohen [then head of the department], if you worked in the department you did rats and fruit-flies with absolutely no admission of anything else. Cohen wrote books on

footballers, how they scored goals, on gambling, and so on. Yes?

Alan Costall

Yes!