

From my Diploma in Social Work course at the University of Leicester, early 1974:

Note: At that time Scope, the charity for people with cerebral palsy, was still called the Spastics Society, so the term 'spastic' was acceptable if already becoming problematic; and also I was still then following the traditional convention that 'the male pronoun includes the female'. 'Casework' was the term then widely used, especially in the USA, to describe the interaction between social worker ('caseworker') and 'client'.

Is Casework an art or a science?

It would be tedious to pretend to argue this question in the form of a debate, only to work through finally to the inevitable (as I see it) conclusion that casework is both art and science – amongst other things. This premise, rather than conclusion, might be justified and elaborated by the words of Florence Hollis:

“Intuitive insights and spontaneity...” – artistic talent? – “...are combined with continuous effort to develop and systematise knowledge and understanding of objective truths about man and his social expressions, relationships and organisations.” – scientific method?
[Roberts and Nee, ed, *Theories of Social Casework*, Chicago 1970, p38]

This sort of comment one can agree with, not only because it has a basic truth, but also, like so much else in the writers in Roberts and Nee, it is suitably vague. Up to a point this is quite justifiable, for they are writing about basic principles. Thus much of what each writer says, particularly by way of introduction, can be expected to be in abstract form while they initially enunciate their basic principles. But all these writers develop their theses by further abstraction rather than illustration, and in this manner conform to C.Wright Mills's category of social scientists whose level of thinking is in 'grand theory', and whose value is very limited because of their “...choice of a level of thinking so general that its practitioners cannot logically get down to observation”. [*The Sociological Imagination*]

This seems to be a real aspect of the Roberts and Nee writers' belief in scientific method – that elaboration consists of refinement of definition and principles rather than illustration. Nowhere in the book is an example of any substance related to theory. The result can be felt when reading Bernece Simons's “overview” of the book, as she finds many similarities as well as differences between the theories. Yet she still continues to discuss these in terms of a search for an all-embracing grand theory of casework. To my mind, this offers little to the development of casework theory at all. Yet surely they are right when they say social work theory could be scientific, so in what way can a caseworker be scientific, and where does the 'artistic talent' come in?

This issue is important to determine, because it is too easy to react against the Roberts and Nee syndrome to set up a 'use your common sense' syndrome instead, supported only by straight or coloured accounts of case histories that interest one personally. Yet what a way of opting out that is! Social workers/probation officers need the widest possible breadth of understanding and action, and perpetually need to be stretching their personalities as far as possible in order to accommodate that ideal, just because one of their basic facts of life is that every case is different. Unless one is to trust one's own intuition infallibly, all possible resources – artistic, scientific and philosophical – need to be drawn on, as far as is possible, to attempt to allow for that basic 'fact of life'. This process I consider qualitatively similar whether a caseworker is working on a particular case, or studying a general casework issue. In both instances he should be relating to the body of casework 'knowledge' – his craft – in the same way.

Apart from the lucky chance that it is a subject of which I have some prior knowledge, there are some good reasons for drawing a parallel with the study of history to illustrate the nature of scientific method, as I understand it, in casework. History has long been classified by many as an arts subject with a scientific method (I would call it a social science), and it has the further similarity that every event in history is in the last resort uniquely individual. It is in that sense that the two subjects have the same problem of organising their bodies of knowledge in a scientific way.

It is interesting to note straight away that historians generally accord little interest to the evaluation of grand theories of history: the ideas of St Augustine, Tolstoy, Toynbee, and even Marx in this respect tend to be treated as interesting diversions from the real study of history. Unfortunately this partly derives from many historians' excessive abhorrence of being thought in any way scientific – a certain snobbery keeps them from classing history as a social science. Here both subjects could learn something by looking at psychology, and the way psychologists seem to have the right idea of how to relate Freudian grand theory to their discipline as a whole. Freud's ideas are considered, but they do not govern the whole study of psychology – instead they inspire, regulate, initiate and are tested by the empirical work of those psychologists who have at least some respect for Freud. This course is now at last preferred to long discussion in abstract isolation of their absolute truth or falsity. Compare this to the solemn hopes of Robert Roberts, Robert Nee and Bernece Simon that somehow social work theory will make a step forward just through the coming together of the various theories exposed at their symposium.

For although the ideas put forward by 'grand theorists' such as Marx, Freud and Hollis are very useful to their disciplines and also appear very logically consistent and universal, it is very clear to most people in their respective disciplines that they still fail to account for the infinite variety of man which is the subject of all three studies – not to mention the infinite variety of circumstances in which he can appear. It is because of this infinite variety of man that the formulating of grand theories comes close to being pretentious indulgence. Instead the central advancement of these subjects will come about fundamentally through their empirical work – historical research or social work practice. Breakthroughs can be expected in historical method and social work theorising, but that they might be the main channels of study in their respective disciplines seems absurd.

The question remains for both subjects – how is man's behaviour – past or present – to be studied and evaluated? Sheer accumulation of 'facts' combined with straight narration was once the vogue in history and it is interesting how distorting and unscientific this apparently 'common sense' mode of study turned out to be. This was because of the large degree of selection (despite appearances!) of material to be described, all the more insidious because this selection was unthinking and uncontrolled. It placed emphasis on day to day political events rather than underlying economic factors, and concerned itself with what appeared important, rather than finding what might actually be important. It is obvious that descriptions of a similarly unthinking selection of case histories would make a very limited contribution to casework as a subject.

For although empirical material must be used (unlike the Roberts and Nee writers), its selection must be with a purpose. It might be used to illustrate a general point; a good example of this can be found in pages 79 to 81 of Mark Monger's *Casework in Probation* where's the case of 10 year old Billy is related in some detail in order to illustrate the

importance of demonstrating feelings towards a child probationer, rather than expecting just words to suffice. A more valuable use of material in the long term is to test, verify and question hypotheses concerning casework, and in this way stimulate further hypotheses in turn. A topical example of this would be the large scale investigation of the relative effectiveness of psychodynamic and behavioural methods of treating an aggressive child. Even if a clear pattern emerged, say in favour of behavioural methods, the investigation would not end there for we would need to know which of the behavioural methods were most effective, and in what circumstances which methods were best, and why such methods fail in some cases (I assume they do!).

It is in the form of such a dialogue – between evidence and ideas – that I consider all knowledge to progress. In the social sciences the variables are enormous which is why progress in them is slow. It is significant that my example of how this interaction might work is in the field of psychology rather than casework as commonly expected. Psychology is the social science with the most highly developed a methodology, presumably because its variables are the least difficult to pin down – though not all that easy, as the reader of chapter 2 of *McCandless*¹ quickly realises. It is in this sense that I understand casework to be a science; the furthering of its knowledge is chiefly brought about by the same means as the physical sciences and, on a good day, by the social sciences – a theme I return to later.

But casework is not just an academic subject; it is also a practice, and this adds a whole new dimension to it as a discipline. If a purely academic subject were likened to a game of chess, where one tried out the moves (hypotheses?) on the chessboard (evidence?) to see how effective they were, one could understand how all the action is in the mind – spastics [sic!] and even blind people are capable of playing chess. But if one could not move one's pawn to K4 in the normal manner, but had to put it there with a cue, or 'hole' it with a golf club, then a whole new dimension of skill would be added to the basic game/academic subject. Such is the sort of dimension I consider the interview situation– the fieldwork action – to add to the casework discipline. If the parallel with sport seems to have unpleasant connotations, perhaps it can nevertheless be appreciated the main comparison I wish to draw. This is that casework in practice is a skill, requiring aptitude. Some people may have a greater aptitude for it than others, and some may never be any good at it, yet on the other hand it is still a skill that requires a lot of learning and developing. For no innate aptitude is anything concrete in itself, but it needs disciplined training– if only by the self. To my mind this skill has little of the conscious reasoning and calculation of the sciences, and instead the importance (though not all-importance) of 'aptitude' gives casework the smack of being an art, as I understand it, in the same way as is a craft, like joinery.

Having given the main indications of the way in which I consider casework to be both science and art, I wish to briefly raise another factor before making a few further remarks about the term 'science' as applied to casework. This extra issue concerns the way in which casework must inevitably be an expression of its practitioner's social philosophy, or attitude to life. This I take to be true of all academic subjects to a greater or lesser degree, but in the social sciences in particular, where man as the direct subject concerned causes the widest possible issues of value (rather than supposed pure 'fact') to arise, then this seems especially and crucially true. The classic (but I suspect infrequent) example of a large value issue most clearly facing a social worker giving casework supervision is where a worker feels that his clients problems are largely beyond his control, or perhaps caused by a part of our political system such as a

¹ I think I was referring to Boyd R McCandless, *Children: Behavior and Development*, 1969

private landlording. He might be squarely faced with the decision whether to help his client accept the situation, thereby helping to perpetuate the social/political problem, or help his client become aware of the situation and perhaps make him more frustrated. There is no neutral position when such issues do arise. Just selection of field of casework, of cases for special consideration, and of course of choosing casework as a career in itself represent aspects of a worker's social philosophy. Of course it might represent his lack of thought-out values, but I would suggest that such a worker would be the poorer in such an instance.

Having recognised that, it remains to explain why I consider casework to be chiefly, if anything, a science. If someone needs a social philosophy to decide in which direction one's casework is to fly, and if one needs artistic(?) intuition, aptitude or skill to get one's casework off the ground, it is scientific method which engineers that it is a purposeful aeroplane one is flying, not a kite. Of course my plumping for casework as a science is partly because of my own understanding of what science is, and/but it is no attempt to say that casework is 'really' A Science any more than bread is 'really' flour, until one has combined it with yeast and water and then baked it.

My understanding of what science is is a pretty broad one, yet I hope it stands as a tight, coherent one. In his classic study of *Philosophy of Science (1953)* Stephen Toulmin draws a distinction between descriptive and explanatory science. His implication is that the explanatory are the 'true' sciences, and in this I follow him and believe that casework should fulfil such a role. This is important because popular prejudice by non-scientific academics, especially historians, against physical science is the idea that scientists are mere technicians who flog away at inanimate matter trying to find tiny obscure laws or pieces of information, and whose progress is simple because their path is constrained by rigid theories already given. Florence Hollis seems to make the same equation of scientists with technicians – it is this underestimation of science that I would like to prevent.

For this hunting of regularities to transform observations into generalisations by deduction is a method confined to Toulmin's descriptive sciences such as natural history, where such 'laws' may emerge as 'all polar bears are white' or 'all ravens are black' – frequently laws of terminology. Instead the task of the explanatory physical sciences is to give intelligibility and form to regularities already given or realised – their laws must have "explanatory fertility" (Toulmin). In this they demand a comparable intellectual flare to such other sciences (as I see them) as history and casework. The physicist's explanations and laws are not absolute entities or droplets of pure knowledge as they are sometimes implied to be, but serve as models or maps to represent the physical problem concerned in order to make it more intelligible to the human mind – they are a means to an end. These models are not absolute or all embracing: for example a physicist will at different times represent light as waves or beams or merely as straight lines in order to convey early certain ideas about light ('explanations') – to other people.

The difference for the social scientist or caseworker is that the regularities to be explained are not obvious (if they exist at all) because of the infinite variety of the human subject matter. This has led many students to think that their true task as 'scientists' was to find regularities, and that only those that hold without exception could be deemed 'scientific truths'. This assumption seems to lie behind the apparent wish by the Roberts and Nee writers for an all-embracing theory of casework. Yet the question to ask of scientific theory is not "Is it true?" but "When does it hold?" That is how the dialogue between the hypotheses and evidence will most frequently progress, not by treating theories as absolute laws "of which we are not yet

sure” (Toulmin). I look forward to reading a casework book which is able to intelligently discuss theories of casework by relating them in detail to case histories in order to show just when different theories applied and how. Accordingly I hope that, if I had to expand his essay into 50-odd University of Chicago Press pages, I would be able to relate its content to specific case histories, rather than elaborate it by further refinement and regurgitation of my hot air of abstractions.

If my interpretation of casework has seemed excessively academic, perhaps I should justify myself by pointing out that this a rationalisation of how I think a caseworker should be thinking about casework both when he is caseworking and ‘only’ studying. It could be claimed that in practice his thought must be far more instantaneous and instinctive than my laborious account implies, and might therefore regarded as ‘artistic’. However, although I would to some extent agree with that – for I do not deny the ‘artistic’ element – I believe that it is really nonetheless scientific for that, for presumably the physical scientist has knowledge and faculties behind him in practice that are second nature to him. This background remains, if not explicitly. I would say that both he and the caseworker are each working on a scientific craft.

For, as must be abundantly clear, the question whether casework is an Art or a Science is not particularly important in itself, or even answerable. The concepts overlap painfully: the understanding and progress of quantum theory requires some imaginative thought (though I would still say that this was really scientific), and a good piece of criticism of King Lear needs scientific thoroughness of thought behind it. The importance of the question has been the issues raised by it for thinking about casework.

My tutor Ann Holden wrote: “A very interesting and well produced piece of work. It appears from the essay that you have spent considerable time in thinking through the issues raised and that the emphasis on this is very positive – you do not get caught in the trap of simply taking the argument that casework is an art and thereby not a science or vice versa, and you have thereby produced a much more meaningful piece of work. I liked your very broad use of analogies and feel that given the title of this essay they were used appropriately.

Well done! A”

[It was not immediately apparent to me that she was awarding a grade A to the essay, as at the time I was unused to grades being given for non-examination work.]

Afterword: It was not until the late 1980s that I started working out how to turn the theory of this ‘scientific method’ into operational practice, in a form of what would now be called ‘performance management’. It is much harder to do in practice than most people seem to realise, but it is not impossible, as I have explained in my much later writings.