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Science and social work: a sketch Videnskab og socialt arbejde: Et udkast

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While recognizing that understanding of ‘science’ varies across time and countries, there are strands of a shared albeit diverse inheritance. Failures to see where we are located within this inheritance make the social work community vulnerable to simplistic claims regarding what, for example, ‘doing science’ is like. This in turn makes it difficult to deal adequately with questions such as in what ways can or should we distinguish social work science from other kinds of knowledge? Is science in some recognizable way a unified form of knowledge? How ought we to deal with disputes and disagreements in social work science? What kinds of consequences might we envisage from social work science? I deal in turn with each of these questions.

Keywords: Social work science; tacit knowledge; science language; science practice; history of social work; kinds of knowledge

Forståelsen af ‘videnskab’ varierer mellem forskellige historiske perioder og fra land til land. Imidlertid er der elementer i opfattelsen som hidrører fra en række forskellige fælles kilder. En manglende erkendelse af, hvordan vi positionerer os indenfor denne fælles arv, gør vores sociale arbejde sårbart overfor simplificerende betragtninger, fx vedrørende forståelsen af ‘videnskab’. Dette gør det efterfølgende vanskeligt at beskæftige sig med spørgsmål som fx hvordan vi skelner videnskabelig indsigt i socialt arbejde fra andre typer af viden. Kan videnskab overhovedet identificeres entydigt som en bestemt form for viden? Hvordan skal vi forholde os til diskussioner og uenigheder indenfor forskning i socialt arbejde? Hvilke konsekvenser kan vi forudsætte at denne forskning vil have? Jeg vil i det efterfølgende beskæftige mig med en række af disse spørgsmål.

Søgeord: Forskning i socialt arbejde; tavs viden; videnskabelig sprogbrug; videnskabelig praksis; det sociale arbejdes historie; vidensformer

While it is not unusual to encounter writers who offer a fairly categorical stance regarding the relationship between social work and science, surprisingly little has been written on the subject by way of exposition. This may be because, like politics and religion, such positions are too often taken to be matters of value rather than judgement. In the following pages I sketch a few outlines, hoping I do not ‘end up a cartoon in a cartoon graveyard’.¹ I will suggest a view not unlike Timms’ ‘delineation of social work as concerned with understanding rather than information, and understanding not necessarily with a “practical” or predetermined end in view’ (Timms,

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1972: 1f). Because thinking and acting are ineluctably united we should consider social work as primarily ‘neither an applied science nor simple good works but a kind of practical philosophising’ (p. 3).

The sequencing of this article is a matter of judgement. The train of thought is as follows. Understanding social work science is not possible without understanding that to which we are historical heirs. But having begun to do this, questions of words and language cause us trouble, and I move to consider some of the main questions and challenges so raised. These two sections can be regarded as clearing the ground for a consideration of three interconnected questions. How does a consideration of the process of doing science help us to locate social work science both in its own right and, importantly, in relation to other traditions and forms of inquiry? Second, how is science related to other forms of knowledge that are a central part of social work practice? From this broad field I focus primarily on what is being said when we speak of knowledge being in some sense taken for granted, or tacit. Third, having seen that science is not a sealed off or superior way of knowing, this raises the question in what sense, if at all, is science in social work a unified enterprise. I close the article with some conclusions about how different frames of meaning and explanation exist side by side in social work science, and suggest some principles of courtesy, manners and respectful engagement across such differences.²

Inheritances

By and large social work’s predecessors a hundred years and more ago held a far more optimistic view of science, although the picture was not as monolithic as sometimes we are led to believe. In the UK that position has often been detected in the work of the Charity Organisation Society and its first general secretary, Charles Loch. Loch argued that charity ‘is not spasmodic, casual and emotional, but, like science, an all-observing, all-comprising intelligence. It is not antagonistic to science: it is science – the science of life – in operation – knowledge doing its perfect work’ (Timms, 1968, p. 59). In the USA this unflinching faith in science can be seen in Arthur Todd at the University of Minnesota. In a secular version of Paul’s chapter on love to the Corinthians, Todd says science is

broad, tolerant, earnest, imaginative, but poised and self-controlled. It is not impatient of contradiction and criticism given honestly and sincerely. It is fearless, truthful, teachable. It is able to withstand mob mind, sentimentality, sensationalism, and petty partisanship ... Finally, the scientific spirit means generosity, fellowship, and hearty cooperation untainted by jealousy. (1919, p. 73)

But it was equally evident in those who backed the Settlement movement. Take Clement Attlee—later to be one of the most significant if least appreciated of British Prime Ministers. On the emergence of social science he concluded ‘The careful dissection and investigation of social phenomena is a comparatively modern achievement, but it has perhaps done more than any single factor to change the outlook of men and women on social problems’ (Attlee, 1920, p. 14). ‘Research has been made into almost every phase of poverty, and many of its causes have been elucidated’ (p. 15). ‘Science has been rescued through the work of the practical social worker, the experimenter, and the investigator’ (p. 17), such that science in this field has become ‘the hopeful science’, and social work ‘the legacy of the prophets’ (p. 18).

The connection between the scientific motive and social reform was seen by Atlee as more than a means to an end.

There are numbers of social workers who find in the work of research and investigation the best outlet for their desire for social service ... the scientific motive takes its place as one of the incentives that lead men to devote themselves to social service.

‘Each group of social workers, each Settlement, has been a laboratory of social science in which new theories are tested’ (pp. 16, 17).

From what roots had Atlee and his various contemporaries grown? In terms of the UK, the English position on social evolution plausibly has been seen as the outcome of tension between older English positivistic attitudes to science and German romanticism (Burrow, 1970). In the English speaking world, and perhaps more widely, it is possible to view much shared intellectual history and to a significant degree welfare interventions as a battle between those who wish to give emotion, spontaneity, intuition, and the life of the imagination their due recognition against what was seen by those as an all-eroding positivism. Within this, the positivist tradition was never monolithic nor invulnerable,

not only because of what was formidable in its opponents’ case, but because of the inconvenient surprises which its own programme, the attempt to apply scientific methods to as wide a variety of social phenomena as possible, inevitably laid in store for it. (Burrow, 1970, p. 1)

Confidence in science was frequently—almost inevitably—associated both in European and American social work and social science with a periodized model of society’s progress.

Not all held this position. Beatrice Webb is an important figure for her role, with Sydney Webb, in founding the London School of Economics and for her influence on the future Welfare State in Britain. As can be seen in her journal entries and later recollections, she came quite early to reject what she viewed as the religion of science. She expressed it tellingly in a diary entry for 8 and 9 December 1903 on how she had an increased ‘distaste for all varieties of utilitarian ethics, all attempts to apply the scientific method to the *Purpose* as distinguished from the *Processes* of existence’. Talking of Herbert Spencer she concludes that ‘His failure to attain to the higher levels of conduct and feeling has sealed my conviction in the bankruptcy of science when it attempts to realize the cause or the aim of human existence’ (Webb, 1971, p. 62). Indeed, already by 1887 she was writing in her diary that ‘The religion of science has its dark side. It is bleak and dreary in sorrow and in ill-health. And to those whose lives are continual suffering it has but one word to say – suicide’ (p. 115). The religion of science entailed ‘an implicit faith that by the methods of physical science, and by these methods alone, could be solved all the problems arising out of the relation of man to man and of man towards the universe’ (p. 102).

She identifies ‘two outstanding tenets, some would say, idols of the mind’:

There was the current belief in the scientific method, in that intellectual synthesis of observation and experiment, hypothesis and verification, by means of which alone all mundane problems were to be solved. And added to this belief in science was the consciousness of a

new motive; the transference of the emotion of self-sacrificing service from God to man. (p. 146)³

In consequence ‘Social questions are the vital questions of the day: they take the place of religion’ (p. 164).

From the flight of emotion away from God to the service of man, and from the current faith in the scientific method, I drew the inference that the most hopeful form of social service was the craft of a social investigator. (p. 165)

The legacy of such positions helps elucidate why relations between the social sciences and social work—now as then—are not straightforward to understand and explain. Sociology and social work began:

not as distinct fields but as part of a general impulse for social science that emerged out of the reform activism of the nineteenth century. What we today take for granted as the “natural” division of social science into separate disciplines, including sociology and social work, was a decades-long development out of that original impulse. (Lengermann & Niebrugge, 2007, p. 63; cf Shaw, 2014b)

This did not prevent sniper fire from one side or the other. In the USA, during the 1920s, Edith Abbott in Chicago complained, in a barely coded reference to Chicago’s Sociology Department, that ‘some of our social science friends are afraid that we cannot be scientific because we really care about what we are doing ...’ (Diner, 1977, p. 11; C.f. Abbott, 1931; Shaw, 2014d).

Social work science words⁴

We almost daily run across science words that form an unavoidable part of our vocabulary—disciplines, experiment, positivism, nature, science and scientific method, and theory; then again subjective and objective, ideology, pragmatism, and empirical. Almost all strong, difficult and persuasive words.

We use words to bind together certain ways of seeing society and social work. But we may also use words to open up issues and problems of which we need to be much more conscious, such that it yields an active vocabulary, a way of investigating and presenting problems of meaning. By doing so this does not mean that a given problem will thereby be solved. To borrow an example from a different vocabulary, understanding the complexities of the word ‘class’ does little to resolve class disputes and conflicts; just as understanding evidence-based practice or postmodernism does little to improve social work. But these disputes cannot be thought through—or probably even brought into focus—unless we are conscious of words as elements in the problems. Such an exercise can contribute, if not resolution, then, in Williams’ words, ‘just that extra edge of consciousness’ (Williams, 1983, p. 24).

As Hitchings remarks in his remarkable book *The Secret Life of Words*, ‘words contain the fossils of past dreams and traumas’ and so enable ‘an archaeology of human experience’ (Hitchings, 2008, p. 4). Take for example ‘eugenics’. ‘Darwin’s polymathic cousin Francis Galton came up with *eugenic* in 1883; the politics of Social Darwinism were made respectable by means of a handsome Greek name’

(p. 274). This illustrates how ‘a more self-inspecting attitude tends to call for, or give rise to, a vocabulary more clearly touched by science – or by the illusion of scientific nicety’ (p. 275). In these ways ‘language betrays frailties, anxieties and the precariousness of self-image’ (p. 325).

There are still echoes in social work of the response that says it is basically a matter of education, and that when we see a word the first thing we need to do is to define it. For many words that does help—but for words that involve ideas and values we need to start from how such words have a history and complexity of meaning. It is common to speak of the ‘proper’ or ‘strict’ meaning of a word although many in social work may be more likely to reply that a word means only what it is now taken to mean, or that meanings are defined by contexts of use. Of course context is hugely important, but ‘the problem of meaning can never be wholly dissolved into context’ (Williams, 1983, p. 22).

Drawing largely on Williams (1983, pp. 278–279), a glance at the word ‘science’ serves to illustrate. In the 17th century ‘science’ began to be distinguished from ‘art’, though not at all in the way we make that distinction now. Science then was a kind of *argument* rather than a kind of *subject*, thus foreshadowing some sense of how we use the word ‘research’. Our use of science as particular fields of study had still to appear. But Constable was convincingly still saying in 1836 that ‘painting is a science, and should be pursued as an inquiry into the laws of nature’ in a lecture to the Royal Institution. But a key differentiation came from elsewhere in the 18th century distinction between experience and experiment. Changes in the idea of ‘nature’ encouraged development of the idea of experiment towards the external world and the conditions for the emergence of science as the theoretical and methodical study of nature were then complete. The idea of neutral methodical observer and external object of study became generalized to be associated not only with ‘science’ but also with ‘fact’, ‘truth’ and ‘reason’ or ‘rationality’, and thus to have profound consequences for other areas of study. The specialization of the word science ‘is perhaps more complete in English than in most comparable languages’ which causes us difficulties especially between English and French. There are also continuing difficulties in our use of ‘scientific’ to mean ‘methodological rigour’ or the old and continuing sense of ‘a demonstrative proof in an argument’.

Doing science

Leaving to one side the relationships between different fields of science, I want to listen at length to the voice of Peter Mitchell, biochemist and Nobel Laureate. Interviewed for the BBC by Lewis Wolpert (Wolpert & Richards, 1997), and asked how he became a scientist, he says when he was very young he ‘tended to be better at thinking about the general rather than the special’ (p. 83f). On having chosen chemistry he responds ‘I don’t think I’ve ever really *chosen* anything much in my life ... You recognize that there are *possibilities* there for your psyche, for your soul, for your being to fulfil yourself’ (p. 84).

Asked why he believes science can as well be done in a house or in the countryside as an urban university, ‘the ongoing process of science is really the gentle art of investigation into the nature of the world and ourselves. It *is* a gentle art ...’ (p. 86). He talked about how he got to his ideas. The problem he is basically interested in is the relationship between what goes on in a single cell and what goes on in the organism

as a whole—the chemistry and the physiology. He wanted to find ways of understanding this as one thing not two things:

When I was first at university I became very interested in the Greek philosopher Heraclitus, and I began to think that there were two kinds of things in the world. One kind, which I called for myself “statids”, were like teacups, which don’t evolve in any way except that they gradually get broken, they get less recognizable. And the other sort were things like rivers, which Heraclitus talked about, which flow. The identity of the river is different, its environment is constantly going through it. So I called these things fluctids, “flow things”. Flames are like that and people are like that. (p. 86)

The interviewer responds ‘But Peter, are you really telling me that your highly technical theory of how you get energy made in the cell was really based on these very general, and somewhat romantic images of the world?’

Yes, completely so. And I specially agree with the word “romantic” I think this is something which more scientists ought to explain – that we don’t do science because we are scientists, because of science – we do it because we are human beings. It is a most wonderful, romantic, cultural activity, just as much as being a sculptor, in fact, more so ...

But in order to be creative one needs ‘to go through the ... dreary business of school ... There is a huge amount of information that you have to absorb before you can start walking about in it’ (p. 87). He was asked if he knew once he had the answer that it was right:

No, I don’t think you ever know, because it is never right, is it? When you are trying to appreciate the nature of the world, you’re looking at the real world, and you’re making images of it as if you were a sculptor ... You think you’ve got a good model, but it’s still only a model of reality ... And so, when you think you’ve hit on an answer, it’s never going to be more than a partial answer and that’s very good for your modesty. (p. 87)

He concluded with a reference to Jennifer Moyle his collaborator, who since retiring had had a full artistic life. ‘Maybe that’s been something about both Jennifer and me. I’ve often thought we’re not really scientists at all, but we’re just people, and we happen to have spent a lot of time doing science’ (p. 90).

The tendentious contrasts too often made by some social work writers that are dismissive of the natural sciences look almost untenable in the light of this and numerous other interviews in Wolpert and Richards. Choice, art, sculpture, the interlinking of chemistry, physiology and philosophy, the romantic, (un)certainly, and modelling reality all echo science talk across the social sciences. By way of partial connection, here is a much briefer but possibly not altogether different social work voice:

Being a professional social work practitioner or researcher is not a purely intellectual endeavor. It is an “art.” I say “art” because there is between artists and their material a unique and special connection, as there is between scientists and their subjects. “Science” and “art” are not dichotomous. They are complementary. Both move away from the sensory to the realms of the theoretical and abstract in the pursuit of “truth.” Both are enterprises of discovery, begin with observation, rely on similar forms of metaphor and analogy, go beyond nature, endeavor to rise above the literal, translate “data” into higher orders of conceptualization, plumb into deeper levels of

significance, locate patterns and themes, decipher underlying meaning, and seek general truths. (Fox, 2012)

Doing science poses further questions. What kinds of relationships exist between people who are doing science? How is doing science influenced by and interactive with other parts of our lives? Do scientists have epiphanies (or as Sinclair suggests ‘slow revelations’, Sinclair, 2012). What do/should we think about the diffusion and dissemination of knowledge? Are there research networks in social work science? Have there been—and are there—schools within social work in the sense that US colleagues may have spoken of a Functionalist School, or sociologists may speak of the Chicago School? These social aspects of doing scientific work can be seen when exchange and deliberation take place. These often are invisible, but the extract in **Box 1** from a case study of social work research networks by Shaw and Lunt (yet unpublished) illustrates something of the dynamics that might be present in an informal network held together solely by mutual research interests and with very low-tech support. One of the men in the network is speaking.

Box 1. Network Dynamics

... well there's, there's a dynamic within the network which is applied versus basic if you like, or another way of putting it would be theoretically oriented versus practically oriented, and that's an important dynamic in the network, it's unresolved, and that's good. I mean it ought to be unresolved, because there isn't ... a way of saying that Alexis' interest in Marxist social policy for example, Marxist theory, is irrelevant to the question of whether or not a welfare to work possibly could work in the Netherlands. There should not be, and there never was I think in the network, a position on that, because it was clear that there were theoretical perspectives which people could offer which would inform the way in which practical application of research to policy would take place, and for me, keeping that unresolved, valuing people's theoretical contributions, without advancing theory over practical relevance was incredibly important.

There was a wonderful exchange in [Finland] where a guy called Lucas Johansson attended from Sweden, taken over from Katrin, and John gave a highly theoretical account of a particular development, and Lucas said at the end of this, ‘But what use is this?’, and the question was almost breaking the rules, because the point was it was, it was an offer from John to expand an area of thinking, and it's for network members to consider how it might apply in their case, or be useful to them, there shouldn't have been a request to John to say to any individual network member ‘And this is what it means for your country or your research project or your specific research interest.’ But I think for me that's, that was an interesting example of the dynamic within the network that holds some things together in tension all the time, in tension.

Science and not science

In what ways can or should we distinguish social work science from other kinds of knowledge? The famous (in the UK and the USA) essay by Snow on ‘The two cultures’ still serves as a reference point for such discussions (1956). He calls these the ‘traditional’ (mainly literary) and the ‘scientific’, and laments the ‘precious little communication’ between them. The scientific culture is expansive, ‘certain that history is on its side, impatient, intolerant, creative rather than critical, good-natured and

brash'. He opines 'neither culture knows the virtues of the other; often it seems they deliberately do not want to know'.

Aspects of this brash sense of being on the side of history are not unknown in social work. For example, empirical practice has been seen by some advocates to entail a claim to 'move away from vague, unvalidated and haphazardly derived knowledge traditionally used in social work toward more systematic, rational and empirically-oriented development and use of knowledge for practice' (Fischer, 1993, p. 19), together with predictions of a historical tide in its favour (c.f. Okpych & Yu, 2014).

Snow wittily observed that 'the difference in social manners between ... Los Alamos and Greenwich Village would make an anthropologist blink'. For example, 'sometimes it seems that scientists relish speaking the truth, especially when it is unpleasant'. Dannie Abse, the Welsh poet and doctor captures the sense of deep difference in his poem *Song for Pythagoras*:

White coat and purple coat
can each be worn in turn;
but in the white a man will freeze
and in the purple burn.

Snow asked what the loss was for those within the traditional culture. 'Those without any scientific understanding miss a whole body of experience: they are rather like the tone deaf, from whom all musical experience is cut off and who have to get on without it.' He concluded tellingly that the greatest enrichment the scientific culture can give those within the traditional culture is a moral one.

Among scientists, deep-natured men know that the individual human condition is tragic ... But what they will not admit is that, because the individual condition is tragic, therefore the social condition must be tragic too ... The impulse behind the scientists drives them to limit the area of tragedy.

Art, practice, the practical, experience, expertise, common sense, faith, tacit knowledge, lay knowledge, personal taste and even skill or technology are some of the words and phrases that are placed in counterpoint to science. In terms of how this relates to science and social work three observations are in order. First, a simple binary categorization of the distance between science and other forms of knowledge is almost always the wrong place to start. The section on 'doing science' above serves to make this point. Second, there has been some empirical work, most prominently in and influenced by the sociology of scientific knowledge and practice. Third, the level and extent of social work attention to these issues has on balance been inadequate and deleterious in its consequences for the field. There are exceptions to this charge of neglect around, for example, expertise, art, and practitioner forms of knowledge, but some of the more interesting earlier contributions do suffer from negligence.⁵

A major source of lost opportunity in this area lies in the lack of 'translation work' of key writers in other disciplines, and the failure to recognize the potential relevance of certain fields. Four examples spring to mind. First, an area of thought that does not seem to have been addressed in social work is the question of what differences should or can be regarded as differences of *judgement* and what should or can be seen as matters of *taste*. The distinction rests in seeing matters of judgement as 'discussable'

such that evidence and argument can be brought to bear. ‘Taste’ is ‘incorrigibly subjective; it is private; and for that reason there is nothing that can be said about it that has any consequence’ (Shapin, 2012, p. 172). In this connection we have little empirical understanding of questions like how does one become an enthusiast for, for example, randomized control trials; or how does one become a good researcher?

Second, the generally thin discussion of matters of *faith* in relation to social work science would be enriched and unsettled by considering how arguments for objectivity have been dealt with by some reflecting on Christian history (Hooykaas, 1972; McKay, 1987). Third, the rather undifferentiated ways in which ‘practice wisdom’, common sense and experience-based knowledge (as in the phrase ‘experts by experience’) are too often treated would be diversified by looking at what some have done around the notion of *disinterestedness among citizens* (Evans & Plows, 2007; Schutz, 1967a) or being a *connoisseur* of something (Eisner, 1988, 1991; Polanyi, 1958). Fourth, there is a special case of ‘translation’ in terms of how *methods of inquiry* developed primarily as forms of research can be redirected, translated and newly inhabited by social work as forms of direct practice (Riemann, 2005, 2011; Shaw, 2011).

Tacit knowledge

To avoid a hortatory listing of missed opportunities, a more extended comment on tacit knowledge indicates the ways such closer attention may pay dividends. At first glance the complaint that tacit knowledge has been treated too infrequently and without due depth may seem unfair—it is not, after all, that difficult to cite social work writing that makes reference to the idea (Dean, 1989; Imre, 1985; Martinez-Brawley & Zorita, 2007; Pawson, Boaz, Grayson, Long, & Barnes, 2003; Zeira & Rosen, 2000). But I think the charge holds good.

The name most frequently associated with tacit knowledge is that of Michael Polanyi, and the allusion to knowing more than we can tell, as when ‘This fact seems obvious enough; but it is not easy to say exactly what it means’ (Polanyi, 1966, p. 4). When we think of tacit knowledge in professional work, it can be defined as knowledge or abilities that can be passed between experts by personal contact, but cannot be or has not been set out or passed on in formal statements, diagrams, verbal descriptions or instructions for action (Collins, 2001a). There are actions, judgements and recognitions that we accomplish spontaneously. We do not have to think about them prior to performance. We are often unaware of having learned to do them. While we may remember once being aware of the understanding necessary for the action, we typically are now unable to describe the knowing that our actions reveal. It has become ‘thinking as usual’ knowledge’.

Polanyi comes at this from thinking about how individuals learn and act. But tacit knowledge is not limited to what goes on in an individual’s mind. For example, sociologists of knowledge in practice have explored ways that practice is a social form (Collins, 2001b, p. 110): ‘People in different social groups take different things to be certain knowledge but they are not aware of the social basis of their certainties’. Collins has been more concerned to understand the significance of such collective forms of tacit knowledge.

He suggests that there is a problem with the term. There is a tension between tacit as ‘is not’ and tacit as ‘cannot’. Hence do we think of tacit knowledge as knowledge that *is not* made explicit or that *cannot* be made explicit? In the first case the

antonym of 'tacit' is 'explicit' whereas in the second case the opposite of cannot is 'explicable'. Take, for instance, Altheide and Johnson's remark that 'Tacit knowledge exists in that time when action is taken that is not understood, when understanding is offered without articulation, and when conclusions are apprehended without an argument' (Altheide & Johnson, 1994, p. 492), where they imply tacit knowledge as knowledge that is not explicated. This distinction is rarely if ever made in social work writing, but is central to how we understand the significance of tacit knowledge as part of social work science and practice.

Collins also takes issue with Polanyi's 'know more than we can tell'. The key words 'know', 'tell' and 'can'/'cannot' all are in need of explication. When Polanyi says 'All knowledge is either tacit or rooted in tacit knowledge' this does not mean that all knowledge is tacit knowledge, but it does mean that the idea of 'explicit' is much more complicated than we (once) believed. 'There is nothing strange about things being done but not being told – it is normal life. What is strange is that anything *can* be told' (Collins, 2010, p. 7). As Becker remarked, there is nothing intrinsically wrong with an implicit, unarticulated theory: 'Reality is what we choose not to question at the moment ... [and] the better shape science is in, the more the positions are implicit' (Becker, 1993, p. 220). The following example is from a well-known American social work textbook.

Tpis for qvttluaiaie inwirtneveig:

- Fzriiamliae ylsuorlf wthi yuor qnisuetos as mcuh as plisosbe in acadvne.
- Ask fololw-up qnisuetos beasd on waht you are hnreaig; use the ptaiiratcpns' wdors wehn plisosbe.
- Aivod Indeaig qnisuetos.
- Erlxpoee iessus but dno't itgrentroae the pnpectaaaiirt.⁶

Collins says there are 'three remarkable things' (Collins, 2010, p. 116) about passages such as the one I have 'taken' from Padgett (2008).

- (1) We can read the passage and we do so via meaning. We are working through an interpretive process.
- (2) When I sent this to the journal the reviewers and copy-editor did not even think that the paragraph needed correcting despite the red wavy lines under almost every word. They are demonstrating meaning at a different level.
- (3) If I were to give the same passage but broken so that every letter in each word is in completely random order, you would have nonetheless tried initially to make sense of it, and known when you should give up in the one case but persist in the other.

It will be clear by this time that when we speak of tacit knowledge we are talking about various things. While his position is not without its critics (Godin & Gingras, 2002), Collins has done more than anyone to unravel this, and a reading of his argument sets productive questions for social work science. He distinguishes three forms of tacit knowledge, namely relational, somatic and collective.

Regarding the first of these he insists that 'any one piece of relational tacit knowledge can be made explicit, because the reason it is not explicit is contingent on things that can be changed. But all relational tacit knowledge cannot ... be made explicit at

once' (p. 98). This is tacit in a loose sense. It is 'knowledge that could be told and some of it is not' (p. 92). It refers to instances when, for example, someone knows something but does not know what is the important part of that knowledge, and so cannot tell someone. Also it includes ostensive knowledge—'knowledge that can be learned only by pointing to some object or practice because the description in words, though everything is there to be seen and described, would be too complex to be spoken and apprehended' (p. 93). Hidden knowledge, kept secret, also enters. In a major study of the England and Wales' governments' introduction of the Integrated Children's System (ICS) (Shaw et al., 2009), there were social workers who portrayed the need for using ICS forms when talking to service users as being an obligation to comply with a new administrative system. To borrow a distinction from Heidegger, this was correct but not true.

Bicycle riding, due to Polanyi, has become a paradigm of tacit knowledge. We speak of some skills as being 'just like riding a bike', meaning the body's 'knowledge' of this skill once acquired is not forgotten. Collins uses the term somatic tacit knowledge to refer to this. I recall in days long past that I employed a skilled copy typist to type my doctoral dissertation, and found that she had little idea what she had been typing. In other words, she did not need to have meaningful words passing through her head in order to type swiftly and efficiently. Her brain 'knew' where the letters were without having to make sense of the words.

Collective tacit knowledge—the third kind—is, for Collins, 'the irreducible heartland of the concept' (p. 119). Bike riding and car driving in traffic are different to, for example, bike balancing, because in both cases social interaction is central. Also this varies between countries and between different kinds of setting—mountain biking, city, rural or *Tour de France*. In these respects 'the right way to do things can only be captured through experience' (p. 122). How does an individual acquire this kind of knowledge? We may say it is through immersion in society, by participating in the talk and practices of society. To become fluent in a language 'is to master the tacit knowledge inhering in the conceptual life of society' (p. 135). 'We can describe the circumstances under which it is acquired, but we cannot describe or explain the mechanism nor build machines that can mimic it' (p. 138). Schutz' essay on 'The Stranger' (Schutz, 1967b) expressively depicts the social position of the newcomer to a community where thinking as usual has its customary tacit qualities.

In the light of the foregoing, how might we consider the question whether tacitness is inherent in our knowledge? Is it a part of how we are or is it a 'God of the gaps' kind of explanation, where we might conceivably be able to render our knowledge explicit? This question 'matters' when we are engaged in identifying the different forms of knowledge that are part of professional work. For example, in a helpful example of such an attempt in the case of social care knowledge, Pawson et al. (2003) devoted effort to suggesting how tacit practitioner knowledge can be rendered explicit. The aspiration may be wise, if only to avoid the alchemy of intuitionism and appeals to 'personal style' that beset some professional practice, so long as we do not deceive ourselves that the goal is fully achievable. But a problem is that they in effect treat all tacit knowledge as, in Collins' terms, relational. Yet doing practitioner research, using a standardized social work assessment instrument, preparing a report for a court or a case conference, or applying a task-centered model in a given case probably all entail a mix of relational, somatic and collective tacit knowledge, which in principle could be split out, to consider how far knowledge can be rendered explicit and

reproduced ‘mechanically’. In doing so it would be important to remember the different meanings of ‘making explicit’ and also the different things we may mean when we say we ‘can’ or ‘cannot’ do something. Collins suggests that Polanyi made it too mysterious and mystical and over-stressed the personal. While some social work writing sanguinely pleads for knowledge to be made transparent and explicit, it is also paradoxically too prone to intuitionism and appeals to the mystery of practice wisdom.

Sciences and science

Remarks about the differences of social work science from (and often superiority to) the natural sciences in general and perhaps some of the social sciences are badly in need of disentangling. The problems come from two directions. First, qualitative researchers are too prone to make unfavourable comparisons with natural science and often drag in the ‘positivist’ charge. From others one gets the image of the natural sciences as offering a benchmark of experimental, neutral rigour. Both are wrong. There are several issues tangled up in this, some of which are open to empirical work as well as other kinds of judgement:

- (1) Are the *concepts and theories* of social work of a similar or different order to those of the natural sciences?
- (2) Are the *methods of social work research* of a similar or different order to those of the natural sciences?
- (3) In what ways are the *practices of scientific communities* in social work, the social sciences and the natural sciences comparable?
- (4) Should social work engage with the natural sciences—and with natural scientists?

Phillips reviews these issues in the context of naturalistic ideals in social science. By ‘naturalistic’ Phillips means ‘a social science that is in important respects structurally or methodologically similar to the natural sciences’ (Phillips, 2000, p. xii) and carried out by someone ‘who holds that, in some fashion that he or she is obliged to make precise, the social sciences are similar to the natural sciences’ (p. 86). ‘In some fashion’ and ‘obliged to make precise’ are important caveats. He suggests four strategies for arguing that the social sciences parallel the natural sciences.

- (1) The use of certain concepts and findings from the natural sciences. This process ‘often requires great ingenuity and intellectual persistence’ (p. 92). However, ‘the use of such borrowed ideas only makes intellectual sense if the borrower is already committed to the view that there is no relevant difference between the natural and social or human sciences’ (p. 93). An obvious example here is the way ideas of evolution have been applied to society—Herbert Spencer was the best known such case though there are others closer to social work. For example, in writing about human needs Charlotte Towle argued for seeing the child guidance clinic as more than an agency but a laboratory and as having a place in social evolution.⁷ The current social work interest in neurosciences is of this kind.
- (2) The use of ‘deeper’ presuppositions from the natural sciences. E.g. that causation works the same in both spheres. This proceeds on the basis that causal

language is appropriate in the social sciences and that even in social affairs ‘there is a “fact of the matter,” a “social reality,” that is independent of what the researcher happens to believe’ (p. 94). The issues here are very complex. The work on critical realism is one field where the issues are being worked on (Houston, 2001).

- (3) The adoption of ‘structural’ features from natural science, e.g. theories. The early Chicago ecological models of the city were of this order, as is more recent work on systems and ecology.
- (4) That the methods of the natural sciences can be used fruitfully in the social sciences. This view of science takes us almost immediately back to Comte.

If we want to argue that social work science works in different ways we need to do more than simply refute one kind of argument for naturalism. There is cogency in Giddens’ position on the double hermeneutic. To describe ‘what someone is doing’ in any given context means knowing what the agents themselves know—it depends on ‘mutual knowledge’ shared by social scientists and the participants. There is no parallel in the natural sciences ‘which does not deal with knowledgeable agents in such a way’. ‘Lay actors are concept-bearing beings, whose concepts enter constitutively into what they do; the concepts of social science cannot be kept insulated from their potential appropriation and incorporation within everyday action’ (Giddens, 1993, p. 9). But while common sense may and rightly does shape the way hypotheses and problems emerge in the natural sciences, ‘The relation between the natural scientist and his or her field of investigation ... is neither constituted nor mediated by mutual knowledge, in the way I have defined the term’ (p. 14).

Conclusion: disagreements and manners

Much of this article has centered on the relationship between science and social work progress, and the significance of different kinds of knowledge to this purpose. I am conscious that points made in one section could be extended and used to counteract points similarly extended from other sections. This follows from how five different explanatory structures have been deployed to frame and argue the relationship between social work and science.

- (i) Fall from grace or rising from the pit. Expressed more formally, these set Romantic and Enlightenment views of history against one another. They are often manifest in debates regarding the merits or hazards of evidence-based practice, and in disputes regarding the implications of technological initiatives.
- (ii) Cycles of history. It is not easy to find this position expounded in the history of social work or research. It is often presented as a relatively pessimistic position, as in conversational exchanges where change seen by some as progress is dismissed by others as the return of past positions and arguments. I have not been able to trace a social work version, though it is possibly implicit in various arguments. For example, Davies’ debated and significant argument about social work as often involving ‘maintenance’ (Davies, 1994) certainly seems to move away from a linear model of social work. Davies would place his position within the merits of systems theory and Mertonian

functionalism for justifying the existence of social work. 'I remain of the view that the underlying dynamic is concerned with the security of society, the social survival (or maintenance) of people deemed to be at risk and the provision of new opportunities for individuals to regain the initiative to further their own welfare.'⁸

- (iii) Science and social work as historically relative. This can be seen in some forms of postmodernism, in the critical, poststructural interpretivism adopted by Denzin and Lincoln (Denzin, 1997; Denzin & Lincoln, 2005), and in the development of strong paradigm positions with the idea of the incommensurability of knowledge between paradigms.
- (iv) Science and power. Ideas developed within critical theory from Horkheimer onwards, and later in Foucault, refuse to treat knowledge as autonomous. Caroline McGregor (formerly Skehill) has applied Foucault's position to the history of child protection in Ireland (Skehill, 2004), and has written one of the more helpful expositions of the 'history of the present' for social work, illustrating how it is well suited to studies using a problematization approach (Skehill, 2007, p. 450) and affords a 'concern to use history as a means of critique of taken-for-granted "truth" in the construction of practices and discourses' (p. 451).
- (v) The story as told by the winners. To give just one example the historian of sociology, Platt, complains that 'Accounts written from within sociology, as history of sociology, generally treat both other disciplines and groups outside the academy as parts of the background. They are seen as instrumental to the main aims of sociologists, or as introducing distortions into the ... course of pure sociological development' (Platt, 1996, p. 264).

Such frames of meaning are not held like paradigms, in that arguing from one position may not exclude arguing from another. One difficulty is that many deliberations about what is believed by advocates of postmodernism, evidence-based practice and so on, are presented primarily through the arguments of those who disagree with those positions. Sometimes they are also advanced in almost personalized ways.

How should such disagreements be handled? My approach is shaped by a position on the vexed question of the relationship of philosophy in general and epistemology in particular to research (Shaw, 2014a, Witkin, 2013, 2014). I am comfortable with neither the strict notion of incommensurability and paradigm differences, nor with the view that methods of research are purely rational or empirical matters. I assume that I can—and probably must—talk with (not at) those with whom I profoundly disagree, on the assumption that in principle we can hear each other. And that I should respect and assume that their positions are held with integrity and have as much right to be heard as mine. In accord with this, I have long taken the rather challenging (for me) approach of asking people with whom I deeply disagree to comment critically on my draft papers. To borrow a phrase from the poet Robert Frost in 'Mending Wall', I spend time thinking what I am walling in or walling out. And also with Frost I have no time for hunters who leave 'not one stone on a stone ... to please the yelping dogs'. I aspire to provoke conversation and debate. I doubt it would make consistent sense to do any of this if I believed that those with whom I disagreed lived in another paradigmatic world.

In general I bring a view about science that in some but not all ways is like that of Weber. Science, Weber insisted in the gendered language almost universal at the time he was speaking (1948 [1919]), demands a ‘strange intoxication’. ‘Without this passion ... you have no calling for science and you should do something else. For nothing is worthy of a man unless he can pursue it with passionate devotion’ (Weber, 1948 [1919], p. 135). As Medawar later expressed it, ‘to be good at science one must *want* to be – and must feel a first stirring of that sense of disquiet at lack of comprehension that is one of a scientist’s few distinguishing marks’. That entails ‘power of application and (a) kind of fortitude’ (Medawar, 1984, pp. 9, 10).

But while enthusiasm is a prerequisite, no amount of enthusiasm, remarked Weber, ‘can compel a problem to yield scientific results’. ‘Calculation’ is also indispensable. Perhaps tongue in cheek, recognizing how such aspects of science have been thought at different times to be inferior, he advises that ‘No sociologist ... should think himself too good, even in his old age, to make tens of thousands of quite trivial computations.’ But further, some ‘idea’ has to occur if one is to accomplish anything worthwhile. ‘Normally such an “idea” is prepared only in the soil of very hard work’ but not always the case. ‘Ideas occur to us when they please, not when it pleases us’. For example ‘when taking a walk on a slowly ascending street’ (Weber, 1948 [1919], p. 136). ‘Yet ideas would certainly not come to mind had we not brooded at our desks and searched for answers with passionate devotion’. This is reminiscent of a remark attributed to Isaac Newton, who when asked how he had arrived at the theory of gravity said—not by seeing an apple fall but – ‘By thinking about it all the time.’⁹

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Notes

1. The line is from Paul Simon, ‘You Can Call Me Al.’
2. I have dealt with the issues in this article at length in book form (Shaw, 2014e), but I drafted the article before writing the book.
3. She never came to accept either for herself. ‘All that happened was my detachment from Christianity’ (p. 107). For her and for many it was the Great War that undermined these confidences ‘now that we have learned, by bitter experience of the Great War, to what vile uses the methods and results of science may be put’ (p. 146).
4. In adopting the expression ‘social work science’ I do not wish to imply that social work *is* a science, but only that in some sense and to some degree aspects of social work scholarship can plausibly be regarded as possessing scientific characteristics. I have discussed this in a conversation with John Brekke (cf. Brekke, 2012, 2014; Shaw, 2014c).
5. E.g. Timms work on art and science in his *Language of Social Casework* (1968).
6. This is from a passage in Padgett (2008). Each word retains the first and final letters in the same position but randomizes the sequence of the other letters.
7. Though the majority of social work uses of evolution are loose references to, e.g. ‘the evolution of social work knowledge’.
8. Personal communication, 21 August 2014.
9. Quoted by Lewis Wolpert in Wolpert and Richards (1997, p. 14).

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