FOCUS

Mike Lloyd

ABSRACT

Adopting the mantra of sports psychologists – be focused – can sometimes work for social scientists, but just as clearly we need to think about how focus might or might not improve the quality of social inquiry. One way of focussing is to begin with definitions, but these so often turn out to be tendentious or essentialising. We have to learn to live with perplexity, and to question anew our assumptions about scale – could the smaller be the bigger thing there is? This paper reflects on such issues in an intentionally provocative manner, using as exemplars the work of Latour, Garfinkel, and Grimmett. The latter will not be familiar to many, as he is a spin bowler, nonetheless, his endeavours in mastering his cricketing art provide a useful lesson for those who wish to gain pleasure from detailed, theoretically informed, empirical social science inquiry.

INTRODUCTION: NO FOCUS, NOT LOST

Wittgenstein's *Philosophical Investigations* (1958) is one of those books that is often cited but seldom read, at least in its entirety. It's organisation in the form of 'perspicuous presentations' does not lend itself to easy identification of key points or an overall argument, in fact, you can quite easily get lost in it. Moreover, Wittgenstein realised this, as he was reluctant to see the book published, perhaps reflecting his perfectionist tendency (see Monk 1991), something exemplified in these words from the preface:

> After several unsuccessful attempts to weld my results together into such a whole, I realized that I should never succeed. The best that I could write would never be more than philosophical remarks; my thoughts were soon crippled if I tried to force them on in any single direction against their natural inclination.— And this was, of course, connected with the very nature of the investigation. For this

> compels us to travel over a wide field of thought criss-cross in every direction.— The philosophical remarks in this book are, as it were, a number of sketches of landscapes which were made in the course of these long and involved journeyings. … I should have liked to produce a good book. This has not come about, but the time is past in which I could improve it (1958: v–vi).

Despite the book's lack of what is traditionally called 'focus', it is one of the most influential, sharp, and insightful works in twentieth century philosophy. It is certainly not possible to enunciate in a few words what it is 'about', yet anyone with a mind to do so will find something significant within its pages.

This gives us an initial point to reflect upon: it should not be taken for granted that focus is inherently positive; instead, it deserves critical scrutiny. It is to opening up focus in this kind of manner that I wish to reflect upon here.[1] Remaining with Wittgenstein for a moment longer, it could be asked, given that the work is said to be like a 'number of sketches of landscapes', why not attempt to provide an overall 'map' to the work? To introduce a related term, if a work lacks focus, why not attempt an exercise in 'scaling'. Focus and scale might be thought to share 'family resemblances': by scaling, cartographers, geographers, urban planners, trampers and the like, are able to manipulate the 'too large' real world, to a manageable, mapped, reality – something they can spread on a table to find their way, to focus their endeavours. The same can be done for a book, after all, this is what a table of contents, a preface or a short conclusion attempt to accomplish. One reason why Wittgenstein may have rejected scaling might be seen in a remarkable thought experiment on the question of scale, extracted from Lewis Carroll's book *Sylvie and Bruno Concluded*:

> 'That's another thing we've learned from your Nation,' said Mein Herr, 'map making. But we've carried it much further than you. What do you consider the largest map that would be really useful?'
>
> 'About six inches to the mile.'
>
> 'Only about six inches!' exclaimed Mein Herr. 'We very soon got to six yards to the mile. Then we tried a hundred yards to the mile. And then came the grandest idea of all! We actually made a map of the country on the scale of a mile to the mile!'
>
> 'Have you used it much?' I enquired.

> 'It has never been spread out yet,' said Mein Herr: 'the farmers objected: they said it would cover the whole country and shut out the sunlight! So we now use the country itself, as its own map, and I assure you it does nearly as well'
> (Carroll 1893, quoted in McMaster and Sheppard 2004: 1).

Without going into the details, it is clear that Wittgenstein's directive to 'describe' and avoid generalising explanation is consistent with the notion of working with a map on the scale of a mile to a mile. He seems to be saying, 'consider this X in its full detail, then let's criss-cross to the full detail of Y in this other piece of the landscape'. The question of why we should focus on X and Y, and how they might be related, is not his concern, at least not in any explicitly formulated manner.

Strange as it may seem, there are good reasons for adopting such a stance, not the least because of the nature of language, something to which Wittgenstein devoted a great deal of thought. Consider a thought experiment. Imagine you need a precise definition of the word 'sovereign'. I grab the dictionary I have (the *Collins Concise*, 3rd ed. 1992), find 'sovereign' and read the first entry: '1. a person exercising supreme authority, esp. a monarch'. But then I am not sure of the meaning of the word 'monarch', so I turn to that and look it up, finding: '1. a sovereign head of state'. Interesting: to understand 'sovereign' we are referred to 'monarch', but when we look that up, we are referred to 'sovereign'. To show that this is not an odd case out, try it with a 'reality' word, that is, 'thing'. In the same dictionary, I find under thing: '1. an object, fact, affair, circumstance, or concept considered as being a separate entity'. This time I go to 'object' and find: '1. a tangible and visible thing'.

This circularity of language certainly does not result in difficulty understanding the use of words. Clearly, this is because we can look to the context a word is used in to uncover its meaning (exactly as teachers instruct new entrants). That is, words are found some*where*; sense-making is an irredeemably situated practice. We get into trouble in defining things only if we approach definition in a certain way. The key is to seek working definitions, definitions that will do for the practical purposes at hand. If you do not ask for a definitive account, we have no difficulty, we just go on making do, using the resources that are readily and visibly available. It is only 'as soon as we ask 'What is…?' in the sense of 'What is…essentially?' or 'What is…in general' [*that*] we can get into deep water' (McHoul 2004: 425). Maybe this has been a core source of trouble in the socio-cultural disciplines: too often we have asked focused questions like, 'What is Culture, essentially?', or 'What is Society, in general?', or 'What

causes social order, essentially?' Such questions need dissolving, they do *not* need answering in a definitive fashion. If these questions can be let go, it is as if a large weight is lifted from our shoulders, for there is no answer to the problem of how to start with the correct definition, the correct focus for resolving our academic problems. In other words, we have to learn to live with perplexity and incompleteness, something well put by Strathern: 'If one thing observed close to appears as perplexing as many things observed from afar, the perplexity itself remains. Each single element that appears to make up the plurality of elements seen from a distance on close inspection turns out to be composed of a similar plurality that demands as comprehensive a treatment' (1991:xv). What becomes key, then, is reporting only what you need in order to accomplish the tasks you have set yourself; this means relinquishing the 'full story' approach to socio-cultural inquiry (see Law 2004 for a fuller treatment).

THE SMALLER IS THE BIGGER THING THERE IS

The circularity of language example raises the possibility that the greatest complexity lies not in the structure of language per se but in its actual use, something that reverses the traditional scale assumptions about structure and practice. This is the intriguing argument, well expressed by Latour (2002), that the smaller is the bigger thing there is:

> We are so used in the social sciences to speak of levels of complexities, of higher order, of emergent properties, of macrostructure, of culture, societies, nation states, that no matter how many times we hear the argument, we immediately forget it and start ranking local interactions from the smallest to the biggest as if we could not think without stuffing Russian dolls one neatly into the other (2002: 124).

Latour explicates Gabriel Tarde's (a contemporary of Durkheim) argument that we can equally well argue that the big, the whole, is not larger than its individual elements (or monads), but is only a simpler, more standardised version of one of the monad's goals.

Thus, for example, I am not deluded in thinking that, while employed by the large institution called Victoria University of Wellington, as a monad I am more complex (or 'larger') than the entity which employs and surrounds me. Much like the thing we call 'society', Victoria University does not exist as a concrete entity, that is, an actual gathering of its constituent parts (where would such a list end?) bounded in space and time. Victoria, as an entity, is

more rhetorical-discursive in nature. It is replete with figure-heads who speak for it, espouse its mission statements, and, when it comes down to it, pursue relatively limited goals: they attempt to convince everyone else of the institution's 'excellence', they claim that a Victoria education 'makes you think', clearly so that students will continue to enrol and the institution can balance its budget. In contrast, when I am alone in my office with my books, papers, and computer, I am a far more complex entity. I may critique the institution that feeds me, I may contribute to its bureaucratic machinery, I may write articles focused on criticising my own discipline, I may recommend to a colleague that he or she apply for a job at another institution, I may check the sports news on the Internet, and so on, all in the course of one day.

It is this reading of complexity and scale that is often missed in the socio-cultural disciplines, particularly, as Latour argues, sociology:

> To be a good sociologist one should refuse to go up, to take a larger view, to compile huge vistas! Look down, you sociologists. Be even more blind, even more narrow, even more down to earth, even more myopic. …The 'big picture', the one that is provided by that typical gesture of sociologists – drawing with their hands in the air a shape no bigger than a pumpkin – is always simpler and more localised than the myriad monads it expresses only in part: it could not be without them, but without it, they would still be something (2002:124).

Is Latour suggesting that sociology got off on the wrong footing, right from the beginning, that it got its focus wrong? If you read any of Latour's key works (e.g., Latour 1987, 1993, 2005; Callon and Latour 1981) you would have to answer yes, but there is a more friendly way of approaching this issue. A good example is provided in Anderson, Hughes and Sharrock's classic book *The Sociology Game* (1985), worth briefly detailing.

The intention of Anderson *et al* is not to say that sociology is *just* a game, rather they adopt the word 'game' as a metaphor, partly to understand why so many students find sociology strange or puzzling.[2] This puzzlement stems from the chronic lack of agreement in sociology – it is riddled with disagreements and never seems to solve anything. This situation is closely connected to the common presentation of sociology as a collection of perspectives. A perspective is taken to be a more or less unified approach to doing sociology – for example, Marxism, structural-functionalism, symbolic interactionism, feminism, and so on. So, it seems that what sociology offers is a variety of different angles of

vision, or foci, on the social world. While the perspectives model has been a useful way of presenting sociology, Anderson *et al* argue that it also has some serious drawbacks. It tends to produce an unsatisfactory either/or choice. If sociology produces different points of view of the *same* subject matter (classically, of society), then the relationship between the points of view should be complementary. That is, given that each perspective yields a distinctive, but legitimate, portrayal of society, a key theoretical task of sociology is to synthesise and reconcile perspectives. In other words, we can add together perspectives for a better, more complete picture.

On the other hand, perspectives can be seen to be in a competitive relationship. Each perspective is actually seeking to be self-sufficient, in which case they cannot be added together because the very nature of sociology's subject matter cannot be agreed upon. On this view, the key theoretical task of sociologists becomes critiquing sociologists who espouse other perspectives, with the aim of showing that they are wrong and you have the best perspective. This can tend to degenerate into simple name-calling, or at best, the proliferation of misunderstandings. A simple check of any university's new book shelves, or where the new issues of journals are housed, will show the value of this argument: sociology is full of syntheses, re-evaluations, critiques, and what Smith (2002) has aptly called 'rescue narratives' (i.e. attempts to rescue the discipline from incipient crisis).

Like the perspectives view, Anderson *et al*'s metaphor of the sociology game focuses on diversity, but in a different way. When we use the word 'football' we have little difficulty in listing specific games – soccer, rugby, grid-iron, Aussie rules – that belong to this collection of games. But just because we can do this easily, it does not follow that these games share any one focus. More to the point, nor does it mean that any one of these games could not be collected under another label. For example, I have always thought that many racquet sports could be linked with rugby or soccer as 'possession games'. In the latter two sports, a key winning-strategy is possession of the ball in the right territory, that is, in the opponent's half of the field, and the closer to the tryline or goal the better. With the former (racquet sports), there is the clear difference of the materials of the game – court, net/tin, racquet, bouncy ball – nevertheless, winning strategies also depend on 'possession' of territory on the court. In either tennis or squash, after hitting any shot, a good strategy is to move to the central court area, thus 'possessing' the best space to move to any subsequent shot – by 'covering the court' you improve your chances of winning. So, the category 'possession game' could include soccer, rugby, tennis and squash, whereas the category 'football' would not include the last two games.

The point of the games metaphor is to emphasise that all kinds of connections can be drawn between specific games, and this is exactly the case with sociology's so called perspectives. Sociology is better considered a collection of games, orderly, but not overly so:

> As soccer consists in, but is not exhausted by, the use of various skills such as dribbling, kicking the ball, using space, tackling, drawing opponents out of position, and so on, so we can conceive of sociology 'games' as assembled out of appropriate activities such as theorising, using evidence, producing data, presenting an argument, all within rule-like constraints which 'players' must interpret as best they can (Anderson *et al* 1985: 15–16).

In this statement we arrive back with Wittgenstein. To play the sociology game we do not need to agree upon its focus, we do not need to have a consensus about its object of inquiry. Instead, we can see sociological competence centred around the various skills that we need to acquire – Anderson *et al* name theorising, using evidence, producing data, and presenting an argument.

Recently, Harold Garfinkel, the founder of ethnomethodology, has made a similar point in a most forceful way:

> Our first day of graduate work we were told to take Durkheim's aphorism to heart. Put it on our door sills. Seeing it there each time bless Durkheim and bless yourself and learn the mantra. According to Durkheim's aphorism, 'The objective reality of social facts is sociology's fundamental principle.' Sociology's fundamental principle? There's the rub.
>
> Durkheim had it beautifully and originally right, but his particular word 'principle' was ill advised. … For ninety years he was understood to have been speaking on behalf of contemporary detailed formal analytic social sciences. … But this is wrong. For ninety years Durkheim was talking about something else even though that something else was distinctly and uniquely sociology's subject matter. Which is what the studies by Ethnomethodology's authors are all about.
>
> Ethnomethodology understands Durkheim's aphorism differently. Ethnomethodology has been respecifying Durkheim's aphorism so that it reads differently. Yes, his aphorism reads, 'The objective reality

> of social facts is sociology's fundamental *phenomenon*' (2002: 65–66).

Garfinkel argues that there is too much 'mantra' learning in sociology. While Durkheim is singled out for mention, you can pick your mantra from any one of the founding fathers. It is this tendency to follow mantras that Garfinkel takes as the origin of the 'formal analytic social sciences', but the reason why he singles out Durkheim is the interesting contrast between 'principle' and 'phenomenon'. A principle is a guide for inquiry, something that sets up how you should proceed; this is where, by using 'formal', Garfinkel means proceeding from an established form, that is, by convention. He does not view such formality as a positive move. He thinks that it is by beginning from theoretical principles and problems that sociology gets off on the *wrong* footing. The alternative is to treat the everyday orderliness of daily life as a concrete *phenomenon* for investigation. This is to make focus not a noun or verb, but a task, an imperative. Exactly how focus is achieved is a contingent matter for every case of inquiry, not some kind of foundation based upon essential definitions.

This is not to say that a focus should be divorced from theory, or making arguments, or knowing a literature, for it should always be in some kind of relationship with those skilled activities. But maybe by placing more emphasis upon descriptive detail we could redress the imbalance where so often sociologists take detail to be trivial and banal, or something that gets in the way of theory. By doing so we might move beyond the speculation that is so rife in sociology. As Miller and McHoul so cogently put it,

> if you want to know about how people do things in everyday life and how it's possible for them to do those things in regular ways, why speculate? Why not - given that everyday life is everywhere and by no means hard to access - go and have a look at what folks are doing? And in going to look, one may discover properties of everyday events as they are, in their own right, rather than as *representations* of other matters… (1998:xi).

GETTING OUR HANDS DIRTY? TWO 'SOIL SCIENCE' EXAMPLES

Taking seriously this argument about the importance of studying everyday life, let's consider some aspects of soil science before concluding. The first example is a fieldwork account by Bruno Latour (1999), who gets himself involved with a group of French soil scientists who travel to the Amazon forest. They are engaged in a data gathering exercise designed to help them answer questions about whether the forest was encroaching on the savannah, or the

savannah was taking land away from the forest, being particularly interested in the role of the soil in this ecosystem battle. To answer this question they set out a well-planned grid on which they will take samples. At designated points they dig to a certain depth and take out a soil sample. This is carefully deposited in a small cardboard cube, marked with a reference number, and then deposited in its proper place in a box construction, called a pedocomparator, that houses many individual cubes. The pedocomparator is a remarkably simple construction, nonetheless, extremely crucial for their endeavour, for it allows them to accurately picture the changes in the forest/savannah soil distribution. Because each cube is placed in its proper, mapped, place, they are literally looking at a scaled down distribution of the forest soil. The pedocomparator provides a remarkable focusing device, as Latour puts it:

> What a transformation, what a movement, what a deformation, what an invention, what a discovery! In jumping from the soil to the drawer, the piece of earth benefits from a means of transportation that no longer transforms it. Having made the passage from a clump of earth to a sign, the soil is now able to travel through space without further alterations and to remain intact through time. At night, in the restaurant, Rene *[a soil scientist]* opens the cabinet-suitcases of the two pedocomparators and contemplates the series of cardboard cubes regrouped in rows corresponding to holes and columns corresponding to depths. The restaurant becomes the annex of a pedolibrary (1999: 51).

If only the socio-cultural disciplines could avail themselves of the equivalent of a pedocomparator. The soil is transported, thus allowing analysis, but the samples themselves are not transformed, thus addressing the desire of science to be objective. Of course, some social scientists would say that this is what sampling from a population attempts to do, but we do not need to rehearse the critique of positivism to realise that such sampling of social things always transforms what is transported. Sociological data can only be traces of the complex, heterogenous social worlds from which they are taken. Unfortunately, in our disciplines, transportation of social data does transform it. There is no avoiding that, but there may be minimisations of this transformation, so surely we need more focus on what we are transporting as data, and how we do this – yes, a call to reconsider methodology (but of a different kind – see Law 2004).

The second example is of a more lay type of soil science. It has to do with a New Zealand born cricketer called Clarrie Grimmett, described as the 'first

master of leg spin' (all details from Neely 2000). As a boy Clarrie lived very close to Wellington's Basin Reserve, where he and his friends spent most of their summers playing cricket. It was also there, as he grew older, that he was able to watch touring international teams to learn more about the skill of leg spin. By the age of twenty-one he was representing Wellington, and by 1913 was chosen as a non-travelling reserve for the New Zealand tour to Australia. Unhappy with being a reserve he decided to move to Australia, where he gained employment as a signwriter, but outside work hours persevered with his practise and experimentation with spinning the cricket ball.

Clarrie's level of perseverance was nothing short of fanatical. Married, and with a move to Melbourne:

> in the backyard of his house in Prahran he laid a pitch of Merri Creek Soil, which was used at the Melbourne Cricket Ground, and with the help of a trained fox terrier called Joe spent his spare moments working on a new delivery – the flipper. This ball was bowled with a leg-break action and the ball made pace from the pitch, and came in slightly from the off. Grimmett maintained that it took him 12 years to perfect this delivery. It was to bring him many lbw victims (Neely 2000).

Many New Zealand kids learn to play cricket in their backyard, and they may become relatively good spin bowlers by doing so, but I do not know of anyone who has gone to the lengths that Clarrie did. Consider the details of this story a moment longer. Wishing to perfect his spin bowling, Clarrie recreates the MCG pitch in his backyard, training his loyal dog to retrieve the ball (we could guess he has to do this without putting teeth marks in it). With this setup Clarrie labours long and hard to make the 'flipper' work. In the article describing Clarrie little additional detail is provided, but it is not hard to imagine Clarrie coming home from work, muttering 'practise makes perfect, focus, focus, focus', and checking in a notebook which aspect of his bowling he wished to work on that night. Maybe he had discovered that a certain angle of the wrist, or a slight change to how the ball was held in the fingers, was paying off, so he continues his trials on the way to perfecting the flipper. Twelve years later, based on this attention to detail, it brings a haul of lbw decisions (including many in games playing for Australia).

Clarrie is engaged in a finely detailed art. His focus is to perfect his new delivery – the flipper – which he hopes will bamboozle many a batsman. But to do this he does more than invoke a mantra-like command to focus, focus, focus.

What he does is *materialise* his focus: he recreates the pitch conditions where he wishes to enact his flipper. Then he practises. So, Clarrie works out *where* he wants to focus. This is meant in a perfectly spatial manner: it is the MCG where he wants the flipper to work; that is his focus. The focus comes with a materialised place. The delivery itself requires micro-management of his body – how he holds and spins the ball from his hand while striding in to the crease – but it also depends on reproducing the conditions of the MCG pitch. This affords an interesting comparison: the French soil scientists transport without transformation; Clarrie transports the MCG soil with the express goal of transformation. That is, he wants to transform the game of cricket on the MCG; he wants a stunning new delivery that he knows will work in particular soil conditions. In this he succeeded, and subsequently set up a tradition of spin bowling which culminated in Shane Warne, the highest wicket taker in world cricket history.

What is the point of these two examples? It is nothing less than to engage with the infamous micro/macro split, and to suggest how artificial this binary is. The Amazon forest is a large entity and the soil scientists are engaging in their equivalent of the sociologist's or anthropologist's 'structural' level of inquiry. But this macro-level inquiry depends on the little cubes of soil so carefully deposited in the pedocomparator. Without the little cubes there will be no paper written up in a soil science journal, no possibility of an enhanced understanding of soil ecology in the Amazon. Their macro-inquiry is but a slight extension of their micro-focus, that is, the cubes of soil that they literally transport about with them. Clarrie is engaged in an incredibly detailed training of his body; again, his macro-result is but an extension of micro-level practice. He, too, carries it about with him, in what, following Bourdieu, we could call his cricketers' habitus. Thus, we have not done away with talk of micro and macro levels, but we have transformed our focus to the 'doing' activities that are at the heart of these transportations and transformations. As Rose puts it:

> In English you can put the word do to all sorts of specific efforts and then you can wonder, not simply about those efforts, but how all such things are *done*, about how all things are brought about in the world. You can wonder about how to *understand* the *doing* of ever so many things (2004: 77, original emphasis).

Perhaps if we can undertake inquiry into the 'doing of ever so many things' without being tempted to 'draw with our hands in the air a shape no bigger than a pumpkin', then we would be well on track to answering the imperative to focus.

CONCLUSION

All our work involves extension. There is little point in pure descriptivism because that only takes us into the terrain of the one mile to one mile map, which, after all, remains a thought experiment. Was Wittgenstein so tortured about publishing his work with its strong argument for description and against explanation because he realised that it was a purist's argument? Those of us in the socio-cultural disciplines have traditions partly based upon 'getting our hands dirty' with empirical material. Even hard-core theorists do not reject the need for empirical research. Despite our differences, there are linkages amongst the most disparate perspectives in the socio-cultural disciplines. Clarrie Grimmett would probably have been thought of as 'odd', to say the least, if he only shared his new flipper delivery with his dog on that backyard pitch. Similarly, it is by avoiding dilettantism and elitism, and instead saying 'look, I can show you how my inquiry works' that we can share our activities with those of a mind to listen, whether academics or not. As Harvey Molotch (2001) has wonderfully put it, we need to get our noses out of our literatures and 'go out' more often:

> … our literature cocoons keep many of us safely out of view. The literature becomes topic, *sui generis*, and 'progress' in refining, testing, elaborating what the last journal article refined, tested, or elaborated becomes a sign of salvation, or at least tenure. In too much of our writing, authors' names perform the role of narrative tension: Jones was wrong but Schwartz is right, rather than education counts more than father's income. The plot line revolves around which scholar holds the trump, rather than which version of the world holds the wisdom. This allows underlying substance to be lost; after a while, somebody has to ask 'what was that we were talking about?' (2001: 178).

Without a principle, a foundation to a discipline, we are not lost, because we have phenomena that we can share and discuss. One thing that all of us can do, and do originally, is to focus on some detailed description of social worlds. We must then do something analytical with it, which is far from easy, but you first have to be able to collect and transport the detail that is available. Like the full-scale map, there is no shortage of it. If we can avoid getting locked in our campuses, and ignore the tendency to speed up our research, as the contemporary university seems to encourage, then we may be able to rediscover the pleasure of detailed, theoretically informed, empirical work. Unlike Clarrie,

we may not perfect a new delivery, but on the other hand our own 'invention' should not take twelve years, and should not require a dog for an assistant.

NOTES

1 Originally, this paper carried the sub-title 'a polemic'. On reflection, I realised that it lacked sufficient focus to qualify as a polemic (and besides I have always wanted to publish an article with a one-word title). I should be clear that the intent here is to be provocative, to stimulate by digression. I eschew heavy citation, being in strong sympathy with Inglis when he states that 'the contemporary habit in academic writing of assembling more or less trite summaries of other people's work, and disfiguring the prose at the end of every second sentence with a scattering of references, seems to me calculated to turn thought to lead and the eager reader into concrete' (2004: vii).

2 I continue to speak of sociology here mainly because it is my base discipline, however, I suspect the argument would apply to many other disciplines.

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