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28 *Anthropology and Ethics*, op. cit., p. 34.
29 Ladd, op. cit., pp. 351–2.
30 A discussion directly relevant to these problems is found in Cora Diamond's article 'Eating Meat and Eating People', *Philosophy*, Vol. 53 (1978), pp. 465–79. See esp. p. 467.
31 Ladd, op. cit., p. 348.
32 Ibid., pp. 273–4.
33 R. F. Holland, 'Good and Evil in Action', see Note 13 above. Quotation from p. 113.
34 Ibid., p. 114.
35 Peter Winch, 'Wittgenstein's Treatment of the Will', in *Ethics and Action*, op. cit., pp. 110–29.
36 Ibid., pp. 118–19.
37 Ludwig Wittgenstein, *Philosophical Investigations*, Basil Blackwell, Oxford 1976, p. 174.
38 I wish to thank Dr. Lars Hertzberg for his generous and inspiring comments on various drafts of this paper, and Professor Peter Winch for his kind encouragement. An earlier version of this paper has been published in *Norsk filosofisk tidsskrift*, Vol. 17 (1982).

II. Wittgenstein and Comparative Sociology

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Focusing on a discussion by Ruddich and Stassen of the 'Remarks on Frazer's *Golden Bough*', this paper shows that some of the usual criticisms made by sociologists of Wittgenstein are misplaced. He does not reject causal explanations of beliefs and actions and replace them with some other form of explanation, but dismisses the idea that any explanation is called for here. His argument that the origin of the desire to explain beliefs is to be found in a misconceived parallel between science and magic is explained and discussed.

Wittgenstein's views on comparative sociology[1] have been the subject of a fair amount of discussion and debate over the past quarter of a century.[2] One could have been forgiven for thinking that in all that time Wittgenstein's ideas ought to have been well enough ventilated to be generally understood. But, alas, this appears not to have been the case.[3] Indeed, almost the opposite has happened. The more his views are publicized and discussed, the less Wittgenstein appears to be understood. The reason for this is, we think, both simple and profound. Wittgenstein's conception of what doing philosophy entails and what philosophy can be about, is almost totally at odds with the general attitude of most sociologists, and many philosophers too, towards that discipline. This is not merely a question of inclination, opinion, and emphasis. It is a difference of mentality. Whereas, for most sociologists, philosophy is a prestigious and often poorly comprehended discipline whose task is to lay the foundations for proper method, secure knowledge, and just social relations, for Wittgenstein the practice of philosophy amounted to self-imposed therapy whereby the philosopher struggled to free himself of the limitations of particular ways of thinking. Philosophy is not a foundational science, it cannot tell us anything new about the furniture of the world, to use Hilary Putnam's phrase;[4]

all it can do is point out the consequences for the ways that we think of the ways that we talk. Wittgenstein's philosophical method eschews dogma, theories, and programmes. Its topics are existing philosophical dogmas, theories, and programmes, whether they be the classically articulated positions of scepticism, platonism, and nominalism, or the more inchoate and disguised ones of Freud and the instance to which we will direct our attention, Frazer's *Golden Bough*. So, while sociologists have been uncomfortably aware that Wittgenstein did not like what they commonly do, and has all sorts of objections to it, the clash of mentalities has made them incapable of appreciating the context in which he was arguing. The net result has been that all too often in considerations of Wittgenstein's views on social science, attempts have been made to find a philosophical camp to which Wittgenstein can be consigned, and then to find him to be irrelevant because of the well known failings of the camp to which he has been allocated.[5] In this paper we will try to show just what might be gained by an *appreciation* of Wittgenstein's arguments rather than an insulation from them. We will do so by focusing attention on a paper by Ruddich and Stassen[6] which examines at length Wittgenstein's 'Remarks on Frazer's *Golden Bough*',[7] the only place where he takes up in any systematic way the problems and possibilities inherent in comparative sociology. We do so not because Ruddich and Stassen's contribution has become prominent in the literature, for it has not, but because with regard to this one central text and in a very brief space it exemplifies nearly all of the crucial mistakes, confusions, and misunderstandings which sociologists make about Wittgenstein.

Although Ruddich and Stassen have some difficulty in following all of Wittgenstein's argument, they are fully aware of his conclusions. They summarize these quite correctly as follows:

> Wittgenstein's attack on Frazer's work is really an attack on the very idea of historical understanding, or, more exactly, on any causal account of history. (Ruddich and Stassen, 1971, p. 88)

A little later they say,

> Wittgenstein's method would discourage the search for general laws and principles in the social sciences. (ibid., p. 89)

Which is quite true. Wittgenstein did doubt the feasibility of a single generalizing strategy in social science. He did suspect that it might be best to start with description on a case-to-case basis and proceed slowly from there. But because Ruddich and Stassen fail to see why Wittgenstein says this, their counter-argument consists in the reaffirmation that social scientists do go in for generalization and that many sociologists think that this is right. They assert, without justification of any kind, that

> neither science nor philosophy can remain satisfied with a method that, by definition, precludes the concept of explanation. (ibid., p. 88)

And that,

> [the] scientific idea of explanation contains . . . universal applicability within the range of phenomena explained. (ibid., p. 89)

But how do we know that generalization and explanation are requirements for a social science such as sociology? How do we know that such generalization and

explanation is to be obtained as the outcome of a comparative method? In the end, it turns out that we know we can do so because authorities such as Edmund Leach tell us that we can.[8] But to say that Leach, or even the majority of sociologists, disagree with Wittgenstein is not to show, *de facto*, that Wittgenstein is wrong. What Ruddich and Stassen need to do is to show where Wittgenstein's arguments are shaky, where his inferences are wrong, and where he can be corrected. This, of course, they fail to do.

Let us chart the moves that Wittgenstein makes in his consideration of *The Golden Bough*. In essence, they are:

(1) Frazer wants to explain a certain religious practice, the ritual of succession of the King of the Wood at Nemi, and thinks that it is to be explained by the beliefs which the ancients held. By extension, any practice is to be explained by the beliefs associated with it.

(2) Since the beliefs are 'obviously' in error, it cannot be their truthfulness which underpins the existence and persistence of the practices. It must be something else. This something else, Frazer holds, is the evolutionary chain that the beliefs stand in. The practices develop because the beliefs are already in place as the descendants of earlier forms. To work through the evolutionary development of a set of beliefs associated with a practice is to explain that practice. Hence, showing that the beliefs associated with the practice of assassinating the King of the Wood are really kindred with other priest- and king-killing beliefs from other parts of the world constitutes a causal, comparative, scientific explanation of the ritual of succession at Nemi.

To all of this, Wittgenstein offers the following objections.

(1) There is no reason to suppose that an explanation must be had for the practices in question. To suppose that it must is to suppose that there is something in the patience which *needs* explaining. The inclination to try to find an explanation for everything is a preoccupation which is peculiar to our society. We only want to explain practices such as the ritual at Nemi because we presume that they must be based upon the erroneous beliefs.

(2) The assumption that the relationship between a practice and a set of beliefs is both explanatory and causal is the result of a fixation on the logical form of propositions. The fact that we explain material events by material causes, the working of the windscreen washer by our pressing of the switch, does not mean that we can generalize the relationships so that social events too have to have social and mental causes which explain them. The supposition that it must be so derives from a particular view of explanation and its importance. For Wittgenstein one could just as easily say that beliefs grow up alongside a set of practices because they both share a 'community of life'. The one need not be logically or temporally antecedent to the other.

(3) An explanation which consists of the tracing out of antecedent forms of a set of beliefs is only convincing to those who already accept this kind of explanatory form. Because it does not provide a causal mechanism, it is not, of itself, convincing. Its net effect is to explain away the phenomenon under scrutiny because it shows no sensitivity to the role that the performance of practices may play in the way of life of those who perform them. Such practices are reduced to exotic curiosities.

(4) Sets of practices may or may not be related to beliefs of some kind. To infer a set of beliefs from a set of practices is to run the risk of making the holders of those beliefs out to be unbelievably stupid. The fact that we 'touch wood' or avoid

walking under ladders does not mean that we hold beliefs about the intervention of a spirit world in our daily lives. The fact that we beat the ground and stamp our feet when we are annoyed does not mean that we blame the earth for our misfortunes and think that our actions will be instrumental in rectifying the state of affairs in which we find ourselves. In these cases such practices are not based upon hypotheses encapsulated in beliefs but might be better considered as 'gesture languages' expressing mood, emotion, feeling, and so on. Describing practices in this way is not to explain them as the articulation of a set of erroneous hypotheses about the weather, the world in general, or anything else. If we were to ask any one of these 'superstitious' natives about the natural world, it is likely that his set of practical explanations would be very little different from our own.

(5) The root problem is the inclination to seek explanations for everything, even though in our society there are many things that we do for which we could offer no explanation other than 'this is what we do'. Suggesting that we do these things because we hold a particular set of beliefs is to do no more than rationalize the activities, not explain them. No explanations are needed.

(6) To treat a set of beliefs as if it contained an irredeemably false meteorology or physics is to evaluate those beliefs in terms of our theories of physics and meteorology, and to disregard any intrinsic characteristics that beliefs may have. This is not to say that farmers might as well sacrifice to the field gods rather than fertilize and irrigate their land. It is to say that the *point* of understanding a practice cannot be to find it to be stupid. And yet that is precisely what Frazer does. The point must be to describe what the people who perform these practices are doing when they perform them.

(7) There is nothing intrinsically wrong with cross-cultural comparison and cross tabulation providing we are sensitive to the individual characteristics of each of the cases so compared and do not mislead ourselves by presuming that what we are producing are causal explanations. We would do better to see such an approach as one of 'perspicuous representation' whereby the general similarities of forms of practice are set out and related. Such a procedure might show how an ellipse looks like a collapsed circle, or chess looks like ritualized warfare. Such representations do not offer any kind of explanation, genetic or otherwise. They are simply ways of describing the similarities of form between practices.

Because of the distinctive philosophical attitude which Wittgenstein adopts, Ruddich and Stassen, along with many other sociologists, simply miss the general point he is making. That is most clearly summarized in his observation on 'Cause and Effect',[9] but has long been discernible in all his other writings. In order to start at the heart of the matter and then work our way through the rest of the related misunderstandings, we will cite Wittgenstein's comment in full.

> 26.9.37
> Think of two different kinds of plant, A and B, both of which yield seeds; the seeds of both kinds look exactly the same and even after the most careful investigation we can find no difference between them. But the seeds of an A-plant always produce more A-plants, the seeds of a B-plant more B-plants. In this situation we can predict what sort of plant will grow out of such a seed only if we know which plant it has come from. Are we to be satisfied with this; or should we say: "There *must* be a difference in the seeds themselves, otherwise they *couldn't* produce different plants: their previous histories on their own *can't* cause their further development unless their histories have left traces in the seeds themselves"?

> But now what if we don't discover any difference between the seeds? And the fact is: It wasn't from the peculiarities of either seed that we made the prediction but from its previous history. If I say the history can't be the cause of the development, then this doesn't mean that I can't predict the development from the previous history, since that's what I do. It means rather that we don't call *that* a 'causal connection', that isn't a case of predicting the effect from the cause.
>
> And to protest: "There *must* be a difference in the seeds, even if we don't discover it", doesn't alter the facts, it only shows what a powerful urge we have to see everything in terms of cause and effect.
>
> When people talk about graphology, physiognomics and suchlike, they constantly say: ". . . clearly character must be expressed in handwriting *somehow*. . ." 'Must': that means we are going to apply this picture come what may.
>
> (One might even say that philosophy is the grammar of the words "must" and "can", for that is how it shows what is *a priori* and what *a posteriori*.)
>
> And then you can imagine that the seed of a plant A produces a plant B and that the seed of this, which is exactly like that of the first, produces an A-plant, and so on alternately – although we don't know '*why*', etc.
>
> And now suppose that in the foregoing examples someone had at last succeeded in discovering a difference between the seed of an A-plant and the seed of a B-plant: he would no doubt say: "There, you see, it just isn't possible for *one* seed to grow into two different plants". What if I were to retort: "How do you know that the characteristic you have discovered is not completely irrelevant? How do you know *that* has anything to do with which of the two plants grows out of the seed"? (Wittgenstein, 1976, pp. 410–11)

It is because Ruddich and Stassen are caught in the powerful grip of the obsession with causal explanations that they cannot see that Wittgenstein is questioning the generalized applicability of that picture. And hence they insist that if Wittgenstein rejects Frazer's explanation of magic, this must be because he has an implied explanation himself; whereas Wittgenstein is not just rejecting Frazer but the idea of explaining all practices, magical or otherwise, in causalist terms. Having failed to see this, Ruddich and Stassen cast around for an explanation which might stand as an alternative to Frazer's. Not surprisingly, they find it difficult to come up with one. In the end, all they can settle upon are some asides which Wittgenstein makes about rain-making magic and the Beltane fire festivals. But, in both of these instances, Wittgenstein is *not* offering alternative theoretical explanations for the practices. What he is saying is that, *for these particular cases*, there is just as much evidence to suggest that the reason why the lots drawn in the fire ritual have a button inside them, for example, is because it was the button-maker's birthday. Frazer's connection is just as weak as Wittgenstein's. It becomes, then, a matter of examining the evidence for a relationship between beliefs and practices on *a case-by-case basis*. And, for these examples at least, Frazer's evidence is not strong enough to make the connection. Wittgenstein is not dismissing the possibility of relating beliefs and practices. What he is saying is that we do not have to suppose that the relationship *must be* causal; and for the cases Frazer discusses not enough evidence is produced to show that there is *any* connection.

Wittgenstein indicates that he is doing more than just quarrelling with Frazer's particular theory of magic and ritual when he talks about the role that plausibility plays in the conviction which explanations carry. This seems to mystify Ruddich and Stassen completely; and yet the point would seem to be plain enough. If we see a significant connection between the date on which a practice is performed and the form of the practice (for example, why the number of candles on a birthday

cake is the same as the age of the child whose birthday it is), then we will have no difficulty in also seeing a connection between the placing of buttons in the lots and the button-maker's birthday. Frazer's account will only convince us if we already accept the plausibility of genetic, causal explanations. The *amount* of collateral evidence from other practices will not make its *role* as collateral evidence any more convincing. It will only convince those, like Ruddich and Stassen, who think as Frazer does. Ruddich and Stassen choose Frazer's theory because he offers evidence – let us disregard for a moment whether Wittgenstein has an alternative theory. But such evidence is not of itself convincing; for a start, it would not convince a 'birthday theorist'.

Ruddich and Stassen find some justification for their suggestion that Wiggenstein has an alternative theory to Frazer in some of his remarks about behaviour. The passage they quote from is this:

> One could almost say that man is a ceremonial animal. That is, no doubt, partly wrong and partly nonsensical, but there is something right about it.
>
> That is, one could begin a book on anthropology by saying: when one examines the life and behaviour of mankind throughout the world, one sees that, except for what might be called animal activities, such as ingestion, etc., etc., etc., men also perform actions which bear a characteristic peculiar to themselves, and these could be called ritualistic actions.
>
> But then it is nonsense for one to go on to say that the characteristic feature of *these* actions is the fact that they arise from faulty views about the physics of things. (Frazer does this when he says that magic is essentially false physics or, as the case may be, false medicine, technology etc.)
>
> Rather, the characteristic feature of ritualistic action is in no sense a view, an opinion, whether true or false, although an opinion – a belief – can itself be ritualistic or part of a rite. (Wittgenstein, 1979, pp. 67–68)

The proposal that it is possible to discriminate 'animal' and 'ritualistic' activities is no more than a limited description; it is not an explanation of anything. Ruddich and Stassen say that the passage makes explicit Wittgenstein's theory of ritual and magic.[10] But they do not tell us what this theory is; and, try as we might, we cannot see one.

Having begun by assuming that Wittgenstein must be offering some kind of explanation, Ruddich and Stassen go on to jumble everything else. They do so by getting things partially right and crucially wrong. For example, they say:

> Basically what Wittgenstein is criticising is the notion of implied theories; that is, the idea that the analysis of magical and religious behaviour reveals an implicit interpretation of nature and society on the part of primitive peoples, and that these interpretations when made explicit, turn out to be false according to modern scientific criteria. (Ruddich and Stassen, 1971, p. 87)

And Wittgenstein does say something rather like this. But what he is actually talking about is the way in which evaluating magical practices in terms of scientific hypotheses is, in fact, cooking the comparison in advance. The natives' beliefs will always come out inferior to our own. The terms which are used load the comparison in particular ways. They have magic, we have science; they are superstitious, we are rational. He also says a great deal more. He asserts that matters of truth and falsity do not arise in these circumstances unless we force the comparison and insist upon treating religious beliefs as if they contained the equivalents of scientific

hypotheses simply because they look as if they are the same. To treat a proposition such as 'The Lord of the Winds is killed in his prime to keep his soul fresh' as symmetrical with 'Fruit is kept in cold stores to keep it fresh in winter' is to be misled by the form of propositions. Once that mistake is made, it is but a short step to saying that souls are not the kinds of things that can be kept fresh, and nor will killing someone do it, and hence that the natural degeneration theory of souls is false. But, extending the notion of theory beyond its home range,[11] that is to say beyond scientific hypothesis, is exactly what is at issue for Wittgenstein, because it makes the comparison meaningless. He suggests that if one looks to see what is going on when such propositions are being made, the role that the statement plays is not one of a theory or hypothesis at all. Theories and hypotheses alone can be matters of opinion; since beliefs are not concatenations of theories and hypotheses, they cannot be matters of opinion.

It is important to notice that Ruddich and Stassen do not dispute this argument of Wittgenstein's. However, they do not accept its conclusions. Rather than showing that beliefs *are* collections of hypotheses and theories about which there could be differences in opinion, they wonder whether Wittgenstein can be in his right senses to say that they are not, given the history of religion since the mediaeval period. Have not people been rationally persuaded away from beliefs? And has this not affected their practices? But the point is that where people have changed their beliefs, they have not adopted *new* theories. They have simply moved from talking about doing things in one way to talking about doing them in another. A parallel might be made with our practice of counting. When we moved from the duodecimal system to the metric system of measurement, we did not replace one theory of measurement with another. We changed the way in which we measure; we did not come to the conclusion that the duodecimal system was based upon an erroneous theory. A measurement system is not a theory of objects; a belief system is not a theory of practices. But, both may or may not be used as means of justifying what we do. They are *not* theories of what we do.

This confusion only arises because of two errors which Ruddich and Stassen commit earlier in their discussion. The first is to find a contradiction in the suggestion that beliefs are not theories while at the same time insisting that they are peculiar interpretations. But interpretations are not always to be treated as hypotheses or theories, *as these are conceived of in science*. And this is the point that Wittgenstein is making.

> The nonsense here is that Frazer represents these people as if they had a completely false (even insane) idea of the course of nature, whereas they only possess a peculiar interpretation of it. That is, if they were to write it down, their knowledge of nature would not differ *fundamentally* from ours. Only their *magic* is different. (Wittgenstein, 1979, pp. 74–75)

Compare this to an earlier and more famous remark:

> The same savage, who stabs the picture of his enemy apparently in order to kill him, really builds his hut out of wood and carves his arrow skilfully and not in effigy. (Ibid., p. 64)

To repeat, what Wittgenstein is inveighing against is the drawing of a parallel between scientific practice and magical practice, as a preliminary to the comparison of scientific theories and religious and magical 'theories'. Since scientific theories

and religious beliefs are wholly different in the role which they play in the lives of the societies which have them, they are not to be compared as 'true' and 'false' theories. The practical knowledge of how to get things done in the world is shared by ourselves and the natives. What they don't have is science; but then science is not our form of magic either. When comparing what we do with what the natives do, we will have to pay the closest possible attention to ranges of similarities and differences. We shall have to be on our guard not to export one set of differences or similarities from one sphere of behaviour to another. The whole thing does not turn out, as Ruddich and Stassen suppose, to be a mere matter of interpretation and non-interpretation, but of the comparison, in all their detail, of ways of life; of what we, and they, happen to do. And this depends on the local, particular community of life. 'If fleas developed a rite, it would be based on the dog' (Wittgenstein, 1979, p. 73).

The second error which Ruddich and Stassen make is to underplay the criticism which Wittgenstein offers for the laxity of causal connection which Frazer provides. We have already alluded to this in passing. Frazer suggests that people *react* to drought by murdering the rain-king and the Lord of the Winds, but offers no evidence to show that this *is* a reaction. Without demonstrating the connection, Frazer cannot have the practice caused by a meteorological theory. Wittgenstein does not have to offer evidence to show that it is not a reaction since the burden of proof is on Frazer. All Wittgenstein is asking is how we are to assess the validity of the evidence for the case that is being made out. Because a people do some particular actions at the same time that some other conditions obtain, this does not mean they have a theory of those conditions which explains their actions. The conditions and actions could just as easily occur alongside one another. This case could not be cited, therefore, as evidence for the connection between beliefs and actions when considering some other separate instances; and nor will appealing to its similarities with other quite different cases be a proof of anything. That Ruddich and Stassen cannot see this is testimony to the way that a clash of mentalities lies at the heart of their misunderstanding.

By way of a conclusion, let us summarize what we think Wittgenstein is saying. We will set the points out as a list so that there will be as little room as possible for misunderstanding.

(1) There are no grounds for supposing that every institutionalized set of practices is to be causally explained by an associated set of beliefs.
(2) A genetic reconstruction of different forms of similar beliefs is not a causal explanation of why the associated practice is carried out.
(3) Religious beliefs are not the equivalents of scientific theories and hypotheses, and are not 'false' versions of scientific theories.
(4) Causal explanations do not explain why people engage in activities; they are, in the cases offered, weak connections between beliefs and practices.
(5) The connection of beliefs and practices will have to proceed on a case-by-case basis. It is not possible to generalize the argument in either direction. Some beliefs may well turn out to be closely tied to practices; others may not.
(6) Comparative sociology of the kind that Frazer engages in is possible, but it is descriptive not explanatory. Its descriptions comprise 'perspicuous representations'. Hence, in the comparisons it makes, the differences between practices will be just as important as the similarities.

This is not the place to explore the implications of Wittgenstein's observations for the practice of sociology. As Ruddich and Stassen foresaw, these are profound. In future publications we hope to examine the tasks set and the possibilities made available.

NOTES

1 We are using this term to designate both comparative sociology and social anthropology. We are well aware of the chagrin this will cause.
2 The primary stimulus was, of course, Winch (1958 and 1964). Other sensitive contributions have been made in closely related areas by Dilman (1981), Cook (1978), Hunter (1980) and Rhees (1969).
3 So much so that a recent volume of essays contains the tired old suggestion that both Wittgenstein and Winch are relativists of some sort. (Lukes and Hollis, 1982, p. 10.)
4 Putnam (1982).
5 The usual one is that of cognitive and ethical relativism.
6 Ruddich and Stassen (1971). We are well aware that this discussion is a decade and more old now. Nonetheless, it is the only extensive review by sociologists of the 'Remarks on Frazer' that we know of.
7 Wittgenstein (1979).
8 We are tempted here to quote a certain notorious young lady who, when informed that an eminent personage with whom she had claimed a degree of intimacy, had denied all knowledge of her, replied, 'Well, he would, wouldn't he?'.
9 Wittgenstein (1976).
10 Ruddich and Stassen (op. cit.), p. 87.
11 Cf. Luckhardt (1980) for a succinct review of Wittgenstein's views of this as they impinge on the problem of ethical relativism.

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