Objectivity and There is No Such Thing as a Social Science make an odd pair: one is a substantive historical discussion of a philosophical concept central to philosophy and to scientific practice and debate which provides an explanation of the history of the development and changes in the concept; the other is a defense of a philosophical position which in effect denies that any such explanation is possible, and attacks “the craving for explanation” as a philosophical disease whose major symptom is social science itself. Galison and Daston, the authors of Objectivity, are historians of science whose approach is connected to the “social study of science” without explicitly adopting any of its methodological theses. But in taking on the concept of objectivity they go to the philosophical heart of the scientific enterprise itself. By historicizing it, or rather in the course of historicizing it, they show that the dominant notion of scientific objectivity today was neither inevitable nor is it exclusively valid even now. The book by Hutchinson, Read, and Sharrock, No Such Thing, takes the form of a defense of Peter Winch’s The Idea of a Social Science and its Relation to Philosophy. They update this famous text by defending it,
polemically, against its critics, who are treated as enemies, misinterpreters, and general error-mongers. But this is no mere rehash. There is an underlying argument: that Winch is the pure heir of Wittgenstein working in conformity with the correct understanding of Wittgenstein, that the problem of relativism in Winch is a hoax, that Quine, Davidson and Rorty, along with Norman Malcolm, Wittgenstein’s most faithful pupil, are deeply misguided, and that the idea of a social science—either interpretive or causal—is still a delusion.

There is a real issue here, which bears on both books. Despite the claims of There is No Such Thing to the effect that Winch is theory-free, The Idea of a Social Science relies on a particular set of closely related theoretical ideas: concepts, rules, and of the act of “grasping” these things as a fundamental mental act. The authors never explain why they think they are theoretically innocent terms, though, as we will see, they do explain Winch’s intentions and his model of social science in a way that allows them to think of what Winch proposes as something other than just another theory, namely as a kind of therapy for the understanding. The people the authors attack, notably Donald Davidson in his classic essay “On the Very Idea of a Conceptual Scheme”², avoid these terms. This avoidance points to the big issue behind this book: what sort of understanding can we have of beliefs and actions that are not our own? Daston and Galison’s Objectivity exemplifies one approach: accounting for the historical contingencies and contexts that make beliefs and opinions intelligible. Hutchinson et al defend a radically different approach that denies that “interpretation”, causes, explanation, and the like have any role in the kind of


understanding we can have of others. The issue is fundamental: it is, nevertheless, esoteric.

Webs of Belief vs. Conceptual Schemes

The background involves the concept of “concept”. There is an innocuous use of “concept” to mean, roughly, the idea behind a word that is used in expressing beliefs or claims. The possession of a concept in this sense just means the knowing use of the words in a way that is understood by others and by the user. We can use the words or not. If we do use them, and if we need to use them in order to say what we believe, they are part of our ontology. If not, they aren’t. To say “God bless you” to someone who has sneezed is not to commit to a belief in the Divinity, if we might as happily have said something else. But there is a more loaded way of using the term—to describe a separate realm of reality, the conceptual.

What is the difference, and why does it matter? When we think about concepts in isolation it doesn’t matter much—being able to use a word and possessing a concept are not very different. The differences become more serious when we start talking about sets of concepts related logically to one another to create a scheme. Winch doesn’t use the term conceptual scheme, but appeals to the same core idea of a system of concepts when he endorses the idea that “the relation between idea and context is an internal one. The idea gets its sense from the role it plays in the system”\(^3\). Usually the loaded way is associated with linguistic idealism, the claim, as Ilham Dilman, one of Hutchinson et al’s key sources, puts it: that all awareness of reality is mediated through (linguistic) concepts, or in relation to the claim “that there is no reality outside

language, its grammar and logic"\(^4\). As Dilman says, “... our language is not founded on an empirical reality with which we are in contact through sense perception. Rather, our language determines the kind of contact we have with such a reality and our conception of it. This is Wittgenstein’s “Copernican Revolution”\(^5\). Kuhn’s notion of paradigm is part of this family of idealist notions.

According to the people whom Hutchinson et al reject, beliefs are linked to other beliefs in a web of beliefs. The links in the web are the inferences that we make from the sentences. This notion is analogous to the idea of conceptual schemes or systems, but with a significant difference. Beliefs or opinions are the sorts of things that can be true or false. They can be formulated in sentences. The web of beliefs can be adjusted in the face of new information in order to salvage theoretical claims that appear to conflict with new empirical claims. But at the same time there is underdetermination, the underdetermination of theory by data: conflicting webs of belief, or theories, can be constructed in the face of the same empirical claims. Sentences can be translated, though the meaning of a sentence is a matter of context, or the web of belief that it is part of, so that there is underdetermination, or indeterminacy, here as well.

The difference is this. Concepts are foundational: they are not, like opinions, claims, or beliefs, the sorts of things that can be true or false. They can be “grasped” correctly or not, applied correctly or not, but they are beyond justification. They are the dead-end of analysis. They cannot be compared to reality to see whether they fit, in the way that Weber and the neo-


\(^5\) Dilman, “Wittgenstein and the Question of Linguistic Idealism” (p. 172).
Kantians often talked. They may be organized into conceptual schemes, or paradigms, but by definition these are not subject to further justification or analysis. They don’t depend on a web of belief: they are arranged in a stack rather than a web, and beliefs rest on the foundation of concepts. Concepts, because they are foundational, analytic dead-ends, can’t be replaced or changed in the way that beliefs are, namely by such devices as changing the beliefs in the web that support them. They must be changed by people grasping new concepts and the new conceptual schemes of which they are a part—of jumping from one stack to another rather than readjusting the web by replacing one supporting belief with a different one. Winch is clearly committed to the stack picture. He insists that religion can’t be compared to science, and the reason he gives is this: “each has criteria of intelligibility peculiar to itself”\(^6\). Intelligibility is a notion at the bottom of a stack, below concepts, or as a concept foundational to concepts. He rejects the idea that philosophy or social science has any business “taking sides”\(^7\) in such disputes as those that arise between science and religion, which he accuses Vilfredo Pareto of doing.

Objectivity: Foundational Concept or Belief?

*Objectivity* exemplifies the web of belief approach, with a special emphasis on practice—another problematic term—in the specific sense of procedures and methods of making images. The book describes the emergence of two approaches to the objective depiction of nature. The book is full

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of examples of visual depictions of nature performed under the two approaches. Anyone who has had the experience of being a beginning biology student and looking through a microscope at a slide and identifying the parts of a cell will understand the point already: the pictures of the parts of the cell in the textbook are crisp line drawings with distinct bodies on a flat plane; all one can see on the slide is faint mushy shapes that seem to run into each other. One approach to objective depiction enhances the objects so that what is objectively there in nature can be seen and understood. The other insists on photographic exactitude: the choice is clarity via artistic enhancement over mush. But there is always an element of artifice in getting the right photograph to represent the objective truth: stains, enhancements, etc. are part of the craft. There were bitter arguments over these two conceptions, with the “photographic” or “mechanical objectivity” eventually, though only partially, winning out over what Daston and Galison call the truth-to-nature approach.

Daston and Galison explain their historiographic approach in detail, and their self-explication goes like this:

By the middle decades of the nineteenth century, the epistemology and ethos of truth-to-nature had been supplemented (and in some cases, superseded) by a new and powerful rival: mechanical objectivity. The new creed of objectivity permeated every aspect of science, from philosophical reflections on metaphysics and method to everyday techniques for making observations and images. In our account of the emergence of objectivity, we have focused on the latter in order to show how the airy-sounding abstractions of truth and objectivity had their concrete complement in the ways neurons,
snowflakes, skeletons, and myriad other objects were depicted on the pages of scientific atlases in the eighteenth and nineteenth centuries. Truth and objectivity were not merely the stuff of pious prefaces and after-dinner addresses at scientific meetings; to embrace one or the other could translate into the choice between an exquisitely colored, sharply outlined drawing and a blurred black-and-white photograph, or between the image of an idealized type sketched freehand and that of a particular individual meticulously traced from a projected image. It was choice freighted with ethical as well as epistemological implications.\(^8\)

The argument of the book explores the changes in the various beliefs surrounding the beliefs about objectivity, as well as the practices—practices in the sense of methods of producing representations—of science, that also changed.

The starting point for the story is the problem of picturing representative plants and animals, and depicting them for comprehensive works. Linnaeus is the ideal-typical instance of the “true-to-nature” approach. The practice of representation was to depict a leaf, for example, in a way that showed a perfect specimen and highlighted its distinctive features. The pictures one sees in old books of this kind—which are now art objects that appear on hotel walls—are more perfect than real life. But one may ask quite reasonably how else is one to represent a leaf-type? Picking a random leaf and depicting it without making especially sure that the differentiating features are not visible would not help the user of a botany manual. But a series of developments, mostly involving or leading to photography, made possible an alternative model, the model of

\(^8\) Daston and Galison, *Objectivity* (p. 195).
mechanical objectivity. Pictures like those commissioned by Linnaeus were produced by an illustrator with a pre-existing theory of what was being represented. Photographs were (with important qualifications) theory-free, and the ideal of a kind of representation in which the observer was entirely passive developed alongside and as a justification of mechanical practices of representation. Associated with these practices, justifying their superiority and unique value for science, were doctrines about the nature of observation, of science, and indeed a whole model of the scientific self as disciplined passive observer. The practice of daily recordings in one’s scientific notebooks reflected this new ideal.

The web of beliefs around these two ideals was complex and shifting; as new rationales were developed, new models were produced by new results and better technology of representation as well as new applications. Bound up with the idea of representation—bound up in the sense that beliefs of various kinds were used in support of them, were taken to be supported by them, or where thought to be challenged by them—were beliefs about all sorts of things in science. The various binary oppositions between objective and subjective, between science and art, between raw truth about nature and deeper truth about nature, between the scientist as passive observer and as expert instrument of observation employing trained judgment (such as the interpreter of X-rays), figured into the arguments of the partisans of these kinds of representation practices and conceptions of objectivity and of the nature of the scientific self that supported it.

Among the many revealing examples of the relation between the two kinds of objectivity was the work of a physicist named Arthur Worthington on the physics of splashes. He built an elaborate instrument to allow him to photograph the evolution of a splash, fraction of a second
by fraction of a second. At first he thought he could smooth out the irregularities and determine
the beautiful symmetry of the splash. Later he surrendered to the images revealed by high speed
photography, and embraced their lopsided character. The example shows the interdependence
between the active effort to reveal scientific truth, in this case through a particular technology
and practice of representation, and the ideal of removing the scientist from the frame, making the
recording of images into a passive, inert, component of the process of representation, purged of
every trace of the scientists self. But the point is that the two cannot be entirely separated. Even
passivity depends on an element of art.

The analysis they give of Worthington’s changes of mind is a representative (in a truth-
to-nature way) of the web of belief approach to intellectual change. From 1875 on, Worthington
had sought to

untangle the complex process of fluid flow into a systematic, visual classification.
Sometimes the rim thrown up by the droplet would close to form a bubble; in other
circumstances, the return wave would shoot a liquid jet high into the air. Ribs and arms,
bubbles and spouts– Worthington’s compendium of droplet images launched a branch of
fluid dynamics that continued more than a century later. For Worthington himself, the
subject had always been, as he endlessly repeated, a physical system marked by the
beauty of its perfect symmetry⁹.

The photographs were designed to capture a set of symmetric splash patterns: like so many

⁹ Daston and Galison, *Objectivity* (p. 11).
anatomists, crystallographers, botanists, and microscopists before him, [he] had set out to capture 
the world in its types and regularities— not a helter skelter assembly of peculiarities\(^\text{10}\). He 
published various idealized splash types based on his early, visual images of the splashes. But 
the results of the high-speed photographs didn’t fit: as he himself acknowledged, they “show 
greater irregularity than the drawings would have led one to expect”\(^\text{11}\).

The photographs and the symmetrical drawings conflicted. He had corrected the visual 
experience in the drawings to produce something he thought was true to the symmetrical nature 
of the splashes; but in the end he felt compelled to abandon the idea. He reflected on the method, 
acknowledged that the idealizations involved a problematic intrusion of the observer, error of 
judgment, guided by a theory. He abandoned the method he had used, and reflected on the 
methodological error that led to his original images, and on its persistence in his own mind— his 
inability to give up the theory even when confronted by his own photographs. And he embraced 
the idea of “an objective view” which would “provide knowledge of ‘real, as opposed to 
imaginary fluids’”\(^\text{12}\).

“Worthington’s conversion to the ‘objective view’”, Galison and Daston say, echoing the 
language of Kuhn”, is emblematic of a sea change in the observational sciences”\(^\text{13}\). And this is 
central to the main story of the book: “the creation of a new epistemic virtue— scientific 
objectivity— that drove scientists to re-write and re-image the guides that divide nature into its

\(^{10}\) Daston and Galison, *Objectivity* (p. 11).

\(^{11}\) Daston and Galison, *Objectivity* (p. 13).

\(^{12}\) Daston and Galison, *Objectivity* (p. 16).

\(^{13}\) Daston and Galison, *Objectivity* (p. 16).
fundamental objects. It is about the search for that new form of unprejudiced, unblinking blind
sight we call scientific objectivity”\textsuperscript{14}. But the use of the word conversion here is ironic: the
conversion in question was not a conversion of a Kuhnian kind between two incommensurable
concepts of objectivity, each of which belongs to a different conceptual scheme, but a revision of
a series of beliefs in the web of belief surrounding the expectation that Worthington had to the
effect that more accurate photographic images would confirm his “truth-to-nature” attempts to
provide a typology of symmetric splash patterns. First one belief in the web, and then its
justifications, up to and including the beliefs about the scientific self, had to be revised to make
sense of role of the scientist and the nature of objectivity in the face of the stubborn and
accumulating facts revealed by the photographs.

Kuhn vs. Davidson

What Galison and Daston do at great length, in their book, is to show what the ideas in the web
were, how they shifted, and what sorts of controversies erupted between the partisans of different
approaches, what was thought to be relevant and decisive in these disputes, and who won and
why. What is remarkable is that so much was said, and how much they, as historians, were able
to do to identify the relevant writings. The prolixity of the scientists made it possible for the
historian to reconstruct the issues in a way that traces the connections that make up the web of
belief, and thus to account for the changes as intelligible choices– not religious conversions, but
reconsiderations in which the web of belief needed to go through intelligible adjustments, with

\textsuperscript{14} Daston and Galison, \textit{Objectivity} (p. 16).
radical results for scientists’ image of themselves, of objectivity, and so forth.

How does this differ from Winch and Kuhn? Objectivity is a “concept” with a dead-end character. But because it is a term used in supporting truth claims, it is part of the web of belief that gets revised when something turns out not to be true, in this case that the splashes are accidentally irregular. Hutchinson et al could say that this just shows it is not a concept in the relevant sense, and concede that the web of belief account is sufficient in this case. Even a Winch or a Kuhn can concede that this kind of rational adjustment can go on. But this raises a question often raised in the discussion of Kuhn of whether there are any cases for which the web of belief account is not sufficient.

How might one settle such a question? In a classic paper which is basic to this discussion, Donald Davidson accused Kuhn of a performative contradiction: on the one hand Kuhn claims that past scientific viewpoints are incommensurable and cannot be translated into present terms. But in order to show this, he must explain the past viewpoint, and the differences—in present terms. Incommensurability, and, Davidson goes on to argue, the whole “conceptual schemes”, or what I have called the stack model, are a mistake. Sharrock and Read, in an earlier book on Kuhn to which There is No Such Thing refers at length, insist that this accusation is a mistake on Davidson’s part. The translations Kuhn provides into present terms are partial:


17 Hutchinson et al, There Is No Such Thing as a Social Science: In Defence of Peter Winch (2008).
something is missing. The missing something is what Kuhn thinks it is, namely something that has a dead-end, “no further justification” character. They liken this missing, untranslatable element to style: “something like a feel, an atmosphere, a style (of reasoning, of writing)”\textsuperscript{18}.

Both the truth-to-nature and the mechanical concepts of objectivity could be understood to have this dead-end, “no further justification” character. They are both connected to different ideas— and one might also think of these ideas as dead-end “concepts”— of the proper scientific persona, to different ideas of proper scientific practices, and so forth. Understood in this way they seem like a set of dead-end ideas linked in a “conceptual scheme” or “system”. Perhaps they are good examples of styles, to use Sharrock and Read’s term. But what Galison and Daston show is that things do not quite work like this in the case of objectivity. Mechanical objectivity people and truth-to-nature people understand each other perfectly well. They disagree about, and discuss, the related ideas. But they see their views as contradicting one another’s and as contradicting other kinds of facts which they may share. And the revision of their ideas proceeds in a rational way: expectations are confounded, and they reason about what views they need to revise.

The Argument Radicalized

\textsuperscript{18} Sharrock and Read, \textit{Kuhn: Philosopher of Scientific Revolution} (p. 168).
Hutchinson et al argue that these claims are there in Wittgenstein, taken over in toto in Winch, and correctly explicated by the account they give. The important common thread is this: there is no theory, philosophical or social, involved in this account. Even the notion of rule, which has usually been seen as central to Winch and Wittgenstein, is not a theory and not part of any sort of explanation. Rules are purely analogical rather than real explainers. One can dispense with the use of the term rule completely. Everything we need to construct the right account, to “count as” correctly, we can do on the basis of the understanding we already have as lay people. What counts as something is established by the participants. Consequently Winch doesn’t need an account of rules or rule-following. Nor do rules have the kind of relationship to understanding often claimed in the standard philosophical literature. They are not presupposed and transcendentally required for the identification of objects. All that work is done by our taken-for-granted understanding. But this understanding is not hermeneutic: there are no inferences made when we use our taken-for-granted understanding to “count as”—to put something in a category or describe it.

There seemingly can be an issue with understanding exotic people, who don’t share our taken-for-granted understandings. The Azande, who apparently believe (or believed, many years ago, when E. E. Evans-Pritchard’s famous ethnography was done) in witches, do not reason rationally about them, and are otherwise superficially puzzling. But—and here the argument against Davidson reaches its apogee—the same considerations apply equally to the beliefs and actions of exotic peoples. There is no explanatory problem. The puzzles disappear when we

19 Hutchinson et al, There Is No Such Thing as a Social Science: In Defence of Peter Winch (p. 43).
notice the relevant differences, and apply the right analogies rather than the wrong ones. If we treat the Azande as scientists making an error, we are applying the wrong ones: this is the source, and the sole source, of the puzzle. If we treat their beliefs as like religious beliefs, we are applying the right one, and there is no puzzle.

The lesson is this: to understand, we need only to look, not hypothesize. There is no problem of relativism: the entire 1970s literature on rationality and relativism 20 was simply misguided. What appeared to be a perverse and irrational belief system was nothing of the sort: it was merely religion. The task of understanding, and of social science as well as philosophy, is therapeutic. To the extent that there are puzzles, the solution is within us— to correct our own misguided thinking. In the case of the Azande, our problem is to correct our erroneous attribution of the scientific viewpoint to them; without this mistake, there is no problem. The bedrock of our understanding and the source of our taken-for-granted knowledge are our own practices 21 – and not anything like empathy 22. In the puzzling cases of exotic people we need to compare practices— once we identify the genuinely analogous practice there is no remaining puzzle 23. “To


22 Hutchinson et al, *There Is No Such Thing as a Social Science: In Defence of Peter Winch* (p. 120).

23 Hutchinson et al, *There Is No Such Thing as a Social Science: In Defence of Peter Winch* (p.122).
understand another’s claims, to understand the practices of another culture, one will need, for those claims and/or practices to be intelligible to one, to understand them in terms of things you might say or do”\textsuperscript{24}.

This account of understanding assures that there is no possibility of the kind of situation that relativism feeds on, and which says leads people to say such things as this:

What counts as rational in twenty-first century Western culture is internal to that culture: that of a scientifically rational (disenchanted, to coin Weber’s term) culture. The criteria to be fulfilled for a practice to be accorded the status of a rational practice are different in the culture of the twenty-first century West (advanced, late capitalist, yada yadda yaddda ...) and the culture of our (hypothetical) Amazonian hunter-gatherers.\textsuperscript{25}

Neither Wittgenstein nor Winch, they note, make such claims. And there is no closure of the sort that these claims trade on: “it is an illusion, and profoundly unWittgensteinian, to treat ‘language games’ and ‘communities’ as isolatable entities, independent of one another”\textsuperscript{26}. The understanding we get of others is direct, and does not need the aid of any sort of theory or even

\textsuperscript{24} Hutchinson et al, \textit{There Is No Such Thing as a Social Science: In Defence of Peter Winch} (p.120).

\textsuperscript{25} Hutchinson et al, \textit{There Is No Such Thing as a Social Science: In Defence of Peter Winch} (p. 120).

\textsuperscript{26} Hutchinson et al, \textit{There Is No Such Thing as a Social Science: In Defence of Peter Winch} (p. 120n12, emphasis in the original).
of notions like language games and communities as isolated entities. There is thus no problem
of relativism—just one of correct understanding and incorrect understanding, correct analogies
between practices, and those that produce the appearance of a disagreement that can only be
understood as relativistic.

Where does this leave us with concepts, which, as any reader of Winch would recall,
loom very large in his argument for the idea that sociology is a kind of subordinate branch of
philosophy? As we have seen, it is the dead-end character of concepts and conceptual schemes
that generates the problem of relativism. Hutchinson et al are insistent that Winch is not a
relativist, and that they are not relativists either. The argument here is equally radical, and
amounts to jettisoning the idea that understanding the concepts of another society is the key to
understanding them. Characteristically for the arguments in this book, the point is made in a
“therapeutic” way. The problem is simply an error produced by false philosophical prejudices.
The person in need of therapy (who sounds like the normal interpreter of Winch) thinks like this:

Isn’t the problem of understanding, though, that of bridging the gap between our concepts
and theirs? Doesn’t the comparison of two cultures show that people in them have
different concepts? And, if people have different concepts, and if understanding is
achieved through the use of concepts how can we understand them? If our understanding
is achieved through our concepts, and theirs through their concepts, then our
understanding and theirs are different, and radically so, since our concepts are variant.
Their concepts won’t fit with/into ours, so we will only be able to understand them
through our concepts, not through their indigenous ones: trying to absorb their concepts
to ours will only distort them. We cannot really grasp their concepts, any more than they can grasp ours.\textsuperscript{27}

The therapeutic response is that the reader has imposed a confused notion of understanding, and thinks, absurdly, that “the only way to understand what it is like is to be that person”\textsuperscript{28} This leads them to relativism.

The positive account of understanding we are given in place of the confused account, however, is limited. The relevant comparison is not between opinions (which can contradict one another), as in Davidson, but between practices, which cannot, and which cannot in themselves be translated, but can only be analogized more or less correctly— as in the case of the Zande witchcraft beliefs and religion. A kind of translation is possible, they concede: not “mechanical” translation, but a kind that is sensitive to context, “and to what, amongst our way of doing things, is the best comparison to that”\textsuperscript{29}. This argument revises the stack structure: rather than dead-ending at concepts, which are intelligible, we dead-end at practices, which are a different kind of fact. It avoids the performative contradiction problem, because understanding by practice-to-practice correspondence involves no explanation or reference to the web of belief. Practice, in

\footnotesize{\textsuperscript{27} Hutchinson et al, \textit{There Is No Such Thing as a Social Science: In Defence of Peter Winch} (p. 135).}

\footnotesize{\textsuperscript{28} Hutchinson et al, \textit{There Is No Such Thing as a Social Science: In Defence of Peter Winch} (p. 135).}

\footnotesize{\textsuperscript{29} Hutchinson et al, \textit{There Is No Such Thing as a Social Science: In Defence of Peter Winch} (p. 134).}
their hands, is an even more obscure notion than concept. We are not talking, as Galison and Daston do, of actual practices of the sort someone could be trained in, such as the practice of interpreting X-rays, but something like a form of life. The main property of practices in the sense of Hutchinson et al is that they are unanalyzable and beyond explanation. They can only be compared. And there seem to be no rules or procedures for comparison: comparisons are either correct or not.

The line of argument presented by Hutchinson, Read, and Sharrock is certainly radical and original. They reject explanation. They reject empathy. They insist that all sociology can do in the end is describe, and that this is sufficient and a substitute for explanation, even in the face of radically different cultures or radically different ideas. The craving for explanation is a disease to be therapeutically corrected, by showing that it arises from mistaken philosophical prejudices. They nevertheless allow for “puzzles”, for example about other cultures, but treat the resolution of these puzzles as a matter of therapy, and that all that is needed post-therapy is to look more carefully. They claim that analogies between practices are the solution to such puzzles. They insist that their account does not depend on any kind of theory, philosophical or sociological. They argue that they are giving the correct Wittgensteinian line on these topics, and that Winch, too, adhered to this line.

The Form of the Argument

The claim that their argument is the argument of Winch and Wittgenstein is supported in an odd way—not by a compelling example from actual social science, or by exegesis of Winch and
Wittgenstein, but by attacking the critics of Winch, especially those who think that Winch got Wittgenstein’s implications for social science wrong, or think that Winch and Wittgenstein had theoretical ideas, especially social theoretical ideas. A large part of the discussion is given over to Theodore Schatzki’s presentation of a non-Winchian Wittgensteinian approach to the social sciences, yet even here the texts vanish. Indeed, there are few paginated citations to texts at all, even to Wittgenstein and Winch. There is no response to Schatzki’s lengthy and specific list of citations of places in which Wittgenstein contradicts Winch and their own interpretation. They criticize unnamed others for failing to attend to Winch’s later writings, but they ignore the many concessions he makes in these writings to the idea of a social science, concessions which undermine and indeed directly contradict their own claims. The main targets of the book, Rorty, Davidson, Quine, and Norman Malcolm, are never quoted or directly cited, nor are their actual arguments analyzed. Even Schatzki’s article, the attack on which occupies more than a sixth of the book, is never summarized, explained, or addressed. Instead it is treated as representing attitudes and presuppositions that are in conflict with the correct view of Wittgenstein, namely

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30 Hutchinson et al, *There Is No Such Thing as a Social Science: In Defence of Peter Winch* (pp. 42-67).

There is perhaps a justification for an approach that itself consists largely of attitudinizing. Hutchison et al are, it might be claimed, supplying an alternative interpretation for many of the philosophical claims in question. The brunt of the argument depends on a claim about Winch’s and Wittgenstein’s purposes, which, they claim, were not to offer a philosophical theory but merely to offer a kind of therapy. In the case of Winch, this has the effect of enabling them to argue that Winch can escape the many criticisms of his theoretical terms—it is a kind of generic defense to the effect that he did not mean what he said, because his purposes were different from what the interpreters thought they were. And the defense can be extended to Wittgenstein.

In mounting this defense the authors are explicitly indebted to a school of Wittgenstein interpretation (the New Wittgenstein) which developed around the interpretation of the *Tractatus*. The New Wittgensteinians took themselves to be strictly interpreting Wittgenstein by insisting on the importance of his saying in the text of the book that the propositions in the *Tractatus* were meaningless. This style of interpretation has been (not very successfully and with much less agreement) extended to the *Philosophical Investigations*, in the form of taking seriously the idea that philosophy is therapy, but therapy with a narrow aim: to correct philosophical errors. The strange result of this interpretation is to validate conventional philosophy, and deradicalize Wittgenstein: the later Wittgenstein again becomes a more or less conventional philosopher like the interpreters themselves. The therapy, in the narrow sense, consists of conventional *a priori* pronouncements, but derived from the Wittgenstein texts, and

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32 For the most part attitudes that Schatzki never had.
presented as “reminders.” The a priori dogma that Hutchison et al authors are most intent on enforcing involves the problem of reasons and causes, which was central to the apriorism of the Winch of 1958.

Reasons and Causes Revisited

The issue of reasons and causes is the place where the peculiarity of this interpretation becomes evident. Winch, among others, originally slavishly relied on the claim that reasons cannot be causes. The basic argument was that intentional explanations were not causal explanations because the relations between intentions and the intended outcomes of intentional acts were not causal; cause and effect were not independent, but were logically or conceptually related. Going to the store in order to buy beer describes an intentional action. The reasons for an action are part of the identification and the meaning of an action as an action, and not a cause or part of the cause. To go to the store to buy milk would not merely be a different outcome of the cause but a different action. The relation between reason and act was thus “internal”. There simply was no place for a cause here: once one had identified the intentional action as an action, it was already accounted for. It was an embarrassment that in the case of this dogma, ordinary language philosophy was compelled to reject ordinary language—the supposed authority for the claims of ordinary language philosophy. People did in fact, routinely and untheoretically, speak of causes of actions.

Davidson broke up the reasons and causes orthodoxy with the following argument: we can make sense of ordinary talk about intentions as the causes of actions by a simple formula. A
belief that some act would lead to an outcome, i.e., a reason of a particular kind, and a pro-
attitude toward that outcome, together constituted a “cause” of an action. The mistake made by
the promoters of the idea that reasons and causes excluded one another, he thought, was the idea
that there had to be laws relating reasons to outcomes for there to be causal relations. The case of
action explanation, however, was a case of causes without laws. This argument largely dissolved
the problem, and, as a bonus, fit with ordinary language. The Winch of 1990 seems to concede
Davidson’s point. He says

I found myself at times denying that human behavior can be understood in causal terms,
when I should have been saying that our understanding of human behavior is not
elucidated by anything like the account given of ‘cause’ by Hume (and Mill).³³

The world changed, as Winch well knew. In the 1950s, it made sense to think of ordinary
intentional action and psychological explanations as alternative redescriptions of action, and
conclude that the psychological redescription was crude, uninformative, and not based on much
actual knowledge. By the time he wrote the preface, a large cognitive science literature on
intention had developed, brain scans were made of intentional action, and there were many odd
results that showed that our own descriptions of our intentions were often false. Davidson was
comfortable with this change: he even made the comment, much later, that his model fit with the
fact that intentional action involved both the amygdala and the hippocampus, the place of desire

and belief respectively\textsuperscript{34}.

For Hutchinson et al the old orthodoxy stands: the only thing one needs or can get in the way of an account of meaningful action is a correct description. The reasoning comes more or less directly from Anscombe’s \textit{Intention} of 1957\textsuperscript{35}, a book that loomed large for Winch and in Winch’s time. What Anscombe says about this topic is odd, however. All redescriptions that, as she says, swallow up the original description by virtue of this imply the original description: the original description is contained in the redescription. “He went to the store in a red Fiat Cinquecento” implies “he went to the store.” The longer sentence swallowed the shorter one\textsuperscript{36}. But only some descriptions and redescriptions of action are surrogates for explanation: he went to the store to buy beer. The cause account explains the difference between the two. To correctly say he went to the store to get beer means that he has a belief that he could get beer at the store and wants (or has a “pro-attitude”) to beer. It is very difficult to imagine a story about why the one description is a surrogate for explanation and the other is not without appealing to some notion of causality. Anscombe tries—she thinks that descriptions which contain phrases like “in order to” are surrogates for cause without being causes. But this begs the question: why do these phrases work as surrogates? Davidson’s answer would be that they presuppose the causal structure he outlines. A later literature suggests that the descriptions that serve as explanations should be read as condensed explanations: the “for” in “went to the store for beer” condenses the


\textsuperscript{36}Anscombe, \textit{Intention}, (p.46, para. 26).
facts about pro-attitudes and beliefs without which the “for” would make no sense.\textsuperscript{37} Given this, and Winch’s own concession to the effect that it was appropriate to talk about the causes of actions in a non-Humean sense, why would Hutchinson et al hang on to the idea that description can serve as a surrogate for explanation? Perhaps it is this. Winch, in the 1990 preface to the second edition, discusses “the understanding we already have is expressed in the concepts which constitute the form of the subject matter we are concerned with”\textsuperscript{38}. The idea is that concepts, which contain knowledge, explain, or better yet, identify objects directly; additional explanation is made unnecessary because something special and explanation-like is contained in the concepts. Hutchinson et al elaborate on this idea at length. The sole relevant descriptions of what people say and do are taken from the taken-for-granted background knowledge that all of us have as social agents in a given social setting. There are no experts about ordinary social description: we already know how to do it. Nor is there explanation. Once one has correctly described an action in the appropriate way, one which fits the background knowledge of others, the action is no longer puzzling. We have a precise description, and one which requires nothing additional and allows for nothing additional. Describing replaces explanation, and correct descriptions are an end point beyond which no further interpretation is necessary, nor does it have a point. So correct description precludes the need for interpretation.

This allows them to argue that describing is just describing, not describing in terms of a

\textsuperscript{37} This account of Wittgenstein is discussed in David Henderson, “Wittgenstein’s Descriptivist Approach to Understanding: Is There a Place for Explanation in Interpretive Accounts”, \textit{Dialectica} 42(2), 1988, pp. 105-115.

\textsuperscript{38} Peter Winch, “Preface to the Second Edition” (p. x).
theory, and to treat the idea that explanation rather than description is primary as a groundless prejudice that needs to be therapeutically removed\textsuperscript{39}. Description is primary, and describing does not involve anything more than fitting a type to the act. It is on the surface. There are no hidden causes and thus there is no need for causal knowledge. Applying the types does not involve interpretation, any more than saying that a dog is a dog is a matter of interpretation. There is no need or point to theory of any sort here. Once described, the acts or beliefs are not puzzling, and need no explaining or interpretation. The only issue is what counts as something. Action is action only under a description. The same feature of description that makes something an action is sufficient to account for it. For Hutchison et al, then, the problem of whether a given description was a surrogate for an explanation, Anscombe’s problem, would not even arise: the knowledge relevant to distinguishing what concepts preclude explanation is already there in the concept itself. We would have understanding without theory, without cause, and without social science.

Who wins this argument? It depends on what one thinks falls into the category of non-expert, taken-for-granted background on which Hutchinson et al rely, and what status the background, which they call practices, has. If the content of the background the sort of thing that is neither true nor false it is puzzling to thing that it can provide a surrogate for causal explanation of action. It makes more sense to say that we acquire, by virtue of our common life and common experiences, actual explanatory knowledge about the people around us which enables us to describe in ways that condense this knowledge into descriptions. The child’s acquisition of the ability to make contrafactual inferences about what others would have done if

\textsuperscript{39} Hutchinson et al, \textit{There Is No Such Thing as a Social Science: In Defence of Peter Winch} (p. 39).
they did not know something the child knew—the false belief problem—is a major topic of study in the cognitive development of children, much discussed in the philosophy of mind and even among Habermasians. Saying that a dog is a dog is a poor model for this kind of knowledge. This is causal knowledge, knowledge about the consequences of people’s false beliefs for action, not a matter of concept acquisition. Hutchinson et al either have to sweep this causal knowledge into the category of practice, which begs the question of whether causal knowledge is necessary, or reject the idea that it is causal, which is a hard mountain to climb.

Relativism

But there is more to the problem of the explanatory role of false beliefs that has nothing to do with children. The web of belief approach is non-relativistic and concerned with the sorts of things that can be true or false. We translate the beliefs of exotic societies, and we also translate the beliefs which explain their actions. These beliefs (such as “there are witches who direct events and human actions”) may be false, but they are still the beliefs of the people who are acting and explain their action. If we are not concerned with beliefs that can be true or false, if we say that the concept of witchcraft constitutes the form of the subject matter we are concerned with, as the early Winch seems to suggest, it seems that this account of description as a surrogate for explanation forces us to accept witchcraft, or descriptions like “the witch did it” as a legitimate surrogate for explanation. Hutchinson et al are of course well aware of this problem. And they understand that this reasoning leads to relativism. This is why they are so eager to insist that it is an error to think of Zande talk about witches as analogous to our ordinary action
explanations or to, as they say, science. And although they insist that Winch had a therapeutic conception of philosophy and did not mean what he said, they also understand that the solution to the problem is to get rid of the theoretical machinery that gets Winch into trouble over this issue, namely his account of concepts. This is why they replace talk about concepts with talk about practices.

Winch adds the following to his comment about “the understanding we already have” being “expressed in the concepts which constitute the form of the subject matter”: “These concepts on the other hand also express certain aspects of the life characteristic of those who apply them.”°40 “Understanding” seems to fall on the true and false side; constituting concepts by definition does not. The “certain aspects” which are expressed correspond to the mysterious notion of practice. Anscombe herself said that “the existence of human concepts does of course depend on the existence of a great variety of human linguistic practices.”°41 Hutchinson et al think they can get around the problem of the falsity of beliefs in witches by moving to the level of practices. All that is needed to have direct understanding of the Zande is to have the correct analogy to the practice underlying their talk about witches. But “practices” is a theoretical idea. The appeal to these ideas undermines the insistence that there is no need for theoretical concepts. And Winch does not help the anti-theoretical “no such thing as a social science” cause when he concedes that “there are many cases in which historians, anthropologists or linguists give well

°40 Winch, “Preface to the Second Edition” (p. x)

founded explanations of the existence of this or that practice. Why ever not?**

Daston and Galison’s book is a good example of historians giving a well-founded explanation of a practice: the practice of objective representation that is dominant in present science, but which once was not. They explain by showing how the web of belief changed as a result of new arguments, new conceptions of the morality of scientific observation, and new techniques of depicting images that produced results that conflicted with older ideas. And they recount the passionate arguments and struggles within science over these ideas. When they are done, we have a quite well-developed picture of the web of belief surrounding these ideas. The web of belief account says that by developing this picture, and only by developing this picture, can we make sense of odd views, because this is what it takes to see them as intelligible responses to different situations, as errors, and so forth. There is no pretense that the sayings and beliefs of the scientists of the past can be understood “directly”. Understanding what they believed and showing why these odd beliefs were not only plausible but conventional requires a good bit of explanation. “Translation” is a good model for this, because translations need to preserve the structure of implications—relations of inference between claims—between languages, and this provides a standard of correctness. To translate by using a word in language A that implies something different in language B is a bad translation. It is important to know what the differences are—why an American using “friend” is going to mean something different than a German using Freund. And explaining this may require doing a lot of explaining of things in the target language and the social world in which the target language is used—such things as

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related beliefs, what it is that friends are supposed to do, what a friend does not do, and so forth. The differences ramify a long way into the web of belief and practice of which the term is a part.

There is a Wittgensteinian saying that the limits of our language are the limits of our world, and the task of translation has limits as well. This, indeed, is Davidson’s point in his essay on conceptual schemes: if we could not translate, we would not even know that the thing we were failing to translate was a language. This is the reasoning Hutchinson et al, and before them Sharrock and Read, are adamant about rejecting. But why they are so adamant is far from clear. Their alternative is understandable only in its broadest outline. They do think this: there is something untranslatable that is important. But at the same time they do not want to be “relativists”. They think that the way to avoid relativism is to deny that the objects of comparison are closed units. They realize that “concepts” is a problematic term because the dead-end, unanalyzable character of concepts leads to conceptual relativism. They want no role for explanation, so that the role of philosophy or sociology must be therapeutic. So they have to claim that the important thing that eludes translation is directly accessible if only we attend carefully enough to the details.

Hutchinson et al are convinced that the answer is in practices, the things that underlie concepts and on which concepts depend. And they think they have something better to offer with the notion of analogies between practices. Therapy gets us to the point where we can apply the right analogy. Once our mistaken prejudices—such as the bias of thinking of Zande witchcraft thinking as analogous to science—have been corrected—the material to which we apply it is then accessible directly. Practices are at the bottom in the stack model. They can’t be justified or
explained. They can, however, be described, but only in one way—as analogous to other practices. Thus the goal of understanding, here as with action, is identification, or description, and not explanation.

How would one know that one had the right analogy, and thus the right description? One answer would be that the way to get it correct is exemplified by Daston and Galison: translate in a way that exhibits the web of belief and the differences in background belief, practices in the sense of actual procedures or methods of producing facts, and data, that lead to differences in opinion. Daston and Galison are a model of attending to the details, and they have reasons for attending to the details they attend to: the web of belief model allows them to say what is relevant and why, and shows them what they need to explain in order to make sense of the different opinions about objectivity they are explaining. What “therapy” for the correction of interpretive prejudices, one might ask, could possibly be better than this one?

Practices and the Universal

Hutchinson et al would say that this reply misses their point. And indeed they do have a point, but it is deeply confused. The confusion comes from the notion of practices. One way of thinking about practices is this: practices are just like concepts, but form the unintelligible tacit foundation for concepts. Concepts are, by definition, intelligible. Practices are not intelligible: they are sub-conceptual facts of a mysterious kind which allow for intelligibility. Concepts differ and are not properly commensurable because they rest on different linguistic practices. This is the notion of practices from which Hutchinson et al are arguing. The Azande present exactly the kind of case
where merely translating is not enough. It is possible to translate them as holding false beliefs, and show how these false beliefs are located in a web of beliefs that makes them plausible to the Azande. But this leads inexorably to the conclusion that the Azande are irrational, because the beliefs are not only false but inconsistent. To understand the Azande we need something more and better. Winch says essentially this in relation to the Azande: he explains (yes, explains, not describes) the oddities of their thinking about witches in terms of differences not with our concepts, or with other beliefs that we hold, but with “. . . our intellectual habits . . .”. 43 This is a telling phrase, and a telling confession. What Winch is conceding is that there is a level of explanation beyond rules, concepts, and the like. Hutchinson et al think that practices is a level beyond rules concepts and the like that provides the solution to the puzzle, and that at this level we have some sort of direct access.

There is a reason for thinking that we have some kind of direct access that goes beyond or is different from the kind of understanding that comes from translation and the web of belief. The Azande, with their different intellectual habits, are not inaccessible to us. Evans-Pritchard had no difficult understanding their actual practices and sayings in relation to witchcraft. The practices and sayings are simply inexplicable in terms of concepts—like “Witch” and rules, such as the criteria for identifying witches. These lead to a contradiction. And this would be fatal to the Winchean project of substituting explication for explanation. So it is not surprising that Winch took this example very seriously. Hutchinson et al think that by moving to the level of practice, a term which presumably contains what Winch has in mind when he speaks of habits,

we can still have access to the Azande, still understand them. We merely need to find the right analogy, and this is something that is unproblematic if we are therapeutically freed of the temptation to use the wrong analogies, in this case the analogy to the practice of scientific thinking.

Direct Understanding

An important, though obscure, thought thus lies behind this talk of therapy: that there must be an unproblematic end point to understanding, the point at which understanding is achieved and additional explanation is no longer relevant or necessary. Even web of belief accounts need to concede this: what they seek is intelligibility. Perhaps this is also the thought behind Hutchinson et al’s repeated rejections of relativism: real understanding should be final and not be conditional, or relative to a viewpoint, a conceptual scheme, or some other arbitrary starting point.

We do understand the Azande, even if we cannot articulate this understanding in the way that the web of belief model requires. Winch’s use of “habits” instead of practices points in a direction Hutchinson et al do not want to go—to explanation rather than description. Habit is a quintessentially causal, psychological phenomenon. With habits we are back in the natural, causal world—the world of science. Habits are learned, they have some sort of material reality. Winch, in one of his later writings, gives the example of the child who is part of the community but fails to learn the community language for reasons relating to the development of the child’s brain, or to other social or psychological causes. When our explanations bottom out in habits,
they bottom out not in descriptions, but in substantive explanations, as facts in the causal world. But habits are not intelligible. In this way of thinking, we overcome relativism, but indirectly: by moving to a more fundamental explanatory level than concepts, in terms of which we can explain concepts. But this can’t be answer to the question of why the Azande are intelligible to us. Winch never worked out the conflict between his appeal to habits and the rest of his discussion of Zande witchcraft, which is in terms of concepts. Hutchinson et al don’t see a conflict, because they avoid talk about concepts.

Intelligibility is, so to speak, a top predator in the jungle of “concept” concepts: intelligibility doesn’t derive from anything; if there are multiple intelligibilities, there is no higher notion to derive or criticize them from; one has reached the end when one invokes intelligibility. If we say that there are different concepts (or criteria) of intelligibility, we embrace relativism at a fundamental level—the level of intelligibility. This was the muddle that Winch could not extricate himself from. Both the web of belief approach and the alternative given by Hutchinson et al face this problem, but at different points. For the web of belief approach, when combined with Davidson’s account of action explanation, the problem is that for a belief to be a belief, and thus to play the role of a belief in the causal explanation of action, it must be intelligible. And this means direct or final, in some sense. Hutchinson et al attempt to solve the problem of explanation by replacing it with “description” which stays on the surface. But descriptions need to make action intelligible directly to serve as surrogates for explanation.

Wittgenstein avoids this problem. He never uses language like this, and does not need to. He has an account of direct understanding. But it comes from a place other than the alternative path of understanding through practice. It nevertheless is concerned with the surface.
Wittgenstein instead speaks of “the natural expression of an intention”\textsuperscript{44} as part of natural history. Winch quotes Rush Rhees to the effect that “‘we see that we understand one another, without noticing whether our reactions tally or not. Because we agree in our reactions, it is possible for me to tell you something, and it is possible for you to teach me something’”\textsuperscript{45}. Though Rhees does not say it, the reactions are natural signs, like the expressions of pain, the stuff that empathy works with. This is a pre-conceptual sense of “intelligibility”. Reactions are more basic than criteria. They are part of the natural world. Anscombe treats these passages as an unfortunate slip\textsuperscript{46}. But they were no slip at all: the language of natural reactions and the like pervades the texts, as David Rubinstein has shown.\textsuperscript{47}

Hutchinson et al are overtly hostile to the notion of empathy— the core of the idea of natural signs or “reactions” that “tally”. For them the problem of description is a matter of applying concepts correctly: of identification, or showing that something “counts as” an instance of a concept or a description. This (unmistakably “theoretical”) language does not avoid \textit{a priori} philosophy—it places them in the middle of the most vexing and theoretical


\textsuperscript{45} Winch, \textit{The Idea of a Social Science and Its Relation to Philosophy} (p. 85, emphasis in the original).

\textsuperscript{46} Anscombe, \textit{Intention}, (p.5, para. 2-3).

philosophical discussions of the present, discussions about normativity and rule-following. A coherent formulation of the idea that there is a more primal level of human connection and understanding unmediated by concepts or practices—the very idea they are seeking as a solution to the craving for explanation—eludes them, despite the fact that it is there in plain sight in Wittgenstein, in the form of the idea of natural expressions, reactions, and so forth. They remain trapped in the maze of theoretical concepts they are trying to fight their way out of. The seriousness of the effort to escape makes this a rewarding and significant book. But its polemics are misdirected. The real targets should have been the idealist readings of Wittgenstein given by Winch, Anscombe, and their peers, which keep them trapped and send them in the direction of the concept of practice.

In contrast, Daston and Galison, as good historians, do exactly what their problem requires: tell us what we need for us to be able to directly understand the scientists they describe. This is as much understanding as we can have. They do not appeal to habits of thought and the like, but they do something similar. They depict the actual workings of the scientists who come to their odd beliefs about objectivity in a way that makes their habits of thinking seem natural and plausible. They strive for empathy—which is to say direct understanding.

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