Relativism is widely perceived incorrectly as an anti-scientific position. It is important to remember that there are many forms of relativism and that some of them involve a positive view of the natural sciences and may even be presented as expressions of a scientific orientation. A high regard for science may not merely be consistent with relativism but the inspiration for it. Consider moral and ethical relativism: the fact that no scientific basis exists for setting one set of moral or ethical doctrines above others is often reckoned a powerful argument in its favour. Ontological relativism may be advocated on the same grounds: what is real may neither be observed directly nor reliably deduced from what is apparent, and so it can be argued that no scientific basis exists for any particular ontology. It is intriguing to note as well that some of the stronger forms of rational choice theory arguably imply relativism. If the beliefs of human beings are all rationally held, then the manifest variability of those beliefs, and the evident conflict between many of them, – even in the context of the natural sciences, and among scientists confronted with the same evidence, – might be taken to be an argument for epistemological relativism. And of course strong empiricism points us in the same direction. Bodies of belief about empirical phenomena are sets of generalisations about groups or clusters of those phenomena. But the phenomena themselves do not tell us how we must group them. Many different groupings are possible, as the basis of different conceptual schemes or classification
systems. And this supports a relativistic account of conceptual schemes and hence of the different systems of verbal knowledge of empirical phenomena that can be established on the basis of them.

‘Edinburgh relativism’, as it is now called, nicely exemplifies how a significant form of relativism may conflict with the dominant stereotype. Initially developed as a relativistic analysis of the natural sciences and scientific knowledge, it was nonetheless inspired by an unambiguously positive orientation to them. With this in mind, and aware also of the special focus of this volume on the history of relativism in relation to science, I want to begin by recalling the development and reception of this form of relativism. Be warned, however, that what I have to say should not be relied upon as history. Given my own involvement in a story that began over forty years ago I can scarcely claim to be a reliable and dispassionate reporter of events many of which are now too distant for me even to recall in any detail.

In the late 1960s, I joined a small group of scientifically trained staff at the newly founded Science Studies Unit at Edinburgh University. The remit of the Unit was to develop courses designed to broaden the education of natural science students and increase their awareness of the wider impact and significance of science and scientific research. But alongside this a programme of research soon emerged, with the aim of understanding the knowledge and activity of the sciences themselves, that is, their content and substance and why that content and substance counted as knowledge. As a sociologist, I myself looked to the literature of the social sciences for material relevant to this aim, just as colleagues in the Unit looked to literatures in history and philosophy. And it slowly became clear that whilst existing social-scientific studies of knowledge included little systematic work of the requisite kind on the natural sciences, they did provide a tacitly accepted perspective that could be tried out as a basis for such work. Let me summarise the key points of this perspective.

1. Knowledge was understood as the possession of a collective, not an individual. Durkheim had famously contrasted collective representations with individual perceptions and beliefs, and identified the conversion of the latter into the former as a process functionally indispensable to the life of human beings. Other writers had focused more on know-how than know-what and on knowl-
edge as methods or competences, but again it was only by virtue of being shared and having standing in a collective that these could be counted as knowledge.

2. Understood in this way, knowledge had been identified as part of the shared cultural tradition of a collective. Looking across collectives, what was given epistemic standing was clearly liable to vary and even to conflict. But in any given collective knowledge was in the first instance received from the ancestors and accorded epistemic standing on their authority; and whilst it did of course change in the course of use, such change was a matter of the adaptation of a shared epistemic inheritance not of the piecemeal elimination and/or replacement of those separate components of it that proved untrue or invalid.

3. The distribution and use of knowledge in collectives had been recognised as socially structured. In particular, in societies with extensive division of labour and institutional differentiation, different forms of knowledge and the specialists who carried them had standing in different domains, and the results of their application therein were evaluated in different ways.

4. Accounts of the use and application of knowledge wherein its ‘implications’ were assumed to inhere in the knowledge itself had been recognised as untenable, in large parts of micro-sociology [including ethnomethodology] and anthropology. In these fields the use of knowledge was being studied as contingent social action. The links people made between their knowledge and the actions ‘implied’ by it were increasingly being transformed into foci of empirical curiosity.

One way of understanding ‘Edinburgh relativism’ is as something which emerged along with our growing awareness that these general points could be extended to the knowledge of the natural sciences without any modification or supplementation. The general account applied. And nothing extra needed to be added to it in order to account for the special epistemic standing of science, its putative truth, its alleged use of validation procedures specific to it alone, its character as ‘genuine’ knowledge rather than something merely taken to be such: all of this could be encompassed by the general account as it stood and had its paral-
lels in discourse surrounding other forms of knowledge, including the no-longer-valid knowledge that littered the history of science itself. Of course, this awareness did not emerge in an instant; it developed through an extended period of research and reflection involving examination of case study accounts of scientific knowledge from what we took to be a naturalistic, scientific perspective. And neither did it take on a final form and stop developing; it became an identifiable ‘it’ only when it was eventually rationalised by David Bloor as a continuing research programme that aimed to understand all knowledge, including scientific knowledge, in the same way, and to account for its acceptance and credibility naturalistically and causally, without regard for the truth or falsity of the knowledge in any context-independent sense.¹ This relativist programme clearly articulated the naturalistic, scientific orientation which was its incompletely rationalised starting point and identified that starting point as itself a potential target of its own investigations. The programme was thus explicitly reflexive as relativism requires. Indeed, having extended the scientific project to science, it welcomed its further extension to itself as a part of the endless task of bringing that project to completion.²

It was probably just as well, as awareness of this programme grew in the 1970s, that we were working in an autonomous academic unit, sustained by our teaching activities and the interest and enthusiasm of large numbers of natural science students. For we quickly became aware that we were not supposed to be doing what we had set out to do, that relativism was an anathema, and that science had to be accounted for by reference to reason and experience not ancestry and authority.³ It can be

² It is perhaps worth bearing in mind the context. It did not seem especially subversive, working in a science faculty, teaching science students, to omit explicit expressions of allegiance to science or accounts of what made it so special: there was no demand, no felt loss of authority, awareness that in practice its knowledge and expertise were valued even after the ups and down of the 1960s. Science wasn’t an institution in decline like some of the churches; it was growing and thriving both generally and in academic contexts wherein the bureaucratic wrecking squads had yet to intrude. Unlike some fields, the sciences had no strong need to legitimate themselves and find ways of bolstering their authority.
³ Contrary to what at one point was widely claimed by commentators and critics indifferent to what we had set down in print, ‘Edinburgh relativism’ nowhere denies the existence of an external world, nor even its irrelevance to what humans claim to know of it. It actually acknowledges the existence and indeed the relevance of that world and merely denies that the world is a sufficient basis for understanding any verbal account of it or what
hard to grasp today just how strongly science was sacralised at that time, and correspondingly difficult to understand why so many academics reacted to our work in knee-jerk fashion, as if it was an attack on science, and thought nothing of hurling abuse at fantastical misrepresentations of what we were saying – apparently on the assumption that something so self evidently objectionable needed no serious examination and all that mattered was that audiences were warned against it somehow or other. Things are different today of course, but these initial misrepresentations need to be mentioned, if only because they are still occasionally cited by commentators, out of indolence now perhaps, as much as hostility.4

In any event, considerable labour had to be expended at the time not just on defending our views but on clarifying them, and we found ourselves both referring back to our original sources of inspiration and ransacking other literatures for helpful materials. I vaguely recall appeals to the physiology of the eye and its ability to enrich our understanding of blurred images as readily as focused ones, and to the innumerable different maps that may all alike be accounted maps of the same terrain, in efforts to convey elements of our position. And I recall as well the shared pleasure of finding salient supportive arguments in empiricist and inductivist philosophy, and no less salient case studies in the historical literature and in ethnomethodology. But probably the most important of all the work we drew upon initially was that of Thomas Kuhn, which had been invaluable in the development of our ideas almost from the start.

Even today many people look back on Kuhn as a relativist of a strongly idealist sort, whose primary philosophical concern was with the problem of theory choice in science, and the revolutions wherein one scientific theory or paradigm is replaced with another. We read him differently. The paradigms famously referred to in “The Structure of Scientific Revolutions”5 are not theories: they are exemplary achievements – particular solved problems which a community of scientists has accepted as the au-

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4 Of course there was also a number of thoughtful and well-informed critics: Martin Hollis, Steven Lukes and Donald Campbell come immediately to mind, but there were several others and, albeit via disagreement, we learned much from them.

5 Kuhn 1962.
uthoritative basis for the solution of further problems in their field. In his later work Kuhn refers to them as exemplars. For Kuhn exemplars are the basic units in which scientific knowledge is transmitted and acquired, and the extension of that knowledge involves treating unresolved problems as analogous to existing solved problems and thereby solving them in turn. A process of case-to-case modelling is involved, not the process of secure inference from general to particular of the sort that accounts of scientific theories and their logical implications had long encouraged us to imagine.⁶

Once Kuhn’s notion of paradigm is understood as denoting an exemplary achievement his account of scientific knowledge is made visible as entirely consistent with a sociological understanding of knowledge in general, something that quickly becomes apparent if it is compared with the four key points of such an understanding I set out earlier. In particular, Kuhn’s account can then be seen to be consistent with the final point, which has proved to be at once the most difficult and fecund of the four. The analogies through which the knowledge incarnate in exemplars is extended and generalised are not logically compelling. Even the simplest of such analogies, involving direct intuitions of empirical similarity, do not permit indefeasible inferences to further instances; empirical similarity is not a transitive relation. Accounts of exemplars being extended in processes of modelling and analogy are accounts of contingent actions, of people treating things as the same where they could instead have treated them as different. And accounts of people acting in agreement as they extend knowledge from exemplars are accounts of contingent social actions, coordinated by collective [epistemic] authority.⁷

⁶ This view led some philosophers to revise their understanding of what theories were, in order to save the notion, as it were. Formalisms were constructed wherein sets of exemplars had to be regarded as essential constituents of theories, part of the very notion of what a theory was. And whilst this work may be thought unduly conservative, it was very impressive notwithstanding. But although it did make a mark in philosophy, in Germany especially, it seems never to have been given the recognition it deserved. (Sneed, 1971, Belzer, Moulines and Sneed, 1987).

⁷ Kuhn provided many fine examples of these processes at work in science. But he also offered a useful argument for an understanding of science as contingent action that engages effectively with those who persist in seeing it as involving the validation of theory by observation or measurement. Measurements, he points out, are never in perfect accord with theoretical predictions (Kuhn 1961). There is invariably a difference between the one and the other, so that if evidence is to be taken as supporting a theory it must we adjudged "close
In ‘Second Thoughts on Paradigms’, Kuhn spoke of the “truism that anything is similar to, and also different from, anything else” but one of his prime concerns throughout both his philosophical and historical work was to encourage us to attend to this supposed truism. Evidently, it was a much neglected truism at the time, and we remain less curious than Kuhn’s remark suggests about why we count one thing the same as another. Certainly, in sociology awareness of this question in various guises has long existed and proved productive, but for the most part within the confines of sociological theory and micro-sociology, despite its clear relevance to the whole of the field. And in philosophy, for all that major figures like Wittgenstein and Goodman had highlighted the problem and it was recognised that empirical sameness was an intransitive relation, researchers have often preferred to take no account in their practice of what they have been willing to accede to in the abstract. Rationalist philosophers in particular have preferred an extensional account of knowledge, one where knowledge clearly illuminates an endless path forward from one correct application to another for its users to follow, to a finitist account wherein users themselves had to hack out the path as they went along.

It is worth going into a little more detail on the problem at issue here, even though the key points are not to my knowledge controversial. How, given Kuhn’s ‘truism’, is one thing to be identified as the same as rather than different from another, as we must if knowledge is to be applied? Given the indefinite complexity of the material world, any two objects are going to be non-identical even if we label them as of the same kind. But if we extend the label from one object to another, and from that one to yet another, and so on, every time two non-identical objects strike us as ‘very like’ or ‘near enough the same’ or whatever – we shall be liable to end up referring to anything and everything as of the same kind. Many references based on ‘small’ differences will cumulatively bridge the

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gap between ‘vastly different’ objects; they will take our references every which way, and render our label useless for all practical purposes. For our convenience we may of course further identify our non-identical objects as ‘the same’ in some respect or property, but then the transitivity problem recurs. Using a same-colour relation, for example, we may start with an exemplary red object and end applying ‘red’ to a yellow one. Indeed a chain of sameness relations between objects *empirically indistinguishable* in colour could begin with a red one and end with a yellow one. Moreover, the relation of [empirical] self-sameness of an object over time is no different from the relation between two objects separated spatially. Empirical sameness has to be treated as an intransitive relation even where we are currently unable to discern empirical difference, and even when we extend what we reckon [empirically] to know of an object back upon itself. All this suggests that where and as a science makes use of a classification scheme permitting reference to just so many distinct and separate kinds of object more than mere empirical scrutiny of the objects must be involved. And indeed it is widely accepted that rules and conventions of classification are also necessary to account for how that activity proceeds. But even to accept this is not enough. If we invoke rules or conventions to help to account for judgements of sameness, the transitivity problem recurs yet again. How can an existing instance of the correct application of a rule or convention be reliably be extended to the next instance? Is there any way in which we can reliably figure out what a rule [really] implies, so that we can identify what the rule is ‘telling’ the rule follower to do, ahead of its application in the next instance? The finitist answer was that no such way existed: the actions rationalised by judgements of sameness had to be treated as contingent actions. But I shall not seek to justify this answer further here.

This purpose of this paper is not to advance technical arguments on behalf of ‘Edinburgh relativism’, and those above are not intended to do that. They are intended merely to expose the difficulties that surrounded and still surround an issue that has concerned it for many years. Unlike the other three components in the sociological conception of knowledge

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9 It is interesting to note that the philosophers who have systematically explored the problem here – including Goodman, Quine, ,Hesse, and of course, turning to a slightly different context, Wittgenstein – have varied in their views on their salience for epistemology and relativism. The clearest and most systematic discussion I know of is in Hesse 1974.
set out earlier, which can now seem banal even as applied to natural science, and elicit little debate, the fourth point has been a continuing focus both of argument and further research. Indeed, I now understand my own original words on the subject, written more than thirty years ago, better than I did then, although fortunately I find that I still broadly agree with them. Thus, work focused on this fourth point illustrates particularly well how ‘Edinburgh relativism’ developed and grew as a research programme. Participants did not receive relativism as revelation and thereupon leap happily out of their office windows, as some commentators appear to believe they should have done. What they did, somewhat after the fashion of the sciences in which they were trained, was to try to learn a bit more about, and get a more detailed understanding of, what they were talking about.

Recall now what the purpose of these recollections actually is. They are intended to illustrate how, contrary to widespread philosophical stereotypes, relativism could be inspired by a high regard for science rather than a wish to undermine it. I hope to have said enough now not just to show that ‘Edinburgh relativism’ was so inspired but to convey a little of how it was so inspired, – as well as to indicate how widespread and strongly held the stereotypes with which it conflicted once were, and how easily they could mislead and give rise to harm. But having gone so far I may as well fast track forward and link the story to the present, given that neither ‘Edinburgh relativism’ nor the negative stereotypes of relativism I have described are yet extinct.

In fact the more recent years of the story are the harder to recall and summarise, since they cover exciting times when there was a great deal happening and too much to do. In a few words, what initially was the search for resources to buttress our views became at the same time a search for topics of broader interest on which productive research could be carried out. In philosophy, David Bloor’s work on Wittgenstein had this character,\textsuperscript{10} as did much else produced in other disciplinary contexts.

\textsuperscript{10} Cf. Bloor 1983, 1997. I have cited Bloor’s work throughout because of its continuing direct engagement with the problem of relativism that is the topic of this book. But to get a proper sense of how things were unfolding in the ‘80s and ‘90s, and why nobody was much inclined to leap out of the office window, it is necessary to look as well at much else, not least the superbly original, accomplished and influential work of Steven Shapin and Donald Mackenzie.
And the historical and sociological studies produced in Edinburgh were often inspired as much by the opportunity to study fascinating topics as by the desire to exemplify a relativist standpoint. At the same time too the entire context of research was changing, as more and more people from many different backgrounds directed their attention to science and scientific knowledge and brought new attitudes and perspectives with them. The unscientific reverence for a sacralised natural science ubiquitous when we began our work was rapidly being transformed into the taste of just some groups among many, in a setting wherein a strongly critical approach to science and scientific knowledge was also establishing itself, and there were even those who were critical of relativist claims because they were critical of claims in general and who wrote at length on the virtues of silence. This, of course, was a slightly double-edged development for us, and I recall finding it a little disconcerting when I finally noticed late in the 1980s that in my field at least we were no longer being attacked as subversives but as orthodoxy.

I want to mention an important book published at the end of this period as a sign and symbol of how things had moved on. It is one of a number that would serve, and I choose it partly because of my own continuing interest in genetics and its history. *Lords of the Fly*, 1994, Robert Kohler’s account of Drosophila genetics, can be seen as a part of a long revisionist trend away from the heroic histories of classical, ‘Mendelian’ genetics that I had encountered and learned to question in my youth. It is tempting to relate this trend to an epistemic downgrade of Mendelism ongoing among geneticists themselves as their field became increasingly ‘molecularised’, but that could easily be wrong. Certainly, a major shift of epistemic authority from classical to molecular genetics was occurring as Kohler’s book was being written, and may conceivably have made its writing that much easier, but there is no positive sign of its effects in the work itself. Rather, it offered a history that had set aside any concern with the epistemic standing of the science. From my narrow perspective it exemplified a kind of history, no longer thin on the ground, which was making many of our old arguments gratuitous, as what they sought was built in to historical method. More important than all this, however, were the forward looking aspects of the book. It was firmly centred on newly prominent foci of research and began with an extended justification for a shift of historians’ attention toward them. It was a history of scientists
at work; of their experimental practice; the artefacts involved therein including living artefacts; the ‘material culture’ of science; and its moral economy. Again, some of this shift might have been encouraged by associated secular trends; like the relentless transformation of science from a pursuit ordered along pseudo-aristocratic lines to a specialised form of paid employment, and the larger no less relentless move to a less deferential society, wherein work is work, as it were, hand is valorised as much or more than head, and expertise can no longer expect to be put on a pedestal. Even so, work of this kind was greatly needed, the more so for its earlier neglect. And in subsequent years it has fed back into and enriched the historical study of science generally, not to mention work in other disciplines.\footnote{11}

From roughly this point in time to the present, with several of the groups and sub-fields now studying scientific knowledge doing so in a tolerably naturalistic way, many of the old arguments not so much settled as in abeyance, and the individuals who had carried it forward relocating and finding new challenges in new locations, it becomes more difficult to trace the path of the programme, or even to parry the question of whether a programme following a path is still there to be traced. In my own work, problems thrown up as it has gone along have tended to be the backdrop I refer to, and I tend not to worry about how secure is the chain of sameness relations that connects me to the initial relativist position. Things have stayed the same; things have changed: have it how you will. But I have largely worked in the social sciences, and in philosophy it is easier for anyone looking to trace the path of a programme via explicit citations of its formal tenets and commitments, and to judge whether it is succeeding or flagging in the face of potent criticisms and arguments.

\footnote{11} Kohler explicitly mentions his lack of attention to philosophy and sociology and particularly to the social constructivist sociology which had been important in pioneering the study of experimental practices. But he nonetheless makes an intriguing contribution to social constructivism himself. Read in the light of his new priorities, the true hero of Kohler’s book is not Thomas Hunt Morgan, the intellectual inspiration and overlord of fly room research, but Calvin Bridges, the indispensable technical supremo, ruthless killer of non-Mendelian flies and skilled curator and tracker of those permitted to live. Bridges constructed a set of laboratory flies of which Mendelian genetics was true [or, if you prefer, to which it could be productively applied]. Given the whole earth, he might be imagined reconstructing the entire D. Melanogasta species so that Mendelism was true of it. Here is an interesting way in which the truth of a theory may be established: if your theory doesn’t correspond to reality, then reconstruct reality, as Bridges did.
In the context of philosophy ‘Edinburgh relativism’ actually sits at the site of a potentially endless debate: the war between science and reason is not due to end any time soon and ‘Edinburgh relativism’ has made a distinctive contribution to it that merits repetition whenever hostilities resume, and ‘relativism’ is attacked, as periodically it continues to be. A very recent episode of this kind is the eruption of militant, rationalist atheism among some scientists currently attracting attention in England, with Richard Dawkins its most prominent figure. An earlier, larger and more prolonged series of encounters was the so-called science-wars, wherein the physicist Alan Sokal led an alliance of scientists and philosophers into battle against relativism, post-modernism and various other tendencies offensive to good rationalists. Both Dawkins and Sokal deplore relativism. But the relativism they both deplore is basically that of the venerable philosophical stereotype that characterises it as irrationalist and anti-scientific, – although the stereotype is a model of subtlety compared to the accounts that have been propagated by these two. Both figure among those mentioned earlier, who are prone to wonder why relativists don’t leap out of their office windows.¹²

In the context of the science wars a continuing interest in ‘Edinburgh relativism’ can be documented as its tenets were cited and debated and ancient criticisms were rehashed, but of course what we have here are polemical confrontations between public intellectuals, and if the aim is to check how far its basic tenets and commitments have continued to be interesting at a more technical level they are not salient. Here, the need is to review their standing in the appropriate settings, where some of them at least have continued to prompt reflection and debate. As far as its ba-

¹² It is hard to be fair to eminent thinkers such as these, since it is hard to work out when they are being serious. How serious was Dawkins 1995 when he challenged relativism with the claim that ‘Airplanes built according to scientific principles work […]'. Airplanes built to tribal or mythological specifications don’t? It is hard to say, but the second half of the quote is surely no more than empty polemic. I know nothing of the flightless airplanes built by the New Guinea aircraft industry, but Dawkins evidently knows no more, nor anything else relevant to what is claimed. The first part may seem a plausible empirical claim, but where is the evidence? David Bloor (Bloor forthcoming, see also Bloor 2008 for the basis of the discussion in this note) has studied of aircraft design in England and Germany early in the last century. The English sought to design wings in accord with scientific principles, whereas the Germans knowingly adopted designs that were inconsistent with them. The Germans produced the better aircraft, with wings that achieved superior lift. Dawkins appears to have had an antediluvian view of the science-technology relationship in 1995.
sic commitment to naturalism is concerned, that is, to proceed in analogy with the practice of the sciences in order to understand it and the knowledge engendered and sustained by it, the major focus of ongoing academic controversy is the programme’s readiness to propose causal accounts of intentional human actions. Important constituencies in both philosophy and the social sciences object strongly to use of the institution of causal connection in the understanding of voluntary human behaviour and insist that the appropriate resource here is the institution of responsible action; but this of course creates a dualist scheme of just the kind that the programme opposes – unless, that is, one rejects causal accounting altogether and treats all of the material world as alike possessed of the same Divine spark that, as some believe, animates humans. As far as its model of knowledge generally is concerned, the finitist claim that I have already discussed continues to be the component that inspires the most research and attracts the most critical interest.

Some of this more recent work has gone relatively unremarked in sociology and philosophy of science because it is no longer entirely focused on the distinctive concerns of those fields. Science had long been the sacred form of knowledge to which relativistic theories did not apply, could not apply, were not permitted to apply. But when a relativistic perspective was finally brought to bear upon it, scientific knowledge became for a time the test-bed of its ideas, the place to try out newly legitimated methods and techniques, a site of intense debate. Among those at work in this context, whether disposed toward relativism or opposed to it, understanding of some of the issues came up from behind and then moved on beyond what existed elsewhere. The flow of knowledge reversed itself to some extent in consequence, as researchers who had previously imported insights and exemplars in order to study science looked beyond it for sites to apply what they had learned thereby. The rationalisation for this, of course, already existed: if all forms of human knowledge were to be understood in the same way, then knowledge of scientific knowledge should routinely apply to knowledge generally; it merely involved transferring techniques and exemplars across conventionally drawn boundaries in the opposite direction from before. Indeed, at some point the practice of this relativist epistemology/sociology of scientific knowledge was almost sure to become, what in the abstract it was already, an epistemology/sociology of knowledge simpliciter.
The work that most easily exemplifies this change is that surrounding the finitist claim. My earlier discussion used the example of a kind term being applied to objects: the finitist claim is that there is no inherently correct way of applying the term and every successive application must be treated accordingly as a contingent action. But the discussion applies analogously to laws, rules and norms, principles and postulates, and so forth, which also lack inherent empirical implications. And all of these entities are encountered in contexts beyond science as well as within it, and their study in those contexts is no less interesting and important. Thus, it is important in sociology to study the application of kind terms, and human kind terms in particular, in everyday contexts. As far as laws are concerned, the concept of scientific law actually derives from juridical contexts, with the initial modification en route to science being the replacement of a human with a Divine legislator and a redirection of interest from science back to law may add to an understanding of how laws are applied in both contexts. The problem of the implications of rules and norms arises in every kind of social context: with those who study bureaucratic hierarchies and organisations addressing it as part of the enduring question of how rules are to be enforced, and those interested in joint and collective action asking how agreement on norms and

13 Research is moved by practical concerns here, as in the obvious cases of ‘race’ ‘ethnicity’ and ‘gender’. But there is also increasing theoretical interest in the use of terms that designate human statuses rather than describing their empirical state. Status designations have fascinating self-referential features which are at last beginning to attract the attention they deserve.


15 Legal professionals hold more diverse and conflicting views on law than scientists appear to do. In the US, for example, we have ‘strict constructionists’ angered by ‘judge-made law’, and ‘realists’ who believe there is no other sort. Moreover, different forms and systems of law enrich the legal imagination. It is tempting to conjecture that case and precedent law eases the understanding toward finitism more than statute law does.

16 The conclusion to be drawn from a finitist account here is that any action at the foot of a hierarchy may be rationalised as in conformity with the relevant rules and instructions sent down from above. This is not to say of course that the rationalisation will be accepted by those above, who may make a Hobbesian response to the rationalisation, and indeed ought to do just that sometimes if their organisation is too big to fail. But it helps to account for the recent recession, and to explain why hierarchical organisations make extensive use of expensive lawyers.
what they imply in particular cases is sustained in groups. And this last problem, whether with regard to norms or to values and principles has also long been faced by moral and ethical theorists.

By way of conclusion I want to emphasise the value of addressing this basic problem in different domains by referring to a philosopher who has habitually done just this. Jürgen Habermas moves back and forth between the sciences and other contexts, using his understanding of each in an effort to improve his understanding of the other, – something of which I wholly approve and which I have tried to do myself. But Habermas is a rationalist philosopher, and since he and I have started from opposed positions it is perhaps unsurprising that we have ended with opposed positions. Consider how he has linked science and moral philosophy. Habermas assumes that the knowledge of science has a propositional form, that it consists in statements with determinate universal implications that may or may not be true, the actual truth of which may be addressed through rational argument. And he goes on to assert that moral norms have an analogous propositional form and that their determinate meanings permit rational argument about their acceptability. My own belief, in contrast, is that scientific knowledge does not have a propositional form, that Habermas is led astray by his conviction that it does, and that in morals as in science contingent actions are rationalised as following from whatever formulations are taken to imply them. I have no desire to resolve the difficult issue of which of these positions is the more plausible here; but to be able to evaluate them in terms of exemplars drawn from two such different contexts can surely only be helpful to those who look for some sort of resolution in due course.

As it happens, however, Habermas has himself identified difficulties in his own account of moral norms, akin to those that would be exposed by a finitist critique. “No norm contains within itself the rules for its application” he tells us,

Yet moral justifications are pointless unless the decontextualisation of the general norms used in justification is compensated for in the process of application. [...] discourse ethics cannot evade the difficult problem of whether the application

of rules to particular cases necessitates a [...] distinct faculty of [...] judgement [...]'. The neo-Aristotelian way out of this dilemma is to argue that practical reason should forswear its universalistic intent [...]'.

From a finitist perspective, of course, ‘the application of rules to particular cases’ is contingent action, and does indeed, with a vengeance, constitute a ‘difficult problem’ for a universalising rationalism. It is interesting that the problem identified in this passage was accompanied by no solution; instead Habermas in effect set it on one side and continued to develop his theory of discourse ethics regardless.

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18 Habermas 1990, p. 206.

