The Narrative Reconstruction of Science

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In contrast to earlier accounts of the epistemic significance of narrative, it is argued that narrative is important in natural scientific knowledge. To recognize this, we must understand narrative not as a literary form in which knowledge is written, but as the temporal organization of the understanding of practical activity. Scientific research is a social practice, whereby researchers structure the narrative context in which past work is interpreted and significant possibilities for further work are projected. This narrative field displays a constant tension between a need for a coherent, shared understanding of the field and the incoherence threatened by divergent projects and interpretations. The account has three parts. First, a summary of the general account of the narrative intelligibility of action which underlies the preferred view of narrative in science. This account is then applied to understanding how scientific work acquires significance, and how the scientific literature is constructed and read. Finally, it is shown how this account should transform our understanding of the unity of science, and it is suggested how it can help undercut various realist and anti-realist interpretations of scientific knowledge, while also challenging the ironic stance which many recent sociologists adopt toward the global legitimation of science attempted by many philosophers.

Philosophers inquiring about the cognitive significance of narrative have typically not been thinking about natural scientific knowledge. The recent history of the question about narrative cognition, for philosophers at least, begins in the 1960s with the development of what has been called the ‘analytic philosophy of history’. For philosophers like Louis Mink, a principal concern was to recover the autonomy of historical understanding from the positivists’ attempt to assimilate all explanation into the hypothetico-deductive mode. The question whether narrative had a genuinely cognitive function was decisively shaped by this encounter with positivism in three ways:

1. The question was taken to concern only some disciplines, typically history, biography, literature, and perhaps the human sciences more generally, for which the positivist accounts of natural science were thought to be inadequate.

2. Narrative was taken to be a specific mode of comprehension or form of writing which is or should be employed in those disciplines, and its use was parallel to the role of theoretical comprehension or hypothetico-deductive explanation in the natural sciences.
3. Philosophical discussion was focused upon whether the structures of the completed narrative text (determinate beginning, middle and end, narrative point of view, etc.) are essential to historical or literary understanding, and whether these structures are already found in actions and events or are artifacts of the narrative retelling of them.

Obviously, the turn to narrative opposed the reigning positivism on some important issues, including the unity of science, the universality of the covering-law model of explanation, and the epistemological importance of formal logic. But there were also important similarities. Narrative was supposed to be a form of explanation in history and literature parallel to the positivist account of theories and laws in science. This parallel ran quite deep in the work of Mink and Hayden White. They insisted that narrative form was imposed upon events, which occur in sequence but do not themselves exhibit any narrative structure. ‘Stories’, Louis Mink insisted, ‘are not lived but told.’ The unformed sequences or chronicles which we live through prior to telling stories about them have a similar place in Mink's or White's account of historical understanding to that of theory-independent observations in positivist philosophy of science. In both cases, there was supposed to be a strong separation between the form of comprehension and the unformed content upon which that form was imposed, and in both cases this led to problems about how narratives or theories could ever count as true.

There was also, I believe, a deep similarity in the larger projects of positivist philosophy of science and defenses of the autonomy of narrative understanding in history and literature. Both programs aimed to legitimate a form of knowledge as an appropriate deployment of a general human capacity. For the positivists, there was only one kind of knowledge about the world, which resulted from the ability to construct and test complex logical constructions (theories) against the results of careful observation. Louis Mink offered one of the most sophisticated defenses of narrative understanding by claiming that science and history instead reflected distinct modes of comprehension. But Mink's account had this in common with positivism (and with the neo-Kantian tradition more generally): the legitimacy and autonomy of disciplines of inquiry are secured as a whole by their fulfillment of the necessary conditions for the exercise of a human capacity.

The point of my emphasis upon these connections between positivism and the defense of narrative understanding in history is to redirect the question about the epistemological significance of narrative. Positivism is now dead even in the philosophy of science. History and literature thus no longer seem in such dire need of a theory of narrative understanding simply to fend off the encroachments of positivist methodological imperialism.

Nevertheless, I believe that narrative understanding remains epistemically important, and that we need to recognize its importance if we are to understand disciplined inquiry. To see its importance, I believe we have to break the links I noted between positivism and the question of the epistemological significance of narrative, by rethinking what the question is about.

First of all, I think that the epistemic importance of narrative is not confined to a special group of disciplines, as will be apparent from the very fact that I am concerned with the place of narrative understanding in the natural sciences rather than in history or literature. Second, I am not primarily interested in the form in which the results of investigation are written. I do not want to claim, for example, that scientific papers are really narratives in disguise. Rather, I am interested in the ways in which both the practices of scientific research and the knowledge which results from them acquire their intelligibility and significance from their being situated within a narrative. Third, narrative should not be thought of as a scheme imposed from without on an un-narratized sequence of happenings. Instead, I want to claim that the intelligibility of action, and of the things we encounter or use in acting, depends upon their already belonging to a field of possible narratives. On my view, we live within various ongoing stories, as a condition for our being able to tell them, or for doing anything else that can count as an action.

Fourth, I want to move us away from thinking of the epistemological significance of narrative in terms of completed narratives, with their established beginning, middle, and end, and their unitary point of view. Scientific knowledge should be understood instead as belonging to narratives in construction (or perhaps better, in continual reconstruction). In the narratives of science, I will argue, there is no unitary authorial point of view from which an entire course of events can be surveyed, for there are multiple authors engaged in an ongoing struggle to determine the configuration of the narrative within which they are all situated. Current scientific research does not simply work out what will happen next within a given domain of knowledge; it continually reconfigures its own past. These four points can be summarized in a single claim: the intelligibility, significance, and justification of scientific knowledge stem from their already belonging to continually reconstructed narrative contexts supplied by the ongoing social practices of scientific research.

There is a also a fifth way in which the neo-Kantian legacy of positivism still holds us too tightly. Earlier critics of positivism accepted the project of a grand legitimation of forms of knowledge, even while rejecting important features of its positivist version. Today the legacy of the positivist project of legitimation is still carried forward both by scientific realists and by proponents of various anti-realist interpretations of science. But that project has also been directly challenged by the constructivist program in the
sociology of science. Constructivists have often tried to debunk philosophical attempts to legitimate science. They have claimed that, despite its pretenses, science offers just one more worldview among many, in the service of specific social interests, and that science deserves no greater rational authority than competing interests. In the end, however, I will suggest that the sociologists depend too heavily upon the legitimating project which they supposedly criticize. They mistakenly agree with the positivists that science needs such grand legitimation, and that its absence somehow undercuts the credibility of science. Although my account of the narrative intelligibility of science owes much to recent constructivist sociology, it also serves to undercuts the sociologists’ ironic debunking of the rationality of scientific knowledge, by rejecting any need for a global legitimation of science.

My discussion of these claims falls naturally into three parts. I will begin by discussing the more general view of the narrative intelligibility of action which underlies my discussion of narrative in science. Second, I will discuss in some detail how this view helps us to understand science. Finally, I will sketch how I think this account of scientific knowledge undercuts both philosophical legitimations of science and the sociological debunking of that legitimation.

I

The claim that scientific research is only intelligible in the context of a narrative is an extension of a central thesis of my Knowledge and Power. I argued there that science should be philosophically reconceptualized as something which scientists do, rather than as a body of representations which results from the activity of research, but which afterwards stands on its own. Of course, it has always been recognized that science is an activity, in the sense that scientists perform experiments, do calculations, construct theories, and write papers. But it is a much stronger claim to say that the intelligibility, significance, and justification of these activities and their products depend upon scientists’ practical grasp of a research situation as a field of action. Today I also want to argue that this practical understanding takes on a narrative form, and that scientists, through their research, attempt to fashion that narrative in a particular way. Much of what happens in day-to-day science concerns the social construction of a coherent narrative field of action out of the multifarious doings of different scientists, whose work aims to push the story line in different directions. Only within a largely shared grasp of the current research situation, and the possibilities it opens up, can the work of particular research groups intelligibly proceed; yet the divergences between the ways the various groups take up those possibilities constantly threaten that shared understanding. Scientific knowledge results from this ongoing tension between narrative coherence and its threatened unravelling.

In order for us to see this point about science, I need first to make some general remarks about the temporal structure of action and practical understanding. I will not try here to argue for this approach to understanding action, which owes a great deal to my interpretation of Heidegger, since I have defended its general structure at some length elsewhere. I want to say that an event counts as an action only if it can be construed as having been brought about by an agent (where what it is to be an agent is to be explicated socially, rather than in the mere usual terms of the agent’s beliefs and desires). This, in turn, requires that we understand the action as being done for the sake of something else. But to act for the sake of something is a complex capability. It requires some understanding of how to do many other things, including how to utilize appropriate means, and how to situate the intended action with respect to some further ‘for the sake of which’. A completely pointless or unintelligible action is a contradiction in terms.

The contexts within which actions are both performed and construed as performances are further complicated by being socially situated. The successful completion of actions, and therefore the practical understanding by the agent without which they would not count as actions, requires the action to be appropriately supported by others, and to be adapted to contributing in appropriate ways to the actions and purposes of others. This will not happen unless actions are generally performed in the right way, such that they will be intelligible to those whose activity they must fit in with. As Mark Okrent puts it: ‘My end, the for-the-sake-of of my behavior, is impossible as an end unless I belong to a community and the end is a type of end within that community, an end that in turn performs an instrumental role for others within that community’. This, in turn, makes it ‘necessary that there be a certain standardization of ends and of ways of reaching those ends which we share or, at least, which the artificer takes into account in his work’.

These brief remarks can be summarized by saying that action must be teleological, holistic, instrumentally mediated, and socially regulated. But it can also be recognized that these characteristics of action are inescapably temporal. To act is to be ahead of oneself, that is, to have some understanding of what it would be to have done the action in question. It is also to have some sense of how to initiate or continue the action now. This is, in turn, to have a grasp of the situation one is already in, to which the action is an intelligible response. These three aspects are held together in the agent’s understanding, not in the form of an explicit representation, but of a practical capability. Our projecting ourselves ahead, by taking
over the situation we find ourselves already in, by presently acting, is an understanding both enacted and displayed in the action itself.

This unified temporal grasp has a narrative structure. The understanding projects us ahead into a standpoint from which the temporal field of the action is laid out from beginning to intended end. This understanding in the future-perfect tense places the action in a situation from which we set out as agents and proceed toward an indefinitely intended resolution. What we thereby have is not a story told in retrospect, but a story which the narrator is in the midst of. It is being enacted toward the fulfillment of a projected retrospection, but one which is constantly open to revision, as befits a story not yet completed. This continual revision comes about because of the recalcitrance of things and people (the 'narrator' definitely included) who do not pliantly accept their assigned roles in the story. But such resistance and the ways we adapt to it are only intelligible within the setting provided by the agent's practical grasp of the situation in narratized form. David Carr has nicely summarized the sort of view I am advocating:

We are constantly striving, with more or less success, to occupy the story-teller's position with respect to our own actions. Sometimes we must change the story to accommodate the events, sometimes we change the events, by acting to accommodate the story. The retrospective view of the narrator, with its capacity for seeing the whole in all its irony, is not in irreconcilable opposition to the agent's view but is an extension and refinement of a viewpoint inherent in action itself.  

As always, however, things are complicated by the social dimension of both action and the practical understanding which it embodies. Just as we each constantly count on the other people and things which inhabit our shared world to take on the roles and functions assigned to them in our narrative projections, so we are also characters in the stories enacted by others. But even this description is too individualized. For the intelligibility of our own narratives, even to ourselves, requires that there be some common ground between us and the people with whom we inhabit a world. We conform our own understanding, and the actions which arise from it, to the narrative schemata which we take to inform the actions of others. These schemata in turn result from, and also embody our understanding of, a past which we share with members of relevant communities. Membership in those communities is itself constituted in substantial part by sharing that past as a basis for further action, and by our accountability (to ourselves and others) for the intelligibility of those actions in terms of that past.

Unfortunately, however, the recognition that the intelligibility of action presupposes a social grounding in the shared past of some community(ies) names a problem rather than solves it. Suppose we grant (with Heidegger) that our behavior generally reflects a powerful tendency to enforce conformity (by ourselves and others) to accepted patterns of social practice in order to sustain communities with a shared past and mutually intelligible possibilities for action. Still, the common narrative which actions thereby implicitly invoke must exhibit a continuing pull toward incoherence. Sharing a communal past which constitutes a present situation as a field of action, different people nevertheless act upon that situation in various ways, which develop the supposedly common narrative in different directions. To some extent this reflects the fact that different people often belong to multiple communities, and situate their actions in a variety of narrative contexts, which overlap only partly those of their colleagues in any one of those contexts.

But follow out these divergences, and you also realize that even the members of a community sharing a history have already understood the earlier elements of its narrative to be configured in subtly different ways. What I originally spoke of as a common narrative is actually more like a contested field of competing narratives. I have in mind something like Alasdair MacIntyre's claim that 'what constitutes a tradition is a conflict of interpretations of that tradition, a conflict which itself has (or is) a history susceptible of rival interpretations'. Thus, I want to insist, against someone like Hayden White, that actions already implicitly belong to a narrative field, and are not indefinitely open to different narrative configurations or entailments; but I also want to disagree with MacIntyre, who sometimes writes as if there is one true narrative which most adequately describes a situation with its history and prospects.

Sharing a situation as a narrative field thus makes possible meaningful differences along with convergence. The need to make differences intelligible and a common project possible compels an ongoing struggle to keep in check the divergence of versions of the community's story, even as the various actions of its members strain at the limits of coherent inclusion with one another. This struggle takes the form of a shared concern to construct, enforce, and conform to a common narrative which gives a common sense to everyone's endeavors. The possibilities for failure, and the collapse into partial incoherence of a community's sense of how to proceed intelligibly, are many. But it is important to recognize that when they succeed, it is only through a continuing partial reconstruction of a shared sense of what the community has been about and where it can and should proceed.

II

We can now turn more directly to thinking about science as an activity situated within a narrative field. A good way to begin is to think about what makes a claim, a procedure, or an experiment scientifically significant. This has received rather less philosophical attention than the question of
not for its own sake, but because we now have an assay for the presence of mannose-6-phosphate in a cell. Mannose-6-phosphate plays an important role in cellular function, but the availability of an assay for it has a significance far beyond the importance of understanding its specific function. This assay is a tool for investigating the mechanisms by which the cell captures, transports, uses, and disposes of mannose-6-phosphate. These investigations enable us to investigate the molecules which enable the cell to do this, the receptor proteins. If you ask a cell biologist what she is doing, she might tell you she is trying to understand how the cell works. But this understanding she is aiming at is situated in a rapidly developing historical context, in which what was once the aim of the research, when achieved, is immediately recycled as a means for further investigation. If it cannot readily be recycled in this way, it quickly loses scientific interest. If it neither poses further interesting problems to investigate, nor provides useful tools for investigating other problems, then a result drops out of scientific discussion.

This recycling of means into ends continually reconfigures the narrative within which scientific work is intelligible. What projects are worth undertaking, what results must be taken into account, and what equipment (both instrumental and conceptual) can and should be used to advantage, undergo gradual shifts. Of course, to some extent the longer-term goals of a field remain relatively fixed. Cell biologists are trying to understand the structure and function of cells. Chemical kineticists want to find out what happens in the course of a chemical reaction and how the various intermediate stages of the reaction affect its rate. And so forth. But there can be considerable change in how it seems to go about achieving these further ends, and even what these ends consist in. When studying a cell, which processes are likely to be more informative about how it functions? Which cells should one study? Is it more important to determine where in the cell a particular process takes place, how the process unfolds step-by-step, how the process is genetically and biochemically regulated, whether it is sensitive to other processes or conditions inside or outside the cell, or what?

In the abstract, all of these are important, and arguments could be made for focusing on any of them. In fact, which approaches seem promising will be highly dependent upon the recent history of the field. Where have interesting new results actually been obtained? Which questions do we now have new or more powerful tools for trying to answer? Which ones do a particular scientific group have the skills and resources to tackle more effectively than others can? Engaging in one sort of investigation may provide significant insights or powerful techniques for advancing a quite different one. Sometimes this will have been anticipated and sometimes not. But scientists regularly respond to such new opportunities while
redirecting their research to circumvent unforeseen obstacles. These shifts of direction reflect the reconfiguration of the situation scientists find themselves in, and the directions in which those situations point them. And these reconfigurations are implicitly reconstructions of a narrative.

We can see this by examining more closely some features of the scientific literature. I have already promised not to say that scientific papers are really narratives in disguise. This is because papers do not tell stories about how the current research situation came about and where it seems to be heading. They are instead more complicated speech acts, which can only be understood in the context of a story.

Scientific papers are written for an audience. This audience is usually characterized in terms of its shared background (graduate training, familiarity with the literature, etc.), which enables the authors to presuppose many shared beliefs. But it is even more significantly an audience with a variety of closely related concerns. Most of the readers of scientific papers are themselves engaged in research. They read the scientific literature because they need to know what results they must take account of, what techniques they might usefully employ, and what research opportunities they could profitably take up. They also write their papers to address these interests. Authors couch the issue they are addressing in terms which will attract the interest of other research workers by showing its indispensability for their own work; they situate their methods and results in the context of recent work to show what is added or transformed by their contribution; and they determine what they need to say by anticipating and trying to respond to possible questions or objections from this audience with its specific concerns.9

It may not be immediately evident to the lay reader just how context-bound a paper is, because the context is assumed to be supplied by the reader, and issues are raised and responded to in a sentence or a phrase. Bruno Latour has effectively described this masking of controversy in technical language. Discussing a long and complicated sentence from a paper in neuroendocrinology, he notes:

Reading it after the other sentences, we have not suddenly moved from opinions and disputes to facts and technical details; we have reached a state where the discussion is so tense that each word fences off a possible fatal blow. . . . Each word is a move that requires a long commentary, not because it is ‘technical’, but because it is the final match after so many contests.10

Latour’s contrast between what is technical and what is contested is misleading, however. To be ‘technical’ simply is to be a response to a history of conflicts, a history which shapes a specific content and accounts for its significance.

But even granting that technical language and technically sophisticated work are the outcome of a history of conflict, why say that its technical complexity results from being situated in a continually reconstructed narrative? We can see this in two ways. A scientific paper itself gives clues in the ways it sets up the issue it is trying to address, and above all, in the ways it cites other literature in the course of doing so. Scientific papers indicate how the authors want us to interpret the significance of previous work which sets the stage for their own, as establishing a result they will build on, highlighting a recognizable inadequacy in the current state of the art which they will remedy, proposing a result which their work undercuts or reinforces, etc. In doing so, they propose (often rather indirectly) the ways in which they think their own work should be taken up in turn. Some of these express their own intentions for following up this particular investigation, which may already be underway by the time the paper is published. But for a paper to be significant, it must constrain or advance the work of others as well. Scientific papers are attempts to reshape (perhaps only in a small way) the situation in which scientists in that field see themselves working, and the possibilities for further investigation which arise out of that situation.

The second way in which the narrative context of the scientific literature reveals itself is in its occasional explicit reconstruction in review articles and textbooks. I have discussed this at some length in chapter 4 of Knowledge and Power. The key point for our purposes is that such reconstructions are always prospective. They attempt to bring together what is significant for the purposes of further research. Results are reported not simply because they are thought to be true, but because one may need to take account of them in going further. Often they appear in the retrospective literature in simpler, more standardized forms than in their original development, and they are frequently adapted to new purposes.11 Conversely, they lose all scientific interest if they are not adaptable in some way to the needs of further research or development. As Latour concluded:

The fact of what we say and make is in later users’ hands. . . . By themselves, a statement, a piece of machinery, a process are lost. By looking only at them and at their internal properties, you cannot decide if they are true or false, efficient or wasteful, costly or cheap, strong or frail. These characteristics are only gained through incorporation into other statements, processes and pieces of machinery.12

Or in my terms, they only acquire significance by being taken up into the continually reconstructed narrative within which scientific action makes itself intelligible.

How one’s work gets reinterpreted in the course of further research is largely out of one’s own hands, of course, despite all efforts to write scientific papers in ways which preempt criticism and constrain possible interpretations. Scientific work proceeds from the researchers’ practical grasp of their situation within an ongoing narrative, but that narrative is being developed simultaneously by others as well, frequently in ways which
do not fully mesh with one another. This happens in a variety of ways: results are sometimes directly opposed to one another; sometimes they are not so much inconsistent as they are indicative of very different prospects for a particular line of research; or they may employ different experimental methods or theoretical formulations which compete to become standard ways of accomplishing a particular task. These and other conflicts have many possible resolutions, which generally depend upon how well they mesh with the changing research interests and prospects of the field.

Recent scientific work is constantly being reevaluated in the light of those changing interests and related developments. There is a strong pressure toward establishing a common assessment of the current situation and its background by resolving the incoherence generated by conflicting or divergent research strategies and results. The point is not that anything approaching monolithic consensus results. Various scientists will frequently dissent from the current accepted view of the field, but even they recognize the existence of a conventional wisdom, and take it into account in constructing and reporting their research (often by explicitly trying to challenge or reshape it). This happens because everyone's work acquires its intelligibility from its contribution to a project which all take to be shared. It makes little sense to do the sort of esoteric scientific research which highly developed scientific fields involve unless it responds effectively to what everyone else has been doing, and has some prima-facie claim to making a contribution to what they will do hereafter.

The result is that scientific research is highly competitive, not just in the usual sense that everyone is trying for the same grant money, or racing for priority in a discovery. Researchers compete with one another to shape the future direction of research and make a significant place for their own work in its unfolding. This should not be interpreted as a reductive dismissal of scientific work as a kind of egotistical assertion of one's own importance. The same pattern results if scientists are genuinely convinced of the importance of their ideas and methods, and engage in these competitions for the relatively disinterested purpose of pointing the field in a more productive direction, by securing for these ideas or methods their rightful place. The recycling of scientific results and the consequent reconstruction of their narrative intelligibility are a structural feature of science as an activity, not a consequence of the motives of individual scientists.

If I am right that scientific work only makes sense in the context of an ongoing shared but contested narrative reconstruction of a research field, where is this narrative understanding located? After all, scientists do not usually write or tell narratives about how their work is situated within the development of their field, except in highly simplified versions constructed for outsiders. Narrative fragments do frequently appear in scientists' informal conversations with one another ('X' says she has a good assay for it.

and that would change the situation . . . .' if Y is right about that, it wouldn't make sense to do this, but . . . ', or 'Z's result means that we now have to test for that stuff . . . '), but the fuller narrative context within which scientific work proceeds is rarely foregrounded at all. Scientists can normally presuppose that their peers and readers already understand that situation. Nor is this narrative context quite the same as the story which might be later told by an historian of science. Scientists are situated differently from the historian because they see themselves as agents within an unfolding story, and consequently, that story is configured somewhat differently for them. William Macomber nicely captures the tacit and transient character of scientists' practical grasp of their field, even though he does not explicitly call it a narrative, when he says:

The context [in which scientists understand their work] has constantly to be reorganized and renewed. If we search for it, we cannot find it, for it is constantly expanding, shifting, being modified. The source of the significance of individual discoveries and the basis of their validity, it nevertheless always eludes our grasp. Yet it is a context with which every working scientist is familiar; among scientists, it is what 'everybody knows'.

The ongoing reconstruction of the narrative within which research activities are situated explains the rapid obsolescence of the scientific literature, and the fact that even graduate students learn all but the most recent science from updated textbooks rather than journals. For despite the supposed timelessness of scientific knowledge, its canonical form of presentation, the scientific paper, is ruthlessly bound to the interests and conflicts of a local context which is constantly under reconstruction.

III

We are now prepared to turn to how the narrative intelligibility of scientific research affects the philosophical and sociological debates surrounding the global legitimation (or delegitimation) of scientific knowledge. There is a long history to philosophical attempts to underwrite the legitimacy and autonomy of current scientific discourse. Most contemporary versions of this attempt depend upon some account of our finite epistemic capabilities and what can count as an appropriate deployment of them (surprisingly, this is even true of scientific realism, since realists like Richard Boyd depend upon the premise that scientific methods are highly theory-dependent in order to secure the claim that realism is the best explanation of science's instrumental successes). They postulate as a goal for scientific research the construction of a unified representation of the world as it shows itself to our capacities as knowers. This projected goal of a coherent world-picture has its reductionist versions, in which the coherence comes from explaining everything in a single privileged theoretical vocabulary (typically
that of physics). It also has pluralist versions, in which the results of different fields fit together as different levels of description appropriate for different levels of organization or complexity in the world.

My view has little affinity with either version. Constrained in its traditional way, the unity of science is predicated upon a representationalist view of scientific knowledge, and postulates as an epistemic ideal the unification of scientific representations of the world into a single all-inclusive and coherent picture. There are three respects in which I reject this construal of the unification of scientific knowledge. Most fundamentally, I do not believe that the accumulation of a body of representations abstracted from the activity of research is a goal of science at all. To be sure, many scientific achievements have a life of their own outside the ongoing practice of research which produced them, but this simply reflects their recycling as means to other ends in other social contexts. These contexts (institutionally labelled ‘development’ in contrast to ‘research’) are also practical and historical, they are intelligible in their own narrative constructions, and they in no way involve the unifying of scientific results into a coherent body of representations. If anything, the scientific claims which become the focus of ‘development’ are collectively more disunified and idiosyncratic than the various fields of scientific research in their ongoing narrative reconstructions.

Second, I am arguing that, far from inclusively bringing together all or most established scientific results into a single picture, the ongoing narrative reconstruction of scientific practices regularly excises established results from the background knowledge of a field, in order to focus its subsequent development. The coherence achieved through scientific research is thus practical rather than representational. Third, the advance of scientific research seems centrifugal rather than centripetal. New fields continually spin off with their own interests, methods, and interpretations. Their practitioners show little concern for how retrospectively to reconcile their interests and results with those of their progenitor disciplines. They will more typically consolidate their own results toward a new advance, perhaps in a still more divergent direction, than look back and try to see how it fits together with its origins. Scientists’ concern for coherence, at least in what they choose to investigate, is usually situated more locally than even the level of the discipline, let alone that of the whole of science.

Yet there is an important and different sense in which scientific knowledge does get unified on my view. Disciplinary boundaries in the sciences are regularly breached by researchers looking for tools and clues to further their own work. The more controversial or speculative claims of one field are explored, tested, and criticized through the use of more stable results of another. Hybrid fields are continually created as scientists discover new research opportunities for those who develop the right combination of skills and knowledge. What we get is not a single coherent picture of the world, but an ever more complex network of interconnections binding together various scientific endeavors. Achievements from one field get reworked and interpreted in order to serve the interests of another which may be at cross-purposes. The loss in coherence is often happily compensated by the creation of new possibilities to explore, and new capabilities for doing that.

Since I reject the very idea of the philosophical project of globally legitimating scientific knowledge, not just the current versions of it, I may be read as endorsing those recent sociological interpretations of science which also have no truck with philosophical legitimations of science. This reading will undoubtedly be encouraged by some real similarities between my account of the narrative intelligibility of science and various recent sociological interpretations of scientific research. But a characteristic feature of much recent sociology of science is its ironic stance as counterpoint to philosophical and popular images of science as a model of rational inquiry. Philosophical rationalism about science is countered with an enthusiastic debunking of any claim that science is distinctively objective, disinterested, or rational. Science is supposedly removed from the pinnacle of culture and reduced to just one world-view among others, as if the sciences themselves offered a single unified view of the world.

I draw an alternative moral from the differences between my view and much of the philosophical tradition. Like Arthur Fine, I believe that the currently dominant opposing philosophical accounts of science embody a fundamental mistake: they ‘alike view science as susceptible to being set in context, provided with a goal, and being made sense of’ because it ‘could not or did not do these very things for itself’. Philosophical interpretations of science simultaneously provide both a general explication of how concepts like explanation, justification, and reality apply to science, and a global defense of the rationality of the activity or results to which these concepts apply. Sociological irony contrasts such philosophical interpretations of science to empirical sociological studies of scientific practice, and concludes that ‘real science’ bears no resemblance to the philosophers’ idealized picture. But the force of the irony depends upon accepting the view that science is in need of a global philosophical legitimation. Otherwise, the failure of such legitimation projects would make little difference.

The interpretation of science as action made locally intelligible within socially reconstructed narratives challenges this shared demand for legitimation, from two directions. On the one hand, it suggests that scientific research does not need philosophical explication of its epistemic and ontological standing. The narrative understanding within which scientific work takes place includes a developing sense of what counts as an adequate explanation, of when a claim is well confirmed, of whether a postulated entity can be taken as actually existing, and so forth. Of course, scientists’ interpretations of these concepts are local rather than general. They are
tied to particular (sub-)disciplines at particular points in their history (a history which of course includes their interactions with and comparisons to other disciplines). Topic-specific, comparative, and tailored to the capabilities and prospects of that field, they typically neither presuppose nor point toward a general account of explanation, justification, or ontology.

On the other hand, my account also denies that scientific research is vulnerable to criticism from such general philosophical views, unless the criticism addresses local scientific concerns in their own terms. The epistemological scruples which motivate, say, van Fraassen’s constructive empiricism have no obvious claim on high energy physicists unless they suggest better ways to configure the situation (i.e., reconfigure the narrative) of high energy physics today as a field of research activity. For in this case, van Fraassen’s views are competing not with Boyd’s arguments for realism, but with interpretations of how to understand and deal with leptons, hadrons, and quarks which are the well-entrenched outcome of a sustained and ongoing effort to make sense of these things. The history and apparent prospects for these efforts provide the only plausible context for evaluating this sort of competition.

This pragmatic skepticism toward philosophical legitimations of the sciences results directly from my taking them to be practices locally intelligible within narratives. But the sociological ironists may claim that this local intelligibility is insufficient. It may justify scientific norms and values for those who are already committed to scientific projects on other grounds. But it does nothing to justify the common deference to and reliance upon the sciences within a larger cultural context. This deference may seem omnipresent: in the commitment of public resources to scientific research, the widespread technological reliance upon science, the political and legal authority conferred upon scientific expertise, and the place of science teaching in publicly mandated schooling. And when this deference to science is challenged, its defenders frequently invoke the post-Enlightenment philosophies of history which connect science on a global scale with rationality, utility, and moral progress. So perhaps sociological irony toward science constitutes both an appropriate and powerful political critique of modern society, and a cultural antidote to the project of legitimating modernity.\(^9\)

I nevertheless remain suspicious. The sociologists who adopt an ironic stance seem too thoroughly indebted to the philosophical views which they are apparently so anxious to reject.\(^20\) Their irony tacitly presupposes the modernist dichotomy: either a grand narrative of rational legitimation, or else irrationality, relativism, and the multiplication of world-views. They also too readily accept the efficaciousness of global legitimations of science as accounting for the importance of the sciences in our culture; whereas I suspect that the variable social influence of the sciences is relatively insen-

ative to large-scale defenses or criticisms of the rationality of science. Most of all, the debunking of scientific rationality is too heavily dependent upon the legitimation of the sociological discourses which aim to show that science is (really!) interest politics, the social negotiation of consensus, or the social construction of reality. Finally, the frequent invocation of the notion of ‘world-views’, with the underlying suggestion of a subject as viewer and the world as object viewed, seems too uncritically indebted to the modernist project, which in some respects sociological ironists are at pains to debunk.

I suspect we would be better off if we try to do without the rationalist/relativist dichotomy and sociological replacements for epistemology, and if we replace a conception of the world viewed with the world engaged. We should by all means attend to the social construction of science, so long as it does not obscure the concomitant scientific reconstruction of society. The rejection of any global legitimation of science as an essentially progressive and rational enterprise should open a space for political reflection on the interactions between the sciences and other practices. The ironic stance closes that space as effectively as does any discourse of legitimation.

Much more needs to be said to substantiate these suspicions that the ironic stance of constructivist sociology is fatally intertwined with the philosophical project of globally legitimating science. Since the earlier parts of my paper were guided all along by these suspicions, you may reasonably worry that until this is done, nothing I have said here is believable. But the justification of these suspicions would plunge us headlong into the debates over the politics of modernity and, allegedly, post-modernity. Disentangling the interpretation of the sciences from the politics of modernity is a project which goes far beyond the scope of a single paper. I shall therefore content myself with the suggestion that understanding science as already situated within its own narrative context (where what constitutes its ‘own’ is very much an issue within that narrative field)\(^21\) may provide a fruitful approach to the question of how the interpretation of the sciences is thus politically entangled with the interpretation of ‘modernity’.

NOTES

1 Earlier versions of this paper were presented at Wesleyan University’s Center for the Humanities, Oberlin, and to a session on ‘Narrative and Science’ organized by Robert Crease at the 1987 meeting of the International Association for Philosophy and Literature. I am grateful to my research assistants on all of these occasions, and especially to Thomas Ryckman, commentator for the colloquium at the Center for the Humanities, for their helpful comments and criticisms.


4 In what follows, the discussion of the narrative structure of action and temporality goes beyond both Okrent’s and my earlier account of action.
5 Ibid., p. 46.
12 Ibid., p. 29.
17 E.g., compare Andrew Pickering’s striking conclusion in his book, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press, 1984), pp. 413–14: ‘There is no obligation upon anyone framing a view of the world to take account of what twentieth century science has to say . . . World-views are cultural products; there is no need to be intimidated by them.’
19 A first attempt to consider how one can adequately understand and assess such political criticisms of scientific practices without invoking modernist demands for the global legitimation of science is offered in Joseph Rouse, ‘The Politics of Post-Modern Philosophy of Science’, *Philosophy of Science*, forthcoming.
21 This point is discussed at some length in Rouse, ‘The Politics of Post-Modern Philosophy of Science’, op. cit.

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