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# The Social Location Of Scientific Practices

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Chapter 6

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The mainstream tradition of modern science denies the *historicity* of scientific practices. It denies that their *character* changes, and must change, in fundamental ways that arise historically, and that are responsive to and shaped significantly by varying circumstances. In Section 1 I will identify several presuppositions that have been commonly used to support this denial: most notably, that the object of science is ahistorical and its methodology essentially unchanging, and that the character of basic scientific methodology is not dialectically linked with applied science. In Section 2 Kuhn's rejection of the presuppositions about the object and unchanging methodology of science will be endorsed, thus providing support for what Margolis has referred to as a 'remarkable (but somewhat muffled)' version of the historicity of science.[2] Then, in Section 3, drawing upon a detailed analysis of a contemporary controversy between agrobiotechnology and agroecology, I will move beyond Kuhn and also reject the other presupposition that there are no dialectical links between methodology and application.

Specifically I will argue that the character of scientific practices reflects mutually reinforcing relations with the social location in which they are conducted, that is, relations with the value-outlooks of their practitioners and their enabling institutions, and the interests that will be served through applications of their products. It is a small step from this to endorsing an 'unmuffled' version of the historicity of scientific practices, one that admits that variations in the character of scientific practices may be dialectically linked with historical and cultural variations in the realm of daily life and experience and in the structures of social practice.

## 1 Scientific Practices as Lacking Historicity

Let me briefly recapitulate a story that articulates an important part of the self-understanding of the modern scientific tradition, and that has often been re-told. Science has a *history*, a history of progress: of growth, accumulation and refinement of scientific knowledge, and of elimination of error. It is a history in which *methodology* plays a central role. Provided only that scientific practices are kept free from outside interference and nourished from time to time by the input of creative genius, methodology ensures the continued unfolding of the progressive development of science. Scientific methodology is systematic and empirical, rooted in experiment and measurement. It prescribes that empirical data be brought to bear upon theories that, using the resources of mathematically articulated lexicons, posit representations of phenomena and their underlying order and law. Apart from refinements of detail, scope and precision, scientific methodology does not change. Thus, the 'scientific revolution' of the sixteenth and seventeenth centuries marks the effective *beginnings* of science (anticipated only by scattered fragments of scientific knowledge), not a fundamental *change* in the methodological character of scientific practices.

Since then, the story continues, the cognitive (epistemic) credentials of scientific methodology have been certified and repeatedly vindicated. Technological success that has been informed by scientific knowledge has been one source of the vindication. Another has been the knowledge and understanding of 'the world' ('the natural world,' 'the material world') – of natural laws, and of things, events, states of affairs, phenomena, structures and their underlying components, processes and interactions – that have accumulated and been refined, and whose compass continues to expand, bounded only by the limit of a 'complete account' of 'the material world,' one that in due course would encompass all phenomena.

The story admits of competing versions with different emphases about, for example, the primacy of theory or experiment and the significance of applied science. In all versions, however, the tale of progress attends principally to such matters as theories that have been developed, available data, technical possibilities for experiment and measurement, methodological matters, and the (creative) inputs of individuals (or groups) of scientists. That way the 'rationality,' 'universality' and 'objectivity' of the cumulative and developing process are able to be emphasized. There is a place in the story for social, economic and political factors: sometimes the interests of utility lead to a focus on a particular object of inquiry and, more generally, the rhythms and organization of scientific research depend upon the availability of the

appropriate material resources and social conditions. Even so, the fundamental dynamic of scientific progress is 'rational': science is progressively, and with ever greater refinement, gaining knowledge of objects of 'the material world.' Social (and hence historical) factors may lead to giving priority, even urgency, to gaining knowledge of specific instances of these objects; but gaining knowledge of them contributes to the overall accumulation and refinement of knowledge of 'the material world.' It is the accumulation and refinements that matter most. The rest, including the temporal order in which objects are investigated, is incidental. Nowhere in the story does the character of scientific practices change in fundamental ways; they do not exhibit historicity.

### *1.1 Suppositions Supporting the Denial of Historicity*

At the root of this denial of historicity are the following three suppositions:

1 Science aims to gain a kind of understanding that is expressed in theories that match ever more completely and accurately an ahistorical object, 'the material world,' whose underlying order (laws; and structures and their components, processes, interactions) is *ontologically* independent of human actions, desires, conceptions, observations and investigations.[3]
2 The methodology of modern scientific practices (subject only to refinements of precision, scope and the like, but not to any fundamental change) enables us progressively to gain understanding of this ahistorical object – so that there is no deep historical dialectic of methodology and object of inquiry, and so that the questions posed in basic science (while they might depend on the results of previous inquiries and the availability of instrumentation and appropriate mathematical and conceptual resources) do not concern objects insofar as they are historically variable, socially located, or playing integral roles in human practices.
3 The acceptability of scientific theories depends only on considerations involving their features and their relations with empirical data of selected kinds.

Clearly, and consistent with supposition 1, the actual arrangements of material objects in our vicinity are not *causally* independent of human affairs. These arrangements may be consequences of scientific applications; so much so that, although science supposedly lacks historicity, in virtue of its applications it has become nevertheless a historical agent of extraordinary importance. Indeed, it has been held, the very success of science in informing technological developments is explained in terms of its having gained sound

understanding of 'the material world.'[4] According to the story under review, the historical agency of science may account for the ready availability in the advanced industrial countries of the social conditions and material resources required for the pursuit of science. Moreover, nowadays a good deal of research depends upon the availability of instruments that are the products of the most advanced and sophisticated technology, whose availability is itself made possible by scientific developments. This means that the historical agency of science functions as an 'instrumental partner' of scientific research, one that enables the methodology to be deployed in a more refined way (for example, enabling us to obtain greater precision in measurement and to explore hitherto inaccessible spaces). It feeds back so as to serve the cognitive (epistemic) interests of gaining scientific understanding (a 'happy coincidence' of social practical interests and knowledge-gaining interests[5]), but it leaves the fundamental character of scientific methodology essentially unscathed. Thus, the denial of historicity also involves the supposition:

4 The historical agency of science (exercised through its applications) is only a consequence and an instrumental partner of successful scientific practice; it is not a dialectical partner, one that feeds back so as to influence the fundamental methodological character of these practices.

Affirming the historicity of science involves denying suppositions 1 and 2: denying that the object of scientific investigation is ahistorical, and maintaining that there is a dialectic between methodology and object of inquiry. It is deepened by also denying supposition 4, affirming that there is a dialectic between methodology and the practices of socially applied science. Kuhn has made a compelling case for the denial of suppositions 1 and 2, though he seems to accept supposition 4.[6] Before addressing Kuhn's argument, let us extend the story being re-told a little further.

### *1.2 Do Soundly Accepted Theories Represent the 'Material World'?*

According to our story, *objects as grasped in the practices of basic science*, that is, *objects as represented in soundly accepted theories*, are (approximately) identical to *objects as they are in the underlying order of the material world.* Scientific practices, and the modes of interaction and thought that constitute them, enable us to grasp things as they *are* in the ahistorical 'material world,' abstracted from the context and conditions of our investigations, and indeed from all human related contexts. Since scientific practices are themselves historical, how can this be so? Methodology is the key to the answer; but how

is it that a methodology deployed within a historical practice can enable us to grasp the ahistorical?

It is able to do so, a common response goes, in virtue of the character of and relationships between *theory* and *empirical data*. To elaborate: theories developed in scientific practices deploy carefully expressed posits (and models), typically in mathematical form, about underlying (non-apparent) structure and its components, process, interaction and law; so that theories abstract the phenomena investigated from their places in the social order, in daily life and experience, and even in scientific practices themselves. And data are sought out and reported, and the conditions in which they may be obtained are often created, in the course of experimental and measurement practices. Relevant data, obtained from observing phenomena of which a theory is proposed to provide understanding, meet the condition of intersubjectivity (and, where possible, replicability), and quantitative and experimental data are of special significance. Then understanding of objects of 'the material world' is expressed in soundly accepted theories.

A theory is *accepted* if its posits (pertaining to certain domains of phenomena) are put into the stock of established scientific knowledge, the stock of those posits judged to be such that further investigation or testing of them would produce at most refinements of accuracy and scope.[7] A theory is *soundly accepted* (of the phenomena of a specified domain) if it satisfies certain criteria, that is, if it manifests the *cognitive values* highly in relation to the available data from this domain – if it has specified characteristics (for example, consistency, simplicity), relations with other accepted theories (for example, inter-theoretic consistency, consilience), relations with displaced theories (for example, being a source of interpretive power of the strengths and weaknesses of a displaced theory), and most importantly relations with available empirical data (for example, empirical adequacy, explanatory and predictive power).[8] Theories that have been soundly accepted of specified domains have also reliably informed numerous practical (technological) applications.

What legitimates the move made in the story from (a) 'T manifests the cognitive values highly with respect to D' to (b) 'T represents (matches) order of the "material world" underlying D'? (T is a theory and D is a domain of phenomena.) One might respond: Is it not obvious, given that T represents the phenomena of D in abstraction from the relations they may have with human and social affairs, in terms of their being generated from the underlying order, and that the sound acceptance of T depends *only* on judgments of the manifestation of the cognitive values in T with respect to D? Moreover, that the move has been frequently and casually made throughout the course of modern

science suggests that it is taken to be obvious. In what else could the cognitive value of T consist other than a match with parts of the 'material world'? Well, it could consist in confirming (c) 'T encapsulates well the possibilities of phenomena of D in so far as they derive from the generative power of the underlying order (their '*material possibilities*'),' or (equivalently) in grasping these phenomena *qua* abstracted from their human and social contexts.

Elsewhere I have argued that the move from (a) to (b) is not mediated by (d) 'T reliably informs technological applications';[9] but the move from (a) to (c) is supported by (d). Often the move from (a) to (b) is made against background commitment to materialist metaphysics, which may be considered as a suitable elaboration of the posit that all phenomena are lawful or that all possibilities are material possibilities: the 'material world' – the ahistorical order underlying things – *really is* such that it can be matched by (and only by) the kinds of posits put forward in modern scientific theories. Were there a sound *a priori* case for materialist metaphysics, this might be compelling. But today, for the most part, those who espouse materialist metaphysics do so on the ground that it is an extrapolation from established scientific understanding and the direction of its expected growth. Then, if materialist metaphysics provides the ground for the move, the question is begged.

## 2 Kuhn's Account of the Historicity of Science

Kuhn maintains that there is nothing in the character of scientific practices that justifies the move from (a) to (b),[10] and that, furthermore, attention to the actual history of science suggests that the move would be clearly unjustified. In the history of science, he maintains, we do not find steady accumulation and refinement, but instead periods of fundamental discontinuity in the character of scientific activity – discontinuities (for example) in what is considered a theory worthy of provisional investigation, in what are the appropriate phenomena to investigate for the sake of gaining empirical data (and in the descriptive categories of the data) that are to be fitted by theories, and in what kinds of posits are taken to be central for shaping scientific investigation.

### *2.1 Soundly Accepted Theories are Developed and Consolidated under a 'Strategy' ('Paradigm')*

According to Kuhn, if theory and empirical data are taken to be *the* major elements of scientific methodology, no sense can be made of the actual history of science. Kuhn proposed a third element: *paradigm*, of which

I will consider just one aspect: within a paradigm, research is conducted under a *strategy*.[11] A strategy specifies *constraints* upon theories that are taken to be admissible for provisional consideration (and possible eventual acceptance), and (reciprocally) criteria upon the kinds of empirical data (and the phenomena from the observation and measurement of which they are obtained) that are *selected* as those appropriate for being brought into the appropriate relationships with theories. These are the kinds of data needed for testing and selecting among provisionally entertained theories, and those that describe phenomena so as to enable their explanations and the encapsulation of their possibilities. Admissible theories may be constrained, for example, to be formulated with the resources of a specified lexicon (for example, the teleological/sensory categories of Aristotelian physics, or the mathematical/mechanical ones of Galilean physics), and the data may be selected (generally subject to the condition of intersubjectivity and, where appropriate, replicability) in virtue of (for example) being representative of phenomena of daily life and experience, or of pertaining to experimental and measurement practices.

Given this third methodological element, we are able to identify two key (logically distinct) moments of choice: choice of strategy to adopt in research practices, choice of theory to accept or reject. Choice of theory is, then, in the first instance choice among provisionally entertained theories that fit the constraints of the adopted strategy.[12] When properly made, it involves judgment about which one of them best manifests the cognitive values with respect to the available data, about whether the available data are sufficient and about whether the manifestation meets high enough standards for accepting the theory of the relevant domains of phenomena.[13] Accepted theories encapsulate soundly certain kinds of possibilities that these phenomena permit. (Successful application testifies to this.) So adopting a strategy involves identifying the kinds of possibilities desired to be encapsulated; accepting a theory involves identifying (typically through consolidating posits about how to actualize them) the genuine possibilities of these kinds.

In the light of the Kuhnian insight, our initial story can be reinterpreted or (more accurately) replaced by a narrative of research conducted under a particular set of strategies – that I call *materialist strategies* (MS) – that have been adopted virtually exclusively within the modern scientific tradition.[14] MS incorporate the core methodological elements cited in the story. Under them, theories are constrained to those with the lexical and mathematical resources to be able to formulate posits of underlying order – structure and its components, process, interaction and law, where laws express relations among quantities. These theories identify the possibilities of phenomena in

terms of the generative power of the underlying order (the ones I have called 'material possibilities'), in abstraction from any place they may have in human experience and practical activity, from any links with social value and with the human, social and ecological possibilities that they might also admit. The 'material possibilities' of phenomena include possibilities that are identical with possibilities for technological application. Reciprocally, under MS, data are selected (subject to intersubjectivity and replicability) so that their descriptive categories are generally quantitative, devoid of the categories of intentionality and value, applicable in virtue of measurement, instrumental and experimental operations.

Research conducted under MS has been extraordinarily successful: it has generated and continues to generate soundly accepted theories of a great variety of phenomena; and these theories have been the source of numerous and varied technological applications. Kuhn has little interest in applications. And, as I have said, he does not take the success of MS in producing soundly accepted theories to show that these theories match the ahistorical 'material world.' Instead, for Kuhn, it establishes that the world can be (to a marked extent) well matched to, or become amenable to grasp within, the categories of the lexicons deployed under MS.[15] I add that 'material possibilities' of things are successfully identified under these MS; and, in opposition to those who adhere to materialist metaphysics, I caution that there is no reason to believe that the possibilities of things are exhausted by their material possibilities.

Within the Kuhnian picture, the object of scientific inquiry is phenomena *qua* grasped under a strategy. Since a strategy is a methodological innovation of scientific practices, this object is not ahistorical. For Kuhn himself, the aim of science is to solve puzzles whose very definition is strategy-bounded. In the final analysis the very questions posed in scientific inquiry are not about the 'material world,' but about the power of a strategy to grasp phenomena. It follows that suppositions 1 and 2 (see previous section) are not sustainable. On my additional gloss, the aim of science is to gain understanding of phenomena, and this includes encapsulating the possibilities that they allow.[16] But phenomena allow many and varied kinds of possibilities, not all of which can simultaneously be co-actualized or even co-investigated – so actual scientific investigation opts to pursue certain classes of *valued* possibilities, generally those valued for the sake of application, whose realizability and possibility of being investigated is historically conditioned. Thus the unsustainability of suppositions 1 and 2 is reinforced.

## 2.2 *'Fruitfulness' as Ground for Adopting a Strategy*

What about supposition 4? It could not be sustained, if (for example) the (rational) grounds for adopting a strategy include that it gives rise to applications of special interest for those holding a particular value-outlook. So we must attend to the grounds for adopting a strategy.

According to Kuhn, a strategy is adopted for the sake of defining and solving puzzles, or (as I prefer to put it) for the sake of generating theories and acquiring appropriate empirical data so that theories can come to be accepted in virtue of manifesting the cognitive values highly. Then, a strategy is worthy of adoption only if it is demonstrated to be *fruitful* – actually to be, and continuing to be, a source of theories that come to be soundly accepted of certain domains of phenomena. A fruitful strategy, adopted in the first instance following an exemplary achievement, enables investigation to take place in the relevant field;[17] and, for Kuhn, so long as a strategy remains fruitful, research should be conducted *exclusively* under it. Within the scientific tradition, he maintains, fruitfulness is sufficient, as well as necessary, for the adoption of a strategy. *Normally* a currently fruitful strategy is in place. Then, engaging in scientific research implies adopting it – so that normally questions about adoption of strategy are neither controversial nor addressed explicitly within the scientific community[18] – until such time as the limits of its fruitful unfolding are reached. Such limits become apparent when *anomalous* phenomena (which have become considered important for the unfolding research) are identified: phenomena that cannot, after prolonged and skilful investigation, be fitted into theories that both meet the constraints of the strategies and manifest the cognitive values highly, but at best into theories that retain empirical adequacy at the price of increasingly diminished manifestations of such other cognitive values as predictive and explanatory power and keeping *ad hoc* hypotheses to a minimum.

On Kuhnian views, strategies and the lexicons they bear are human creations; and a soundly accepted theory is one that succeeds in fitting certain phenomena of the world into the structured lexicon of a strategy. So it is expected that any strategy will have limits, that its fruitfulness will eventually become exhausted. (Any one kind of strategy will fail to encapsulate various kinds of possibilities of phenomena.) When the limits of an established strategy are reached, and – according to Kuhn – (allowing a certain latitude of judgment about when they are reached) only then, does the scientific tradition license the search for another strategy; and then the search is for a new strategy that can grasp the anomalies of the old one. At such (*revolutionary*) moments most of the old constraints are lifted, conflicting perspectives are engaged and there

is much trial and error, until such time as a new strategy emerges (in a new exemplary achievement that offers promise of further fruitful developments) that enables the grasp of the old anomalies.[19] Then that strategy comes to demand the allegiance of the scientific community.

Kuhn intends his picture to be both descriptive (under idealization) of the history of science, and normative for scientific practice. Indeed, normally it does suffice for scientific research to proceed under a single strategy, *provided that* one accepts that what count as scientifically interesting phenomena are defined within the unfolding tradition of science, and that the aim of science is to resolve puzzles about them or to come to accept (soundly) theories of them. Proceeding in this way enables there to be successful research, practically ensures that empirical considerations will eventually lead to clear demarcation of the limits of the strategy, and keeps a measure of continuity – through the special role accorded to anomalies of old strategies – across the 'revolutionary' divides that separate the periods of hegemony of succeeding strategies. Note that an argument cannot be extracted out of this that the new strategy is the only one that could have developed as successor to the old one.[20] Within the Kuhnian picture, there are elements of radical contingency: that any successor at all will actually emerge, and if one does, what its specific character will be. The emergence of a new strategy may be influenced causally by all sorts of 'extra-scientific' factors (religious, metaphysical, cultural), but what matters, what legitimates the adoption of the strategy, is that it generates theories in which the anomalous phenomena can be grasped and which define new puzzles. If the aim is to solve puzzles about scientifically interesting phenomena and to introduce new ones to be solved, that is enough.

Kuhn has provided a brilliant account of the transition from the hegemony of Aristotelian to that of materialist (Galilean) strategies[21] as well as some less developed accounts of other 'revolutionary' transitions.[22] Following the former transition, few products of Aristotelian science have remained in the generally accepted stock of knowledge. With the hindsight of developments under MS (including new data, greater sensitivity to the role of certain cognitive values, and higher standards for estimating the degree of manifestation of the cognitive values in theories), it became apparent that Aristotelian physical theories were soundly accepted of very few phenomena.[23]

Some of Kuhn's critics think that his view entails that, with the eventual anticipated surpassing of MS as framers of research, few of its products will remain in the stock of knowledge. (Thus they accuse Kuhn of a kind of relativism that seems manifestly unacceptable when we think of the discoveries of modern science and their applied successes.) But Kuhn's view does not entail this. Under MS, numerous theories have been soundly

accepted of countless domains of phenomena. These theories encapsulate well an increasing number and variety of the material possibilities of phenomena; and – while acknowledging the truism that empirical methodologies cannot provide certainty – there is no reason to hold that subsequent developments of the tradition will lead to removing from the stock of knowledge the confirmation of these possibilities. Similarly, there is no reason to anticipate (for example) that the atoms of modern atomic theory will go the way of the four terrestrial elements of Aristotelian physics, at least if we consider atoms to be the constituents of molecules with capacities for generating specified effects in specified (experimental and technological) spaces. Subsequent research may lead to their refinement and elaboration, but given how soundly accepted atomic theory is, not to their rejection. I have followed Cartwright's language here,[24] and her claim that established scientific knowledge is largely of capacities of objects: that they tend to have certain effects under specified (typically experimental) conditions, without the further supposition that such capacities (rather than others they might also have) will be exercised significantly in all ('natural') situations. Gaining such knowledge of capacities of objects does not ground the supposition that knowledge of the 'material world' – of the world as it *is* independent of its relations with human beings – has been gained. Only idle skepticism would cast doubt on the existence of atoms today: there are atoms in the world 'that we live in' and 'that we have investigated,' and we know their capacities as exercised in various experimental and technological spaces and also (no doubt) in many spaces, not of human causal origin, in which there is no (relevant) human causal involvement. Kuhn's picture fits easily with many kinds of scientific realism. But the 'world that we live in' is not the 'world as it *is* independent of its relations with human beings.'

Thus, it is consistent with the Kuhnian picture that, under MS, we gain accumulating knowledge of *'material possibilities' of phenomena*. But, one might ask, is this really any different from accumulating knowledge of *the ahistorical material world*? It is, and the difference is of central importance. In the first place, the latter idiom, unlike the former, is usually linked with the view that all possibilities of phenomena are (in the final analysis) material possibilities, and in particular with materialist reductive accounts of human cognitive (rational) and moral capacities. Secondly, the material possibilities of phenomena are those possibilities that are encapsulated by the generative power of the underlying order posited of the phenomena; they are constituted as such within scientific practices conducted under MS. Some of them are realized in, and realizable only in, experimental and technological spaces of human creation (having been posited as the possibilities of these historically

bounded spaces). Others are realized in spaces whose underlying *causal* order, as represented under MS, has no relevant human involvement, but where that causal order is posited, generally drawing upon the resources of theories accepted of experimental spaces, following scientific observation aided by instruments themselves authenticated in the course of experimental and technological practices. (In some spaces there can be good reason to hold that their possibilities are exhausted by their material possibilities.) This causal order is constituted in the course of scientific practices as a projection from experimental and technological practices; there is no basis here to infer to the features of an underlying order that is *ontologically* independent of human beings.[25]

For a theory developed of a domain of phenomena under MS, 'T manifests the cognitive values highly of D' implies 'T soundly encapsulates material possibilities of D.' These propositions become established at a particular time. Nevertheless, once established, especially if further vindicated by the success of practical applications, there is no *general* reason to expect that they will become vulnerable to refutation in the light of outcomes of research under different strategies, either current alternative strategies or future ones. Strategies change, and so the fundamental character of ongoing scientific investigation changes, but that permits a permanent residue of knowledge to remain, a residue that may or may not become rearticulated (as a particular case or as an approximation) under a subsequent strategy. Historicity of scientific practices does not imply the historical relativity of scientific knowledge. It does make likely, however, that quite a bit of what is taken to be established scientific knowledge (but with insufficient scrutiny of the degree of manifestation of the cognitive values) will come to be recognized as not properly part of the permanent stock of knowledge. And it fits easily with the historical (and cultural) relativity of interest in applying particular items of scientific knowledge.

As more material possibilities become soundly encapsulated in theories, the greater is the range of technological possibilities opened up, a matter with profound social implications. For Kuhn, technological application remains principally a *consequence* of scientific developments, and also a source of additional empirical data to bring to bear on theories, especially by way of the instrumental partnership referred to in Section 1. That there is widespread technological application, and that it is desired, are not among the (rational) grounds for adopting MS; those grounds are (normally) solely connected with fruitfulness, and also (at 'revolutionary' moments) with being able to grasp the anomalies of the old strategy. Through this complex and subtle narrative Kuhn endorses the historicity of science: denying suppositions 1 and 2, while

retaining supposition 4, and thus preserving an essentially internalist narrative of the history of science.

## 3 The Role of Applications

'Application' refers to two interacting and not sharply separable roles that scientific theories can play in social practical life. A theory may apply *to* significant phenomena of daily life and experience; and it may be applied *in* practical activity. It applies *to* those phenomena when it is used, by way of representing them with its categories and principles, to provide understanding of them. 'Applying *to*' involves identifying (modeling) phenomena as characterized using everyday categories with phenomena as represented in the theory. A theory is applied *in* practical (often technological) activity when its posits inform such concerns of practice as the workings of things, means to ends, the attainability of ends, and the consequences of realizing the possible.[26]

In Kuhn's picture, applications are important to the unfolding of the scientific tradition only as enticement for the provision of the social, material and instrumental requirements of the conduct of research. Its credibility depends on dissociating the value, conduct and *character* of scientific practices from social and moral evaluations of applications of the knowledge they produce.

I will now offer an alternative picture in which applications (*to* and *in*) are more central than Kuhn admits. In it, particular strategies are adopted rationally (in part) – subject to fruitfulness remaining a necessary condition of their adoption – *because* they can be expected to give rise to *certain kinds* of applications.[27] Phenomena are in fact (and should be) brought to the attention of basic scientific investigation, *not only* from the scientific tradition's own unfolding (as Kuhn holds), *but also* from the realm of daily life and experience and social practice, from the 'world in which we live.' Science aims to provide understanding of phenomena and, in doing so, where appropriate to make sense of our experience and to inform our social practices. Strategies worthy of adoption should normally produce theories applicable *to* phenomena significant for current daily life and applicable *in* current social practices – though normally and desirably (for substantive and methodological reasons) the reach of scientific investigation should not be limited to phenomena involved in these applications. Many significant phenomena of daily life and social practice are not fixed across historical change and cultural variety so

that depending on the desired applications, different strategies may be needed. If so, supposition 4 could not be sustained.

In order to provide detail and credibility for this alternative picture, I will show that, for some significant phenomena, competing (fruitful) strategies are possible. I will focus upon phenomena encountered in farming practices. For them, which of the strategies becomes adopted in research depends upon the social location of the investigator, and upon the way in which applications are valued from this location. Different social locations (on the one hand, the neo-liberal global economic project; on the other, grassroots movements of poor farmers) lead to the adoption of largely different (competing) strategies.

### *3.1 Do Materialist Strategies Suffice to Shape Research?*

The modern realm of daily life and experience is unintelligible apart from the applications of knowledge gained under MS, since it has been shaped to a great extent by identifying and realizing novel material possibilities of things. That provides a good reason for MS to be adopted in the scientific community.[28] But, the possibilities of natural phenomena encountered in daily life and social practice are not reducible to their material possibilities, those they have in virtue of the generative power of their underlying structure (and its components), process, interaction and law. Why, then, prioritize material possibilities in the investigation of natural phenomena? Why not attempt to shape and adopt strategies under which other classes of their possibilities might be identified, for example those they have in virtue of their places in human life and experience and social/ecological systems? Why, for example, prioritize investigating seeds so as to identify the possibilities open to them under the genetic modification procedures of current biotechnology, rather than those they have in virtue of their place in productive and sustainable agroecological systems?

The following answer might be part of the continuation of the story told in Section 1: non-material possibilities of objects (phenomena) supervene on their material possibilities. The realization of a material possibility (where human interactions with natural objects are involved) may be identical to the realization of a social/ecological possibility. Successfully producing genetically modified seeds and reaping a harvest from them, for example, is also at the same time (under current socioeconomic conditions) furthering the process in which seeds become commodities.[29] But the class of material possibilities can (in principle) be identified simply in terms of the generability of each of its members from the underlying order. Some material possibilities may also be identified *qua* social possibilities – but *systematically* all of them may be

identified from the generative power of the underlying order. Methodologically, then, it is proper to *separate* the investigations of material and other kinds of possibilities; and so to confine the latter to (for example) the various social sciences, in which the conditions, interests served and other consequences of realizing material possibilities are investigated. The same natural science suffices for all value-outlooks – for, regardless of value-outlook, all of the possibilities of interest are, or supervene on, material possibilities.

This answer leaves it open that particular strategies of the social sciences might be linked with particular value-outlooks, but not those of natural science. Natural science, according to the continued story, is *neutral*: that is, the projects of virtually any value-outlook (for example, of corporation or of movement of poor farmers) can make use of some applications made available by science in ways that strengthen or further its expression; and (in principle) the applications made available by science can serve all (currently contested) value-outlooks in an even-handed way.[30]

I take it to be uncontroversial that a considerable body of scientific knowledge gained under MS (molecular chemistry, viral and bacterial causes of disease, soil nutrients, the components of a nutritious diet, electromagnetic radiation – to give a sample) is available to be applied in ways that can strengthen the social expression of virtually any value-outlook that is actually entertained today. This explains why it is widely valued (across value-outlooks) that scientific knowledge has been gained under MS, and it provides a reason for the esteemed place that research under MS has throughout the scientific community. It does not follow that research conducted exclusively under MS (or that all research conducted under it) is valued, as distinct from inquiry in which research under MS is balanced by (or subordinated to) research conducted under alternative strategies. That is because, in contradiction with our story, the products of research under MS are in fact not neutral; the 'even-handedness' condition is not satisfied. Overall, and especially in fields like agrobiotechnology in which research is dominated by specific versions of MS, their applications favor those value-outlooks whose central practices and projects are conducted so as to further the expression of a *distinctive way of valuing control of natural objects and phenomena.* I call this distinctive way 'the modern valuation of control' (MVC).[31] MVC concerns the scope of control, its centrality in daily life, its relative unsubordination to other moral and social values, and the deep sense that control is the characteristic human stance towards natural objects; so that the expansion of technologies (informed by knowledge gained under MS) into more and more spheres of life and into becoming the means for solving more and more problems is highly valued.

Favoring value-outlooks that contain MVC violates even-handedness because the value-outlooks of various contemporary movements and groups contest MVC; and theories, consolidated under MS, do not apply *to* key phenomena and *in* significant parts of projects of importance to them. Feminist, environmental and anti-'globalization' groups bear value-outlooks that contest MVC, and also (of special interest for my argument) grassroots organizations in Latin America who adopt alternative value-outlooks that emphasize such values as '*local empowerment*,' full recognition of the entire body of human rights specified in international documents, and environmental sustainability.[32] In the agricultural projects of the grassroots organizations, phenomena of sustainable productivity, preservation of biodiversity and meeting the food and nutrition needs of the local community are of central importance, and their practices aim to preserve and enhance productive and sustainable agroecosystems over the long haul.[33] Theories developed under MS have important applications *to* these phenomena and *in* these practices, but they are limited (or subordinated); for example, they have supplied knowledge of some of the constituents and mechanisms of agroecosystems (micro-organisms, chemical nutrients), but they shed little light on the possibilities of enhancing agroecosystems – in contrast, for example, to that they shed on relations between crop yields and chemical inputs to production, and on the possibilities of production with transgenic seeds.

How can that be? Do not agroecological possibilities (like all social/ecological possibilities) supervene upon material possibilities? Despite the way our story continues, I am aware of no compelling argument that they do. Even if they do, however, it does not follow that they supervene upon material possibilities that (even in principle) may be identified under the kinds of MS (with their accompanying lexicons) that it is within human powers to develop. Some agroecological possibilities may supervene on material possibilities of such complexity, subtlety and variability with locale that human beings may not be able to identify them in the course of research conducted under MS. Be that as it may, numerous material possibilities certainly evade the grasp of theories currently or foreseeably accepted under MS. These theories also are unable to identify the agroecological possibilities, whose realization is sought in projects expressive of the values of 'local empowerment.' (This is a symptom of the lack of *neutrality* of the products of MS overall.) If these agroecological possibilities do supervene upon material possibilities, and if the latter are to be identified, then (at least for the time being) it will be *qua* agroecological possibilities, and not *qua* generable from the underlying order. It will be *qua* possibilities that things have in virtue of being part of a more or less self-regulating system, in virtue of relations and interactions

they have with other constituents of the system and the role they play in its self-regulation. In so far as such agroecological possibilities may be pertinent to desired applications, identification of them will have to be gained through research conducted under alternative strategies – agroecological strategies (AES).

It is now clear why the products of research under MS can not meet the even-handedness condition. Applying current knowledge gained under MS (for example, in agrobiotechnology) may require conditions that would undermine the valued agroecological systems. It remains that knowledge gained under MS is genuine knowledge; it is expressed in soundly accepted theories or with the aid of their categories. When alternative strategies are adopted, and their results applied, that remains untouched. *Consistency* with soundly accepted theories is a mark of the rational; *applying* them need not be. Legitimation of applications involves not only that the theory has been soundly accepted, but also that its applications serve the interests of the 'right' value-outlook. When a theory is applicable only in a context where certain values are expressed and embodied, to appeal to its sound acceptance as sufficient legitimation for application implies improperly limiting the range of values that may be (rationally) held. When we separate the investigations of material and other possibilities, we study things in abstraction from the conditions for the realization of their possibilities; so it will not be part of the 'technical' investigation to figure out the social conditions under which the possibilities may be realized – so we may miss that to interact with a thing so as to realize certain of its material possibilities may actually be also to treat it as a certain type of social object.

### *3.2 Agroecological Strategies*

AES and agrobiotechnological strategies (BTS) compete.[34] Their established theories are not inconsistent, but they encapsulate largely different classes of possibilities, which (for the most part) cannot be co-realized in the same fields. The competition concerns which class of possibilities to attempt to realize in agricultural practices: those of biotechnology which are of special interest where MVC reigns, or those of agroecology whose interest derives (in the first instance) from the values of 'local empowerment'? 'Technical scientific' issues pervade the competition: What is possible? What are the risks of application? Can the risks be suitably managed? But, provided that both BTS and AES are fruitful, the conflict is waged in the realms of values, politics, economics and so on – and where one stands in face of this conflict feeds back into the strategies one adopts in research.

Agroecologists clearly recognize this; biotechnologists often do not.[35] Sometimes it is said that the 'theory' of agroecology consists simply of ideological critique, or at best of a patchwork of opportunistically gleamed fragments of traditional local knowledge – so that the proponents of agroecology are said not to be proposing a 'scientific' research program but instead to be submitting scientific claims to ideological critique. This criticism of agroecology ignores that research under AES has been fruitful (see below), and that in fact it itself is 'ideological' rather than 'scientific.' Research under both BTS and AES gains understanding of phenomena of the world and their possibilities – and aims to do so, as well as to gain understanding pertinent to value-laden interests in application: '... political determinants enter at the point when *basic* [my italics] scientific questions are asked and not only at the time when technologies are delivered to society'.[36] If they do not appear to do so 'equally', that may be because inequalities of available material and social conditions enable research under BTS to proceed routinely without its legitimation constantly being called into question.[37] The reasons both for and against the adoption of AES, and conversely for the exclusive adoption of variants of MS, include integrally appeal to value-outlooks. The strategies are *equally* 'scientific': held to fruitfulness, and adopted (in part) because of their relations with value-outlooks. There is *not* the asymmetry that critics of agroecology sometimes claim: under MS, investigation is scientific and non-ideological, whereas research under AES is non-scientific and ideological.[38]

Those who adopt BTS, misled by the myth of *neutrality*, tend not only to downplay the empirical achievements of agroecology and to portray it as simply an ideology without link with fruitful strategies, but also to be unaware that the links of biotechnology with MVC refute the *neutrality* they claim for their own research. For them, BTS are simply particular instances of materialist strategies that enable us to identify the possibilities of things (for example, seeds) that are made available principally from using (for example) methods of genetic modification. BTS are indeed that; they are *also* those strategies whose products do and are expected to inform a particular form of technology, that is widely and almost entirely applied in practices that express highly MVC. The first description of BTS shapes research practices; the second serves to rationalize adopting them rather than other strategies.

Similarly, AES have two descriptions: first, as particular instances of general ecological strategies – that frame research on the relations and interactions between an organism and its environment, considered as a more or less self-regulating 'whole' of which the organism is an integral part – that enable us to identify the possibilities that things (seeds) have in virtue of their place in agroecological systems; second, as those strategies that are intended

to provide knowledge that can inform the agricultural projects expressive of the values (for example) of 'local empowerment.'[39]

It is in virtue of the first description that AES shape research practices. I will elaborate a little. Under AES, research aims to confirm generalizations concerning the tendencies, capacities and functioning of agroecosystems, their constituents, and relations and interactions among them. These include generalizations in which (for example) 'mineral cycles, energy transformations, biological processes and socioeconomic relationships' are considered in relationship to the whole system; generalizations concerned not with 'maximizing production of a particular system, but rather with optimizing the agroecosystem as a whole' and so with 'complex interactions among and between people, crops, soil and livestock.'[40] To illustrate:

> low pest potentials [are likely] in agroecosystems that exhibit the following characteristics: high crop density through mixing crops in time and space; discontinuity of monocultures in time through rotations, use of short maturing varieties, use of crop-free or preferred host-free periods …; small, scattered fields creating a structural mosaic of adjoining crops and uncultivated land which potentially provides shelter and alternative food for natural enemies …; farms with a dominant perennial crop component …; high crop densities or the presence of tolerable levels of specific weed species; high genetic density resulting from the use of variety mixtures or crop multilines.[41]

And:

> Restoration of natural controls in agroecosystems through vegetation management not only regulates pests, but also helps to conserve energy, improves soil fertility, minimizes risks, and reduces dependence on external resources.[42]

Of particular salience are generalizations that help to identify the possibilities for productivity and sustainability of agroecosystems, where 'sustainability' has been defined in terms of four inter-connected characteristics: *productive capacity*: 'Maintenance of the productive capacity of the ecosystem'; *ecological integrity*: 'Preservation of the natural resource base and functional biodiversity'; *social health*: 'Social organization and reduction of poverty'; *cultural identity*: 'Empowerment of local communities, maintenance of tradition, and popular participation in the development process'.[43]

*Theories*, under AES, may be considered to be constrained so as to be able to represent sets of generalizations of the above kinds,[44] and the hypotheses (drawn from general ecological theory) that are entertained for their

explanation and determining the limits of their application. The generalizations of agroecology tend to express probalilistic relations or tendencies, and they may have greater or less specificity. So, discerning the limits of application of these generalizations is especially important. Note how the generalization '... enhancement of biodiversity in traditional agroecological systems [in Latin America] represents a strategy that ensures diverse diets and income sources, stable production, minimum risk, intensive production with limited resources, and maximum returns under low levels of technology' is later qualified by '... we have still not been able to develop a predictive theory that enables us to determine what specific elements of biodiversity should be retained, added, or eliminated to enhance natural pest control.'[45]

*Data* are selected and sought out in virtue of their relevance for appraising these theories and for enabling phenomena, relevant in the light of the values of 'local empowerment,' to be brought within the compass of a theory's applicability. Obtaining the data often requires subtle, regular, painstaking, accurate observation and monitoring of a multiplicity and heterogeneity of details in the agroecosystems. The skills for this are usually only developed by local farmers themselves, so that gaining the data depends on the collaboration of local farmers and the utilization of their experience and knowledge, and the lexicon in which they are reported will reflect the distinctions and categories of this experience. Agroecology cannot be pursued with a sharp distinction between the researcher and the farmer; the farmer's observations are essential to the conduct of the research. Quantitative data are often pertinent: counting the number of pests in a given area, measuring the size of crop yields, amount of water available and so on; statistical comparisons (for example) of pest populations across (for example) monocultures and polycultures, or of the yields of different crops when different methods are used. Experimental data are sought both to support statistical comparisons, *and* to demonstrate that possibilities can be realized in agroecosystems with certain characteristics, for example: '... it is possible to stabilize the insect communities of agroecosystems by designing and constructing vegetational architectures that support populations of natural enemies or have direct deterrent effects on pest herbivores.'[46] In agroecological contexts, an 'experiment' involves introducing, for the sake of observing its systemic effects, a modification (under an investigator's control) of an agroecological system. Given the local distinctiveness of agroecosystems, the mark of a 'good' experiment cannot be its replicability across diverse environmental and social conditions. Note that control is involved in agroecological experiments and farming practices, but subordinated to the values of 'local empowerment.'

Relevant data are often obtained from the study of farming systems in which *traditional methods informed by traditional local knowledge* are used. These systems are appropriately submitted to empirical scrutiny because agroecological studies have shown 'that traditional farming systems are often based on deep ecological rationales and in many cases exhibit a number of desirable features of socioeconomic stability, biological resilience and productivity.'[47] They exemplify many known agroecological principles and others can be expected to be extracted in the course of studying them.[48] They can (with adaptations suggested by research findings) be enhanced with respect to all four of the characteristics listed above, and especially with respect to 'cultural identity' they are often uniquely appropriate for the activities of poor, small farmers. It is worth noting that the methods used in these systems have been tested rigorously in practice, and have been particularly effective (reflecting the experimental approach of traditional farmers) over the centuries in 'selecting seed varieties for specific environments'[49] – these are often the seed varieties (or the original sources of them) that are modified genetically in biotechnology research and practice.[50]

### *3.3 Adopting a Strategy and the Social Location of Scientific Research*

At least in some fields, there can be multiple strategies that compete in the way described above; and each of the competing strategies may be fruitful. Then there arises the question of which strategy to adopt, one for which different answers may be proposed and acted on by different investigators. I have suggested that actual answers (explicitly or implicitly) draw upon mutually reinforcing relations between adopting strategies and the value-outlooks whose interests would be served especially well by applications of knowledge gained under the strategies.

There is, however, a general reason to opt in favor of developing research under *some* strategies other than MS: to test whether all possibilities, or all material possibilities upon which non-material possibilities have been assumed to supervene, can become grasped under MS. By identifying possibilities that are not identical with possibilities currently encapsulated by soundly accepted theories under MS, we can pose concrete challenges for research under MS to meet. This reason would not appeal to Kuhn; he holds that such challenges are unnecessary since, in due course, anomalies will accumulate in the normal unfolding of MS. However, there may be bounds to MS, while within the bounds there remain unlimited possibilities to be identified. Only tests of the kind indicated here can hope to identify these bounds. (I am not sure that Kuhn recognized this.) By identifying possibilities of the kinds indicated, it can be

probed empirically whether or not there are bounds to the development of MS. This general reason sits in tension with the rationales for adopting specific strategies, especially when we remember that there is also competition for resources for conducting research. Resources devoted to probing the limits of MS in this way would be resources taken away from pursuing more favored projects and *de facto* giving the resources to support research whose strategies gain their primary rationale from competing value-outlooks. Only a satisfactory resolution of this tension, I believe, could restore *neutrality as* a compelling value of scientific practice.[51] The tension is heightened when we consider the legitimation of applications.

Kuhn's picture, recall, portrays applications mainly as consequences of scientific developments; and developments under MS have identified numerous material possibilities that have become, and are continuing to become, realized in applications at an increasingly rapid rate. The *efficacy* of applications depends on the input of sound scientific knowledge that can be provided (for many applications) by research under MS. The *legitimation* of some applications depends also on claims about the possibilities of things. Consider: under MS, means (involving developments of biotechnology) may be identified for producing food sufficient in quantity to continue to feed the world's population. Applying the knowledge thereby obtained is legitimated, however, only if there are no 'better' ways of producing sufficient amounts of food – ways, for example, that would be part of agroecosystems that were structured so that the food is not only produced, but also so that everyone is actually fed sufficiently and nutritiously, and that sustainable (and improving) and productive agroecosystems are maintained.[52] But the possibility of producing sufficient food by *developed and expanded* uses of agroecological methods cannot be investigated under MS. So research under MS cannot provide a crucial item of knowledge (or the means for attempting to gain it) needed to legitimate endorsing biotechnological methods as essential to the solution of the world's food problems.

The proponents of bringing biotechnological methods to the core of agricultural production respond that there is *no* evidence that developed agroecological methods could produce sufficient food. In responding to this assertion it is important to keep in mind that producing sufficient quantities of food to feed everyone does not imply that everyone will be fed. Currently, sufficient food is produced, but hunger persists.[53] Given that agrobiotechnology plays an integral role in the global economic system, under which hunger currently persists, one might wonder why the expansion of production of food promised by the new methods will be any more likely to lead to the hungry being fed. Who is fed, and who is not, is not independent of the methods of

production. Even if there were strong evidence that enhanced agroecological methods could not produce sufficient food to feed everyone, it might still be the case that agroecology needs to be developed so that the currently hungry and their descendants will be fed. The legitimation of the furthering of agroecology needs only this more modest claim which, I believe, is the claim that agroecologists actually make. Perhaps, in order that everyone be fed, a variety of farming methods will have to be used. The issue is an empirical one, but investigations conducted exclusively under MS cannot adequately address it. It can only be responsibly investigated within a theoretical framework that investigates the full causal nexus of production and consumption of food, and more generally of human well-being, and in a process that is responsive to the needs, interests and value-outlooks of everyone.

As things stand, it is true that the evidence is *less than compelling* that agroecological methods can be enhanced and expanded to produce sufficient food to feed everyone. However, that could be because, while AES have displayed a measure of fruitfulness, their limits have effectively been reached; or because, due to lack of the necessary social conditions and material resources, there has been much less research conducted under AES than under BTS. This matter could be explored empirically by providing conditions to further develop agroecology in those areas where there is hunger and an available rural workforce (thus furthering it under the legitimation of the modest claim referred to in the previous paragraph, in areas where its effectiveness has been repeatedly demonstrated;[54] this would enable virtually risk-free investigation of the possibilities of agroecological production.

The proponents of biotechnological methods see little urgency in conducting such an investigation because, I think, the widespread implementation of agroecological methods would be incompatible with the social structures, values and policies under which biotechnology is developing. For them, agroecological methods cannot produce sufficient food because they cannot be developed under these social conditions. There is, for these proponents, no *better* way to produce the needed food, because biotechnological methods are confirmed as providing the most efficacious of the available possibilities whose realization could be informed by theories established under MS, and thus could most usefully further the expression of MVC; and, for them, furthering MVC (and so, at the present moment, fitting into the neo-liberal global economic project) has become a condition on a legitimated way. In short, the legitimation available to be offered of prioritizing biotechnological methods – without begging questions whose empirically grounded answers require developments of AES – does not rest upon empirical confirmation that agroecological methods are insufficient for producing the food. It rests upon

commitment to MVC (and the values of 'globalization'), upon valuing the material possibilities of biotechnology because to realize them is, at one and the same time, to realize valued social possibilities.

There are no 'scientific' reasons to decline to appraise empirically the possibilities of agroecology. Given that applications involve issues not only of efficacy, but also of legitimation or social value, it is just arbitrary to insist that what counts as a 'scientifically' interesting phenomenon is determined only in view of the internal unfolding of the scientific tradition and not also by interests connected with application. Thus, the competition between BTS and AES cannot be dissolved by appealing to the general character of science. The marginalization of AES in the mainstream, I have suggested, is explained (when we probe for the reasons) not because, after adequately providing for efforts to develop them, serious doubts about their fruitfulness have been confirmed. Rather it is because they cannot lead to applications of interest for MVC; and perhaps also because, if their fruitfulness were confirmed, the legitimation of prioritizing biotechnology in agriculture would be challenged – though in fact the proponents of biotechnology tend not even to entertain that the far-reaching fruitfulness of AES might, given the opportunity, be confirmed.[55] Conversely, the reasons for adopting AES (which, I repeat, draw upon basic knowledge gained under MS in all sorts of ways) as an alternative to BTS are connected with critique of MVC and with holding such competing value-outlooks as that of 'local empowerment.' Either way, adoption of strategies, and thus the character of research conducted, is unintelligible if separated from the social location of scientific practices and their applications; and thus, in turn, social location can serve as a ground (but not one that downplays the importance of fruitfulness) for critique of scientific practices, and as a source and condition of alternatives.

## 4 Conclusion

The object of scientific inquiry is phenomena as grasped under a strategy, so much so that it varies with strategies and cannot be characterized in strategy-neutral terms. That is Kuhn's insight. Strategies, a key component of scientific methodology, are historically variable, and so too is the object of scientific inquiry.

To understand phenomena is to describe and explain them, *and* to identify the possibilities they admit. A strategy has the resources to identify a particular class of possibilities. Any one strategy is worthy of adoption only if, given the opportunity and appropriate resources, it shows itself to be fruitful: that

is, if it is successful in actually identifying possibilities of the relevant class (encapsulating them in soundly accepted theories). Competing strategies explore classes of possibilities (often of the same phenomena, for example, seeds) that cannot be co-realized: for example, realizing (to any significant extent) the possibilities of transgenic seeds cannot be co-realized with certain possibilities of productive and sustainable agroecosystems.

When two fruitful strategies compete, what are the reasons to adopt one of them rather than the other? Since both are fruitful, reasons based exclusively on cognitive (epistemic) value cannot favor one rather than the other. As illustrated in the discussion of Section 3, my answer is: Adopt the one that enables us to gain understanding that is applicable *to* phenomena and (where appropriate) *in* practical projects of significance for our value-outlooks, thus the one that identifies possibilities that, if realized on application, would further these projects. This provides a good reason to adopt a strategy without, at the same time, denying that the scope and value of the basic understanding gained in scientific research transcend interest in applications. It is a reason that points to the (social) value of research conducted under the strategy.

Both fruitfulness and applicability are necessary conditions for the adoption of a strategy. We adopt a strategy partly for the sake of gaining theories that are applicable in ways that are significant for our value-outlooks. In a particular field of research, there may be no relevant disagreements across value-outlooks about what are the phenomena and projects for which applications of theories are desired. Then competing strategies are unlikely to emerge. Different value-outlooks, however, may (in some fields) lead to different appraisals of the significance (social value) of applications, and thus to their respective adherents adopting competing strategies (for example, AES and BTS). Where this happens, a case can be made that a plurality of strategies should appropriately be supported within the whole scientific community (despite the resulting tensions that would be occasioned by the fact that the classes of possibilities being explored are not co-realizable in the same contexts). Moreover, if my suggestion is correct, it will be no surprise that one kind of strategy comes to be adopted virtually exclusively in the scientific community, and that adopting a strategy is not generally recognized as a matter of choice or as in need of rational support, when in it and its supporting institutions there is hegemony of values (for example, MVC or those of the global economy).

According to the picture I have offered, application plays a central role in shaping scientific practice. It is not just a consequence (or instrumental partner) of successful research, but where it is valued in social practices that one endorses, it is part of the very reason to adopt a strategy. We might put it: Possibilities, in so far as they are identical to possibilities for application,

*partly* constitute the object of scientific inquiry. Such possibilities, of course, are objects of social value and historically and culturally variable, functions of the social location of the scientific practices. The strategies we adopt are those suitable for exploring these possibilities, and so they too must vary as a function of the social location of scientific practices. Thus, applications – successful, desired, anticipated – feed back so as to influence at the most fundamental methodological level the way in which scientific investigation is conducted.

## Notes

1 I wish to thank the National Science Foundation (SES–9905945) for partial support of the writing of this paper, and Richard Eldridge and the editors for helpful comments.

2 Margolis (1995), p. 321.

3 At the present moment this supposition seems to be rapidly losing its grip. Especially in the biological sciences (for example, concerning the Human Genome Project), the components of the underlying order are increasingly becoming thought of as objects to which one may hope to obtain intellectual property rights; thus, among other things, as historically constituted property and commodities.

4 See Lacey (1999a), Ch. 6.

5 Ibid., pp. 124–6.

6 Supposition 3 will play no role in the present argument. Properly articulated, using the notion of cognitive values, it may be compatible with the denial of supposition 4 – see my defense that theories, regardless of the strategies under which they are developed, may become accepted in accordance with *impartiality* (ibid., Ch. 10).

7 Ibid., pp. 13–14.

8 Cognitive values are the criteria of the cognitive or epistemic value (rational acceptability) of a theory. I have discussed them extensively elsewhere (ibid., Ch. 3), and defended that they can be distinguished from social, moral and other kinds of values (Lacey, 1999b). I will not address how the history of the cognitive values might bear upon the historicity of science; this may have implications regarding supposition 3 that I will not entertain here.

9 Lacey (1999a), Ch. 6.

10 Ibid., Ch. 7.

11 Kuhn (1970). For the history of Kuhn's use of 'paradigm' and the terms he introduced in subsequent work to refine and replace it ('disciplinary matrix', 'structured lexicon'), see Hoyningen-Huene (1993) and Sankey (1994). I have elaborated the notion of strategy (my terminology, not Kuhn's), as well as ideas introduced in the next few paragraphs, in detail elsewhere (Lacey, 1999a; 1999b).

12 Under certain conditions, that are connected with applications (see next section), theory choice may be made across strategies (Lacey, 1999a, Chs 7, 10; 1999e).

13 Lacey (1999a), pp. 62–6.

14 I have emphasized elsewhere (ibid.) the variety to be found among materialist strategies. Not all of them represent reductionist tendencies, and not all of them require that laws be deterministic. I say that MS are adopted 'virtually exclusively' in modern science:

ecology (which is not confined to deployment of MS) usually is considered to fall under the umbrella of modern science.

15 Kuhn (1970) has developed this idea in a rich way using the phenomenological notion of 'world.' The objects of the 'world' in which investigations under MS are conducted are partly constituted by the practices themselves. I have discussed this in Lacey (1999a, Ch. 7; see also Lacey, 1999c; 1999e).

16 Lacey (1999a), Ch. 5.

17 I will not keep repeating the qualification: 'in the relevant field'. It applies to all the remarks about strategies (and research framed by a strategy) that follow in this chapter.

18 That helps to explain why often it is thought that science *is just* that inquiry conducted under the currently dominant strategies. (Modern science *is* inquiry conducted under MS.) Since normally there is no controversy about MS in the scientific community, their role can easily remain hidden so that it is not recognized that there may be other strategies, and investigation that might be being conducted under another strategy tends to be dismissed as 'unscientific.' This theme comes up again in Section 3: *agroecological strategies* (see also note 55).

19 Cf. Hoyningen-Huene (1993), pp. 241–3.

20 Lacey (1999a), pp. 172–5; 1999e.

21 Kuhn (1956).

22 Kuhn (1970).

23 Lacey (1999a), Ch. 7.

24 Cartwright (1999).

25 Of course, this does not prove that there is no such underlying order, as Sankey (1997) – see Lacey (1999c) – has emphasized.

26 Lacey (1999a), 14–15.

27 Here I only present my alternative picture, showing how it illuminates an important contemporary controversy. See Lacey (1999a, Ch. 7; 1999e) for fuller argument.

28 Endorsing the values expressed and furthered by the prevailing social order *may* provide a good *reason* to adopt MS virtually exclusively – of course, the reason is only as good as the grounds for endorsing these values (Lacey (1999a), Ch. 6). That they are widely endorsed, and expressed deeply in dominant modern economic and political projects, may largely *explain* that scientific research is conducted almost exclusively under MS.

29 Kloppenburg (1987); Lacey (1999a, Ch. 8) and the references there.

30 Lacey (1999a), Ch. 4; 1999b.

31 See Lacey (1999a), pp. 111–30, for a detailed analysis of MVC and for the argument – also Lacey, 1999d.

32 Lacey (1997); Lacey (1999a), Ch. 8.

33 Control of natural phenomena is, of course, a value for them – as it is in every culture – but, unlike in MVC, it is subordinated to the listed core values.

34 My account of *agroecology* here is derived from the numerous writings of Altieri (especially Altieri, 1995) with some adaptations of terminology (that involve little strain) so as to fit into my general analytic framework. (See also Lacey, 1999a: Ch. 8.)

On *biotechnology*: '... in essence [biotechnology] implies the use of microbial, animal or plant cells or enzymes to synthesize, breakdown or transform materials. ... Traditional biotechnology refers to the conventional techniques that have been used for many centuries to produce beer, wine, cheese and many other foods, while "new" biotechnology embraces all methods of genetic modification by recombinant DNA and cell fusion techniques, together with modern developments of "traditional" biotechnological processes' (Smith, 1996: 2–3). In the text I am using 'biotechnology' in the sense of 'the "new" biotechnology.'

I am also taking it to refer to a field of scientific research: that conducted under BTS, research that aims to produce knowledge that can enhance the methods specified in the quote. Thus, depending on contest, 'biotechnology' may refer either to a field of scientific research or to specific methods deployed in agricultural practices.

Note that the way in which AES and BTS compete does not preclude that each may draw from the positive results of the other *in limited respects*. In this chapter I will not explore whether and how this might happen. On the relationship of the kind of competition, exemplified by that between AES and BTS, with what Kuhn has called 'incommensurability,' see Lacey, 1999e; and see the next paragraph for how it involves the difficulties of communication that Kuhn diagnoses to be part of incommensurability.

35 See the exchange between Altieri and Rosset (2000) and McGloughlin (2000).

36 Altieri (1994), pp. 150–51.

37 See note 18 above.

38 McGloughlin (2000).

39 In both cases (BTS and AES) the adopting of the strategies is rationalized (in part) by reference to particular values. This does not *per se* challenge the *impartiality* (sound acceptance) of the results consolidated under either strategy; it may their *neutrality*. In the case of AES, since objects (including agroecosystems themselves) are not abstracted from their places in human experience and social relations, values enter into the subject-matter of the investigation: under what conditions are certain values (for example, social justice, cultural identity) able to be further embodied? (Under MS, all comparable questions are pushed into the social science inquiries that may inform applications.) Note that the questions (posed under AES) are about the degree of embodiment and manifestation of the values; reaching empirically based results about them (as distinct, perhaps, from having an interest in them) is logically independent of endorsing the values. There can be *impartial* results about the degree of manifestation and embodiment of values (Lacey, 1999a, Ch. 2).

40 Altieri (1987), pp. xiv–xv.

41 Altieri (1999), pp. 24–5.

42 Altieri (1994), p. 150.

43 Altieri et al. (1996), pp. 367–8.

44 See Lacey (1999a), pp. 193–6 for further discussion.

45 Altieri (1994), pp. 7, 38.

46 Ibid., p. 7.

47 Altieri (1987), p. xiii; for details and examples, see Altieri, 1995: Ch. 6.

48 Altieri (1995), p. 143.

49 Ibid., p. 116.

50 Kloppenburg (1987); Lacey (1999a), Ch. 8.

51 Lacey (1999a), Ch. 10.

52 Altieri and Rosset (2000); Kloppenburg and Burrows (1996). Other important issues are also involved in the legitimation of prioritizing (or even using) biotechnological methods: for example, concerning possible undesirable health and environmental side-effects. They have been widely discussed (see, for example, Rissler and Mellon, 1996).

53 Boucher (1999).

54 Altieri et al. (1996).

55 My explanation is consistent with it being the case that, in the consciousness of researchers in biotechnology, they are simply following through on the latest options provided under MS, with no issue of choice of strategy involved (see note 18). If there is no choice of strategy, then criticism of biotechnology becomes seen simply as criticism of engaging

in research *per se*, or as having the effect of threatening funds for research. Some recent reactions of biotechnology researchers to criticism have been of this kind. Ironically, they see threats to the 'autonomy' of science coming more from their critics than from the corporate sponsors of much of their research.

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