The Background to the Forefront: A Response to Levi and Shapere

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The papers by Isaac Levi and by Dudley Shapere, despite their many differences, share a common theme. This is the idea that science in a certain sense builds upon itself, that some of its assertions, having become firmly established, play important roles in the further development of science. Established science not only guides practical action, it also constrains possible conjectures, guides the construction of more extended and related theories, and even guides the construction of their eventual successors (though this last needs very careful handling). This basic idea is surely correct; but I have many disagreements with the particular (and different) ways that the basic idea is developed by Isaac Levi and Dudley Shapere. I shall therefore first outline very roughly the sort of development of this basic idea that I would advocate. And then, ignoring many disagreements over details, I shall identify one general point on which the version I advocate differs radically from Isaac Levi's and one general point on which it differs radically from Dudley Shapere's. In a nutshell, I shall argue that scientific rationality and the process by which science builds upon itself can (and indeed must) be explained *without* deliberately making ourselves myopic (I shall invoke a version of realism which is strictly 20-20) and without falling prey to Shapere's 'bootstrappism' - in my view a disorder as severe as myopia.

1. The different parts of background knowledge

Isaac Levi refers to firmly accepted statements as providing a "standard of serious possibility"; Dudley Shapere refers to them as "background suppositions" or "background information" used in arguing to new theories. Either way, these accepted statements play important though rather complicated, roles in deciding what else is to be accepted in science. This view has to be basically correct. To develop it properly however, we need to make a certain old-fashioned distinction within this body of "background knowledge", a distinction which both Levi and Shapere seem to believe cannot be made.

First there are observational and instrumental laws. Although "post-positivists" have certainly established the naiveté of some older views of the observational basis of science, they have, in my opinion,

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stampeded our profession into over-reaction. They are surely right that the picture of science in which radical change is restricted to the high theoretical levels is strictly incorrect. The natural way of describing even the humblest fact may undoubtedly change as a result of a "scientific revolution"; and low level laws which had previously appeared to hold quite generally and to be free from exceptions may subsequently be demonstrated to in fact suffer striking exceptions. For example, the simple law of the reflection of light could hardly have been more firmly established before it was shown by Fresnel to be only an approximation which indeed breaks down quite radically in the case of narrow reflecting surfaces.

There is, however, a natural reaction to cases like these which surely contains a good deal of truth: namely that such cases involve only modification, not rejection. The simple law of reflection - although now recognised to be always strictly incorrect - nonetheless yields results in the overwhelming majority of instances whose difference from the "truth" lies well below the level of observability. Fresnel's theory did not of course entail that earlier opticians had all been misobserving. No matter how difficult it may be to say exactly what "essential continuity" precisely means, it is surely right intuitively that there has been 'essential continuity' in science at the empirical level. It would hardly be worth saying had it not so often been denied recently, but our empirical knowledge has grown as science has developed - if not strictly, then nonetheless "essentially" in cumulative fashion.

Obviously this forms one increasingly demanding constraint on new theories: they are allowed to contradict previously accepted observational laws, but only if they at the same time explain the observational success of those laws. The explanation will standardly be that the difference between "fact" (as of course characterised by the new theory) and the prediction of the old law, in all the cases of the kind in which the old law had already proved successful, is below the observable threshold with the observational methods thus far applied.

This, then, is one part of "background knowledge". It is this part which, as I shall explain, lends Isaac Levi's talk of serious possibilities what plausibility it has. It is important to note that no "pessimistic induction" can be performed with respect to this part of "background knowledge". For all the post-positivists' propaganda, there have been, so far as I can see, no radical changes in our observational knowledge (at any rate not at the level of what Poincaré called crude facts), but only essential accumulation. Similarly our ability to manipulate nature has simply accumulated. As I remarked, I almost feel ashamed saying this - it sounds so trivial, but I do think that sophisticated arguments may sometimes make philosophers lose sight of even trivial truths.

Observational and instrumental laws by no means exhaust the "background": at any stage, certain high level theories will also be accepted parts of knowledge. For example, in the 1850s it was firmly accepted that light is some sort of wave motion in an all-pervading medium. Some detailed properties of the medium - especially the aether within transparent bodies - were open to conjecture, "up for grabs", so to speak, but the basic wave theory was regarded as firmly established. It was certainly the only active possibility - perhaps, depending on how serious is "serious", the only serious possibility. For of course the time being. The basic wave theory undoubtedly constrained allowable conjectures - in the very strong sense that any such conjecture had to be a precise version of it.

The fact that general theories, like the wave theory, can become relatively hard was recognised, of course, by Kuhn, Lakatos and others, and indeed had already been recognised fairly clearly by Duhem and Poincaré. Using Lakatosian terminology, there existed in the 1850s a wave optics research programme characterised by certain framework principles, which were relatively firmly entrenched and which constrained the development of more readily adjustable specific theories. One important point which Lakatos did not make, and which emerges very clearly from Shapere's treatment, is that many of the framework principles (especially those in Lakatos's so-called 'positive heuristic') are still more general than say the basic wave theory - and in fact cut across different research programmes. Examples of these very general principles are assumptions about the general character of any forces involved (that they are, for instance, reasonably simple functions of the distance from their source), the principles of mechanism and of determinism, and various continuity, conservation and symmetry assumptions. These more general principles tend to be even more firmly entrenched.

There is, in other words, at any one stage in the development of science a hierarchical structure of theoretical statements - stretching from out-and-out conjectures, for which there is as yet no very firm evidence one way or another, through better-established specific theories, well-entrenched general theories ("hard cores"), to even deeper-lying, more general principles of a trans-theoretic, metaphysical nature. When empirical difficulties arise, this hierarchy provides a natural pecking-order - an indication of which assumption is likely to be tinkered with first in an attempt to resolve the difficulty. Generally speaking, the more specific the theory, the more likely it is to be replaced. The scientist's first reaction seems always to be to hold onto the general framework principles (and of course the empirical results, if taken in "crude" enough form) and to search within that framework for a new specific theory which satisfactorily solves the erstwhile problem. If this search is successful - so that the scientist need never get to question the more general, deeper entrenched framework assumptions - then those framework assumptions can, as Dudley Shapere points out, be used as reasons for holding the new specific theory.

Again using optics as a source of examples: two light beams polarised at right angles to each other could not be made to exhibit interference fringes even in circumstances in which unpolarised beams did interfere; this certainly refuted the specific version of the wave theory available at the time, which made light waves longitudinal like sound waves in air; but holding on to the general wave theory, Fresnel took that general theory together with this very experimental result (of no interference) as a solid reason for the view that the optical disturbance is transverse.

Not every development in science, of course, need involve revision: it may simply be a question of extending what we already know. Again background knowledge will be relied on: perhaps the most straightforward cases of this kind are those that have often been described under the heading 'Deduction from the Phenomena'. Such "deductions" when analysed always invoke general "background" assumptions. For example, as Jon Dorling has emphasised, Coulomb argued for the inverse square law of electrostatic force by showing that it could be "deduced" from certain experimental results, but in the deduction Coulomb implicitly invoked certain general, abstract and already firmly accepted assumptions about this force: namely that it is central and dependent only on the charges and distances involved. The name "deduction from the phenomena" indicates, however, that the extra (and undeniably theoretical) assumptions involved must be so well-entrenched, so much part of "background knowledge", that only the phenomenal premises require explicit mention.

Dudley Shapere has rightly drawn attention to other kinds of case where already accepted knowledge is used in building further theories: for example, the construction of certain cosmological theories out of more down-to-earth materials.

But, for all their uses, there is an important difference between these theoretical parts of background knowledge and the observational part I discussed earlier. No matter how firmly entrenched these general theoretical assumptions may have become, and no matter how long they have guided the construction of their more specific offspring, they do seem to be liable to equally firm disentrenchment. History of science provides plenty of examples of theories at various levels of generality, which were once firmly accepted but were subsequently firmly rejected. Of course they need not disappear without trace: the mathematical equations to which once accepted theories, like Fresnel's wave theory of light or Newton's theory of gravitation, gave rise, invariably live on in science - standardly as "limiting cases". But the fully fledged theories themselves, complete with "metaphysical" commitments, have been entirely rejected. The whole idea of an elastic light-carrying medium has been overthrown, not simply modified. And as for once accepted theories of the more general kind - like that of the absolute nature of space and time or of the deterministic nature of the universe - their rejection is still more clear cut.[1]

There have been revolutions which have resulted in radical changes in the theoretical part of accepted background knowledge. I think that the only realistic way to face up to this historical fact is by admitting both the corrigibility and the fallibility of our present background knowledge. Isaac Levi thinks that if we admit fallibility we fall into an absurd sort of scepticism. I think he is wrong - as I shall argue next.

2. For 20-20 fallibilistic realism[2]

Isaac Levi elevates the body of assertions firmly accepted by science at any given time into that time's standard of serious possibility: nothing inconsistent with any accepted part of science is even a serious possibility. Indeed accepted knowledge is to be regarded as infallible - if only pro tem. It is however also corrigible - no other position, Levi admits, is consonant with the history of science. He struggles nobly to argue that the apparent inconsistency here is merely apparent. I do not believe that he succeeds.

Most of the problems concern the dynamics of change in knowledge. For one thing, since Levi allows that what he calls 'routine expansion' may introduce inconsistency into the overall corpus of knowledge, it seems that a rational agent may know for sure an inconsistency - even if only for a moment. But aside from this temporary embarrassment of internal inconsistency, there is a more pressing problem concerning the inconsistency between successive bodies of accepted knowledge. A theory *T'* may, of course, be proposed at a time when some theory *T*, which contradicts *T'*, is firmly accepted. Yet *T'* eventually displaces *T* and becomes firmly accepted in its turn. Therefore Levi needs some mechanism whereby "not a serious possibility at T" can be transformed into "infallibly known at $t + \Delta t$".

In fact he reconstructs this as a two-stage process. In the first stage, the body of knowledge is contracted so that the inconsistency with the new theory disappears. This contraction must be made entirely without reference to the new theory, which is not yet, remember, even seriously entertainable. This in itself seems entirely unrealistic - one of the reasons for the erosion of faith in an old theory may surely be the success of a rival. But there is a more immediate and more general problem with rationalising contraction within Isaac Levis' system, as he himself admits. Why ever jettison something one knows for sure? Especially since this creates the risk of certain error in the form of the later acceptance of a theory inconsistent with what is now known infallibly, that is a theory which is now known infallibly to be false. Levi's response, if I have understood it, is to encourage a studied myopia: so long as no error is incurred in some particular move in the knowledge game, the fact that it may lead to error in the future should simply be ignored (see p. 629).

This position, I have to admit, seems to me to transcend myopia and to be more accurately described as Nelsonian - Admiral Nelson, you will remember, was the one who put his telescope to his blind eye in order to avoid seeing an unwelcome order to withdraw. It hardly seems right for a rational agent to be as Nelsonian as Isaac Levi requires him to be. Surely the historically aware scientist is just going to find it emotionally impossible to regard all his accepted beliefs as infallible. It is true that some scientists do fall into this trap, but the more historically aware do not, and nor do they even act as if all their beliefs are infallible. For example, throughout the long period of domination of the Newtonian corpuscular theory of light - it lasted pretty well through the 18th century - the rival wave theory was always regarded as a seriously entertainable hypothesis, in all probability wrong, certainly facing deep conceptual difficulties which had not been surmounted, but still not entirely ridiculous in the manner of the claim that, say, water might start tomorrow to freeze at 30°C at standard atmospheric pressure.

At all events, I take it that if we could acknowledge fallibility but avoid the absurdities which Levi sees as following from that acknowledgement, then the fact that we would thus also avoid Nelsonianism would make the fallibilist alternative preferable.

Levi holds that fallibilism entails that all scientific assertions are on a par - all being conjectural. It entails that although present knowledge actually rules out the possibility of an atomic explosion in a cold water reactor, while it assigns a small but non-zero probability to the possibility of a core meltdown, the difference in these possibilities is only one of degree, since there is also a non-zero possibility that our present knowledge is wrong. The Levite rational man on the contrary entirely ignores the mere general sceptical doubt that even what he thinks he knows <u>might</u> be wrong, and concentrates exclusively on the 'genuine possibilities' which his knowledge leaves open. Hence he is effectively an infallibilist.

Now I think that the important question about the reactor example is this. (I am ignorant of the details here, and so can't myself supply the answer.) Is the prediction of no explosion a consequence only of <u>high level</u> theory? Or does it follow from a much lower level observational consequence of that theory - an observational consequence which has already been well confirmed? If the latter, if, that is, the possibility of a reactor explosion is akin to the possibility that water may in the future freeze at 30°C at standard atmospheric pressure, then I sympathise with Isaac Levi. But only because the impossibility of explosion then follows from the part of background knowledge for which no 'pessimistic induction' can be performed. We have no historical evidence that this sort of knowledge is corrigible in any practically important sense - on the contrary, the history of science gives us every reason to suppose that, no matter what happens at the high level theoretical levels, this part of background knowledge will be essentially preserved. Certainly, any changes that there are will be subtle and not at the "gross" level of explosions and the like. This part of background knowledge has proved essentially incorrigible, I see no harm in regarding it as essentially infallible.

But now consider the other possibility: that the no explosion prediction depends on a relatively high level, though firmly accepted theory, and that this kind of consequence has <u>not</u> yet been closely checked empirically. It then seems to me that the Levite rational man would be acting most irrationally, and most dangerously, if he ruled out entirely the possibility that his knowledge may be mistaken. Fresnel's wave theory was already firmly accepted when it was discovered to make predictions of a hitherto entirely unsuspected kind about so-called conical refraction. Wave theorists were confident that these predictions too would be borne out - confident but not of course certain. Had some technological application - especially of a dangerous kind - depended on the correctness of these predictions then it would surely have been negligent to base that application on accepted high level theories, no matter how firmly entrenched. Instead the relevant observational generalisation would <u>first</u> have been tested, so that the possibly dangerous application could then have been based only on old-fashioned "horizontal induction" - on, that is, the observational part of background knowledge.

So far as technological, practical decisions go, then I think we need not, and never do, fully rely on accepted high level theories, no matter how firmly entrenched they may be. However we do - as Dudley Shapere has emphasised - sometimes "presuppose" fully-fledged theories, and not just their already tested observational consequences, notably in building theories in further areas. Here I find myself in agreement with Isaac Levi (and Ernest Nagel) on one main point: namely that the "pessimistic induction" will not, and should not, be regarded as a positive reason to doubt our presently accepted theories. The fact that science may eventually need to replace the General Theory of Relativity, say, in the same way that it eventually replaced its Newtonian predecessor, does not prevent the General Theory being our present best guess as to the truth in its field. It seems entirely sensible then in building, say, some particular cosmological theory to presuppose the General Theory as being the best theory we have. Especially since the history of science supports the optimistic induction that this will not lead us too far astray - in that any future theory will surely explain the empirical success of the General Theory of Relativity, probably by yielding it as a "limiting case". I can't see at all, however, why we should need, in the process of presupposing the General Theory, to suppose it infallible. Any more than a rational agent who found himself lost and decided that in the light of all the evidence road A was the likeliest to lead him home would need to suppose that he infallibly knew road A to be the correct one in order to rationalise his choice of it.

3. For the "medium sized" picture

There is a good deal in Dudley Shapere's paper with which I fully agree. One disagreement concerns his treatment of the "pessimistic induction" - but rather than go over that territory again, let me concentrate on a second big disagreement.

Shapere adopts the so-called "big-picture", including within the substantive body of accepted knowledge the very criteria of scientific merit and scientific acceptibility. Indeed their inclusion within science was, in his view, one of science's greatest successes. Says he:

> [As science developed] criteria of success - conceptions of what it is for an idea or a theory to be successful - ... have passed from being science-transcendent to being interlocked with scientific belief, themselves both guiding the knowledge-seeking enterprise and guided by its results ... . To give a name to the process, those criteria have been internalized into the scientific process, becoming subject to the very procedures of revision or rejection which they themselves helped define. It is a process by which science strives to eliminate, and has shown itself time and again successful in eliminating, distinctions of 'levels' of its activities - between levels of 'metascience' and 'science', methodology and substantive belief, criteria and thought ... . (Above, p. 651).

That is why science can "bootstrap" its way to success: "The process of revision [of standards], being one of criticism and refinement in the light of discoveries produced by application of the standards themselves," is thus one "of 'lifting oneself by the bootstraps'." (Above, p. 653).

Well, I once ended a response to Clark Glymour's (1980) book with the remark that, except in fairy tales, all that happens if you pull hard at your own bootstraps is that they break.[3] And Dudley Shapere has, I'm afraid, given me no more reason to believe in the magical properties of his rather different bootstraps.

How exactly could a criterion for successful science be corrected or revised through scientific practice? I assume that meeting these criteria is a necessary condition for success in science. Shapere, perhaps sensing the point I'm about to make, in fact talks rather vaguely here of the criteria "guiding" developments, but we surely want real methodological criteria to require rather than guide. It then just follows logically that successful science could never necessitate the revision of the criteria. The science wouldn't be successful if it didn't satisfy the criteria. Only if success is judged independently of the criteria can successful science require the revision of those criteria. This is exactly what happens on the "medium-sized picture" that I advocate.

Now I completely agree that we won't get very far in our analysis of science if we stick with isolated theories and ignore the multiple interconnectedness of theories and the hierarchy of entrenched assumptions, including very general "metaphysical" assumptions. I also agree that it is natural to talk of these metaphysical principles, like determinism, as having dual status. First they are implicit in some accepted theories and hence figure as substantive, accepted claims about the world. Second they may play a heuristic role - requiring that, if empirical problems arise and a new theory is proposed to solve them, then that new theory also carries the metaphysical principle concerned as an implication. This is just an alternative way of making the point made earlier that such general metaphysical principles, once accepted, are usually more firmly entrenched than the specific theories which embody them. This relatively firm entrenchment means that, at any one stage in the history of science, we shall be able to specify the kind of theory which will be sought, at any rate in the first instance, to solve some empirical problem. And this specification will not simply be the bland one that the theory be unified and enjoy greater empirical success than the present one, but will add that the new theory will be deterministic, based on forces which are distance-dependent in some simple way, exhibit certain definite symmetries, or whatever.

We can if we like stress this heuristic role and speak of these principles as methodological criteria. If we do, then there is, of course, no doubt that methodological criteria are subject to change as science changes. Determinism itself did not survive the quantum revolution. But these changes in "methodological criteria" are brought about by the repeated failure to produce theoretical systems which satisfy them, and at the same time satisfy the seemingly bland criteria of unity, simplicity and above all empirical success. The latter criteria have, so far as I can see, remained fixed throughout the history of science. These seemingly bland criteria are therefore the dominant ones. They also lie outside science, playing the role of unjudged judges. The other so-called methodological criteria which are subject to change are best seen in their heuristic roles as our present guesses as to how to go about satisfying the real, dominant methodological criteria. We can rationalise changes in these heuristic principles only because success in science is ultimately characterised independently of them.

I do not believe that the task of clarifying these dominant methodological criteria is as hopeless as Shapere makes it sound (p. 652). And I certainly believe that the alternative of regarding all criteria, even these basic and very general ones, as within science and therefore subject to change is untenable. To adopt this alternative is to abandon rationality: at any rate in the sense of explaining later theoretical systems accepted by science as better than their predecessors according to neutral criteria.

I concede that the siren who has tempted Dudley Shapere onto the rocks is an especially attractive one. One problem sticks out of my more traditional account like a sore thumb - the problem, namely, of the status and justification of the basic methodological principles which, according to my traditional account, lie outside science. Any attempt to justify them would require further assumptions: we are caught between falling down an infinite regress and making the bald assertion that there are the basic methodological principles that characterise science, and that's that. I in fact see no alternative but to adopt this latter view - to admit, in other words, that acceptance of scientific rationality is itself an irrational, or better non-rational, act.[4] But how much nicer if we could somehow legitimately claim that the very criteria of science are themselves scientifically justified, themselves under rational control. This sounds just the ticket, but, as I said, it is a ticket onto the rocks.

It is, as I argued, just a logical fact that, if the criteria are "internalized" then successful science cannot require the revision of those criteria. This does not, of course, preclude the possibility that these criteria do as a matter of fact change - nor the possibility that we can "explain" science's switch from theoretical system S to system S' by saying that S' satisfies its included criteria of success better than S does. This, in the end, is what Shapere's idea that science bootstraps its way to ever greater success amounts to. Here is how he rationalises the switch from classical Laplacian physics to quantum mechanics: (Quote eliminated in printed version of Shapere text.)

> [Classical Laplacian physics held] that the entities existing in nature are completely deterministic and the laws of their behaviour completely deterministic, and that success in accounting for the world was to be measured according to the degree of determinateness and determinism approximated. For well known reasons, that view in its turn had ultimately to be corrected by quantum mechanics, which replaced the view that 'success' requires deterministic prediction with that of prediction of probabilities. But as the Laplacian view was able to achieve more - to fulfill its criterion of success - more fully, more successfully - than its Newtonian predecessor, so quantum mechanics was able to do in comparison with its Laplacian antecedent.

The point is, however, that relative to the older criterion of success (where this is taken to include determinism) quantum mechanics is entirely unsuccessful. Hence "bootstrapping" in fact is nothing but outright relativism: quantum mechanics is better than classical physics according to the criteria adopted along with quantum mechanics, classical physics is better than quantum mechanics according to the criteria adopted earlier by classical physics. It is ironic that someone who so emphasises rationality and abhors relativism should end up in irrational relativism. The only escape is to be able to say that the new quantum mechanical criteria themselves are in some sense better than the old criteria. But this again requires some super criterion outside the game which evaluates the two competing criteria. Surely the traditional view is altogether simpler and better: that, in view of accumulating empirical difficulties, classical theories of atomic phenomena became ever more disunified in their attempt to be empirically successful; and a non-classical theory, involving a radically different metaphysical framework, was eventually proposed which was unified and empirically successful, and therefore was preferable to any classical theory, according to the very same basic criteria of success that have always held sway. At any rate, the choice is between this traditional view and historical relativism - "bootstrapping" far from being a viable third alternative seems to collapse on analysis into relativism.

Notes

[1] I would certainly want to emphasise, along I think with Dudley Shapere, that even in revolutions there is a certain important element of continuity. The idea of one theoretical system breaking down and causing scientists to go away saying "Yes, we must make some entirely new bold conjecture and hope that it survives subsequent tests." is of course absurd. The new theory is in certain ways systematically developed out of the old and its empirical success and limitations. But the fact that frogs develop out of tadpoles does not mean that there aren't radical differences between the two.

[2] My own views on scientific realism are developed at some length in my 1982b.

[3] See my 1982a.

[4] This was also Popper's position expressed in the Open Society. He was later tempted by Bartley's Comprehensively Critical Rationalism into believing that his own version of rationalism is self-satisfying. In fact CCR fails for much the same reason as Shapere's "bootstrappism".

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