In Praise of Truth and Substantive Rationality:
Comments on Laudan's *Progress and Its Problems*

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1. Introduction

   Like most philosophers, Laudan [7] believes that by and large
science makes cognitive *progress* and that the development of science
is more or less *rational*. His book deals with two major problems:

   (a) In what *sense* does science progress? What is scientific
        progress?
   (b) Wherein lies the rationality of the growth of science? What
        is scientific rationality?

   In the main body of this paper, I first summarize and evaluate
some of Laudan's criticisms of his predecessors. Then I outline and
criticize Laudan's own theory of scientific progress and scientific
rationality. In the Postscript I sketch my own views concerning the
issue of changes in the canons of scientific rationality and the
problem of using history to evaluate normative theories of scientific
rationality.

2. Traditional Philosophical Questions About Science

   In comparing philosophical theories about science, I find it use-
ful to tabulate the answers of various philosophers to the following
questions concerning the aims, methods and achievements of scientific
inquiry:

   (a) What would the/an ideal scientific theory be like?
   (b) What does it mean to say T_2 is better than T_1? (What is
        progress?)
   (c) What sorts of questions should scientists try to answer?
        (What are good scientific problems?)
   (d) What are the best methods of answering them?

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(e) When (if ever) is it reasonable to claim that one has an ideal scientific theory (as described in (a))?

(f) When (if ever) is it reasonable to claim that this T_2 is better than that T_1 (in the sense specified above?)

Thus we can summarize Bacon's theory of science in terms of his answers to the above questions. For Bacon:

(a) The ideal scientific theory would give a true account of what he called "the configuration of the world" or "the fundamental Union of Nature" (*The New Organon*, Book II, XXVII).

(b) T_2 is better than T_1 when it is higher up the ladder of axioms.

(c) Scientists should begin with questions such as "What is the form (essence) of X, (e.g., heat or whiteness)?"

(d) His theory of method consists of the specified use of Baconian Tables and Prerogative Instances.

(e) We are unlikely to achieve ultimate understanding.

(f) However, by using his method one could hope to progress up the ladder of axioms.

We can also use this tableau to compare philosophers' views on key issues. Take question (c) for example. According to Galileo, one should *not* begin science with "What is...?" questions, but with "How...?" or "How much...?" questions. According to Popper, science begins when our expectations are violated – when we ask *why* some irregularity occurs, e.g., the problem of the planets. He also talks about looking for *deep* explanations of known regularities, such as Kepler's laws.

Agassi, on the other hand, argues that science is primarily motivated by attempts to answer metaphysical questions – one proposes testable scientific theories in order to criticize opponents' worldviews or to articulate one's own.

If we look at contemporary answers to question (b), I think almost all would agree that there are several elements which enter into our appraisal of theories. Other things being equal, we prefer (i) theories whose predictions are more accurate, (ii) theories of wider scope, (iii) simpler theories, (iv) deeper theories, etc. Where modern philosophers disagree is on priorities, on how these desiderata should be weighted. Thus Mach placed quite a high value on economy of thought. Inductivists are quite keen on accuracy, but as Popper always reminds us, they sometimes forget to stress content, etc.

Current disagreements on epistemology, especially question (b) and on methodology, question (d), are well-known and I need not rehearse them here.

3. Laudan's Criticisms Of Traditional Approaches

Laudan rejects many of the traditional philosophical attempts to

answer the above questions. First of all, he is an epistemological
pessimist. Since we can never recognize true theories nor even deter-
mine which theories are better approximations to the truth, why define
the aims of science in terms of truth? Laudan suggests we try for
something which would be more *practical* and which could be used in
real-life scientific cases. And by all means we should avoid identi-
fying the rationality of science with the rationality of *belief*.

Secondly, he is also discouraged by the technical and philosophical
difficulties which philosophers have encountered in trying to make sense
of *degrees* of confirmation, corroboration, content, verisimilitude,
what have you. Plus there are all the old puzzles about incommensu-
rability. Laudan wants us to avoid all these difficulties by defining
progress in terms of concepts which are simpler, clearer, and easier
to compare and measure.

Thirdly, Laudan believes traditional accounts are of little service
to the historian of science. Following Lakatos, Laudan would like to
evaluate research programs or traditions, not individual theories.
[I will not compare their views here.] In general he believes that
by focusing on problems, we will better understand the dynamics of
science and the motivations which underlie scientific investigations.

Thus, his account of scientific progress and rationality is in-
tended to be conceptually simpler, technically more tractable, more
practical, and more faithful to history than previous views.

4. Laudan's Views In a Nutshell

The following is a summary of Laudan's theory of scientific growth:

(a) The aim of science is to solve cognitive problems.
(b) There are two basic types of cognitive problems - empirical
     and conceptual.
(c) Theories are appraised according to their problem-solving
     effectiveness.
(d) Problem-solving effectiveness is a function of both the number
     and importance of the problems solved. (Theories get plus
     marks for solving empirical problems and minus points for
     generating either empirical anomalies or conceptual problems.)
(e) Progress is defined as an increase in problem-solving effec-
     tiveness.
(f) Scientific rationality consists in doing whatever we can to
     maximize scientific progress. However, theories about the
     details of what can or should be done change. Rationality
     in the large sense involves using the best specific theories
     of rationality available at the time.
(g) Since scientific research is done within the matrix of a re-
     search tradition (which sets problems, ranks them according
     to importance, suggests ways of solving them, etc.) and since
     specific theories tend to be rather shortlived whereas research
     traditions are more persistent, generally when we think of

scientific progress or scientific rationality, we are com-
paring research traditions.

(h)   The adequacy of a research tradition is a function of the
problem-solving effectiveness of the theories which (in part)
constitute it.   [I will not be discussing his theory of re-
search traditions.]

Even before hearing the details of Laudan's view, you may well be
puzzled by the following: Since Laudan needs to count and weigh
solved problems, why doesn't he run into all the old familiar techni-
cal difficulties which Popper encountered when he proposed a Yes-No
question measure of content? After all, doesn't each theory solve an
infinite number of problems? And, as Grünbaum [1] has pointed out,
even if we focus on questions instead of theories the old incommensu-
rability hassle arises in a slightly different guise - oxygen theory
cannot *answer* questions about phlogiston, it can only "obviate" them
by challenging the question's presuppositions. (For further discus-
sion of these issues, see Koertge [4].)

There are also immediate puzzles about what counts as a problem
solution. Does an almanac *solve* problems (in a scientific sense)
about when the moon will be full? To what sort of minimal epistemol-
ogical standards must a theory comply before it gets credit for solv-
ing a problem? For example, does the theory "All snow is pink" *solve*
the problem, "What color is snow?" It would appear that Laudan has
his work cut out for him.

5.   Laudan's Theory Of Empirical Problems

We now ask: What, according to Laudan, is a scientific problem?
What is a solution? What is a satisfactory solution? How does the
appraisal of a scientific theory depend on problems and solutions?
(As I said earlier, Laudan distinguishes two basic types of problems -
empirical and conceptual. In this section we will only be concerned
with his concept of *empirical problems*.)

According to Laudan, talk of empirical problems and their solutions
is somewhat similar to Hempelian talk of explananda and explanations.
However, there are two important differences. First, although a
genuine explanandum must be true, the state of affairs described in a
Laudanian problem need not be true. "All that is required is that it
be *thought to be* an actual state of affairs by some agent." ([7], p.16).

Secondly, although all known facts are presumably potential expla-
nanda, not all putative states of affairs lead to problems. In order
for a fact to lead to a problem, "...we must *feel* that there is a
premium on solving it." ([My italics], p. 17). In short then, ac-
cording to Laudan, problems arise whenever someone *thinks* a claim
about the world is (a) true and (b) deserving of explanation.

We can see immediately that by moving to a subjective or socio-
logical account of scientific problems Laudan avoids many of the

standard technical difficulties. One can only *feel* puzzled about a few things at a time, so all the problems about infinities disappear. Likewise, difficulties about incommensurability or question-obviating disappear. Since most phlogistonists did not *feel* that weight increase during combustion deserved explanation, then, on Laudan's account, that phenomenon posed no problem for their theory. Oxygen theorists, on the other hand, were not particularly interested in characterizing the essence of metals, so they felt no loss in problem-solving effectiveness upon giving up the phlogiston theory.

However, Laudan now needs an explanatory theory of fashions in problems - why are some putative facts felt to be in need of explanation while others are not? (Popper and Agassi can provide objective answers to such questions in terms of experimental violations of background knowledge or their relevance to metaphysics. It's not clear how Laudan can.)

He also needs a normative theory of which putative facts *should* be taken seriously or else his account will be too vulnerable to the idiosyncracies of individual or mob psychology. An example: My neighbor, Melvin Mushrush, thinks little green men are riding on his cornpicker and wonders why. On Laudan's account he has a problem. I agree that he has a problem, but hardly one whose solution is relevant to the progress of science. Surely the scientific problem is why Mushrush sees little green men, not why there are little green men.

As you can tell, I would prefer to have an objective, but epistemologically relative, theory of problems, i.e., a theory of what is *objectively* puzzling, given the knowledge situation at a particular time. However, I could live with Laudan's subjective account if he followed it up with a good tough theory of what properly counts as an acceptable *solution* to a problem.

What, according to Laudan, counts as solving a problem? In his very short discussion of this question, I find a tension between two inconsistent positions. I will argue that neither is acceptable. Laudan introduces his position as follows: "In very rough form, we can say that an empirical problem is solved when...scientists *properly* no longer regard it as an unanswered question, i.e., when they *believe* they understand... ." ([My italics], p. 22). The key word in this quote, of course, is "properly". Without it, we would have to say that every crank is successful and that every arrogant, dogmatic cult has solved the problem of what is right and wrong, etc.

When, according to Laudan, is it *proper* for scientists to believe they understand something? Are we going to get a theory of epistemology after all? Not at all. As I said, Laudan gives two answers. Here is Answer #1: What counts as a solution depends on the *standards* for problem solution available at the time. He goes on to emphasize that "...the criteria for what counts as solving a problem have evolved so much that what was once regarded as an adequate solution ceases to be regarded as such." (p. 25).

Of course it is completely non-controversial to say that the *substance* of solutions changes over time simply as we learn more. Ptolemy's system becomes a less adequate solution to the problem of how the planets move once we learn about the phases of Venus. But Laudan's claim is quite different. He says the *standards* change.

My first reaction to the claim that basic scientific standards change over time is similar to Heinz Post's response to the boasts of externalist historians of science: "Yes, it may very well be so, but I have yet to be presented with an interesting example of where it actually is so." It is undoubtedly the case that problems change, proposed solutions change, the precision and accuracy of the experimental measurements which are practicable change, metaphysics and ontology change, but I doubt that logico-epistemological criteria for what counts as a problem solution have changed significantly. (In the <u>Postcript</u> which follows the main body of this paper, I retreat considerably from this externalist/essentialist position.)

Let us return to the original question - what is *properly* regarded as a solution? Answer #1: That which conforms to the standards operating at the time (and Laudan can't tell us what these are because they keep changing).

But there is a second answer in Laudan according to which solving a problem is somewhat like giving an Hempelian explanation; thus he says in italics: "...*any theory, T, can be regarded as having solved an empirical problem, if T functions (significantly) in any schema of inference whose conclusion is a statement of the problem*." (p. 25). However, there is one big difference between Hempelian explanation and Laudanian problem-solving. Again I quote and again Laudan uses italics: "...*in determining if a theory solves a problem, it is irrelevant whether the theory is true or false, well or poorly confirmed*." (pp. 22, 23).

First, a minor point. Surely there have been times in the past when the scientific community has espoused standards according to which the problem-solving power of a theory *did* depend on whether the theory was true or false, well or poorly confirmed. After all, *pace* Duhem not all good scientists are instrumentalists.

Secondly, don't we need to draw a distinction between giving *a* solution to a problem (which might be very bad, but a solution nevertheless) and giving a *good* solution, or an *acceptable* solution, or *the* solution? (For example, on my account of problem solving, in order to count as providing *a* solution, a theory must satisfy all of Hempel's formal requirements. In order to provide an *adequate* solution, it would have to pass a material requirement relative to the knowledge situation at the time.)

However, Laudan makes no such distinctions. Recall from my nutshell account that his theory of scientific progress does not require us to compare the adequacy of solutions in any way. Progress is *not* a

matter of getting *better* solutions. It's simply a matter of getting *more* solutions (which must conform to the community's standards).

So here is an instance of quasi-Laudanian progress. (Suppose we are in a madhouse and inmates A and B have both read Laudan.) A: I have a problem. Why am I Napoleon? B: Hey, what makes you think you're Napoleon? A: Shut up. I feel like I'm Napoleon - that's good enough. Now help me solve my problem. B: My explanation is this. You're Napoleon because you're the greatest person that ever lived and Napoleon was/is the greatest person that ever lived. A: Hooray! You've solved my problem. There is a deductive relationship between your theory and what puzzled me. Furthermore, your proposal counts as a solution by madhouse standards because it pleases me greatly. Remember our motto, "The wish is father to the fact."

B: But now I've got a problem. If you're Napoleon, why don't you have a white horse? A: Oh, goody. If I can just solve your problem (without creating any anomalies, etc.) we will have made progress! And I can solve it. I don't have a horse because, being the greatest, they all hate me and they torment me in every conceivable way, including not letting me have a horse. B: Wait a minute. Your proposed solution satisfies the deductive requirement. But what about our community standards? Does that answer please you? A: Of course, it does. Now I have a perfect excuse to kill them all... .

Have the mad people made progress? And if so, is it scientific progress? I think on Laudan's theory there definitely has been progress, although he might wish to deny it is scientific progress because the standards for problem solution in the madhouse are *not* those of the scientific community. A sensible response, but one can hardly claim to have a theory of *scientific* progress unless one gives an account of *scientific* standards, be they mutable or eternal. Laudan has not done this - and given his scruples about talking about truth, verisimilitude, confirmation and all those other good things, frankly it's hard to imagine how he ever could do it.

6. Laudan's Theory Of Conceptual Problems

As indicated above, theories get plus points for solving empirical problems and minus points for failing to solve empirical problems solved by rival theories. But there is a second mode of appraisal having to do with non-empirical *conceptual* problems. According to Laudan, conceptual problems are at least as important as empirical problems in the history of science (p. 45) and their role has been largely ignored, not only by traditional empiricist philosophers of science, but also by "moderns" such as Lakatos and Feyerabend. I wish to dispute aspects of both his historical claim about the importance of conceptual problems and the sociological claim about how philosophers have neglected them.

Laudan categorizes conceptual problems as follows:

(A) *T* has *internal* conceptual problems if its concepts are vague
     or unclear, if it is circular, or internally inconsistent.
(B) *T* has *external* conceptual problems if it is in conflict with
     another theory or doctrine *T'* which is believed to be "ra-
     tionally well-founded" (p. 49); *T'* may be

     (B.1) a well-tested scientific theory,
     (B.2) a cosmological or metaphysical world-view,
     (B.3) an epistemological or methodological theory,
     (B.4) an ethical theory or ideology.

To what extent have philosophers actually failed to recognize
such conflicts and should they have done so? Clearly traditional
empiricist philosophers have not neglected conceptual problems of
types A and B.1. Who would deny that clarity or consistency is im-
portant? (Well, Feyerabend has, but what *logical*-empiricist would?)
And is it not a common place that the biggest problem in physics at
Galileo's time was the conflict between the old physics and the new
astronomy?

As to B.2, conflict with metaphysics, this has been much discussed
by Agassi, Buchdahl, Popper (although mainly in regard to social
science), to a certain extent by Lakatos (the hard-cores of his re-
search programs include metaphysics), Watkins, and Feyerabend. Contrary
to Laudan, I think that if anything "moderns" have overstressed both
the positive and the negative influence of world-views on science.
(See my [2].)

Let me explain why briefly. We only deviate from an empiricist
appraisal of science when we introduce into it arguments which are
supported neither by logical desiderata (e.g., consistency) nor em-
pirical considerations (however weak and indirect). In my view much
of what is often labelled as *metaphysical* criticism is really logical
or empirical criticism in disguise. Examples include Leibniz' crit-
icisms of atomism, Galileo's "thought" experiment regarding tied-to-
gether falling bodies, and Chomsky's arguments against Skinner.

Also, much of what is called *criticism* is really only an expression
of metaphysical hopes or prophecies, e.g., appeals to slogans such as
"God doesn't play dice" and most reductionist-antireductionist debates.
Such disputes about which theories will be successful in the future
can go on forever - or at least until the future is upon us, but they
seem to reflect an entirely different sort of "appraisal" than what
we've been talking about.

But what about B.3, conflicts between a substantive scientific
theory and a theory of methodology? According to Laudan this variety
of conceptual problem is the most frequent cause of controversies in
the history of science and one of the most acute problems which scien-
tists have to face (p. 58). He sees irony in the fact that professional
methodologists have neglected the relevance of epistemological debates
to the history of science.

What exactly are the conflicts which Laudan has in mind? His examples, although they are drawn from diverse sciences and historical periods, all seem to illustrate the following phenomenon: The current theory appeals to unseen entities; the currently popular methodology is a rigid positivism which deplores these kinds of theorizing. Obviously, bold theories get low marks on conservative methodologies.

But *should* this be a mark against the theory? According to Laudan it depends entirely on the philosophical temper of the time. Again we're back to his claim that standards change. I agree that the degree of positivism in the air changes. But surely the job of a philosophical theory of science is to give scientists *arguments* to oppose the philosophical mood of the time if it is stultifying science, be that mood one of paralyzing operationalism or trendy, epileptic Feyerabendism!

There only remains to discuss B.4, conflicts between science and religious or political ideologies. Laudan discusses the Lysenko affair as an illustration of this (p. 63). His conclusion is amazing. If the ideology is "well-entrenched" and has a high problem-solving capacity (p. 64) then it is *proper* to give a scientific theory low marks for contradicting it! Given the very weak requirements Laudan puts on what counts as a solution to a problem, I gather that he thinks it really was rational to discredit (to *some* extent) the theories of Galileo, Darwin, and Mendel because they conflicted with well-entrenched ideologies having a certain degree of problem-solving capability.

Of course, historians of science should discuss these disputes just as they should discuss the impact on science of the exodus of scientists from Germany during World War II, or the effect on research of changes in funding patterns within the NSF. But all this seems to have little to do with our original problems about *cognitive* progress and the *rationality* of science.

7. Postscript

In his oral presentation at San Francisco, Laudan raised a meta-philosophical problem which he believes is crucial. In these added remarks I would like to speak to that issue and also modify some of my earlier rešponses to his book.

The metaproblem can be characterized as follows:

How should one argue for or against a proposed theory of rational method? In particular, how, if at all, is the actual methodological practice of scientists relevant?

Although there has been wide discussion of this problem (see Laudan [6] for bibliography), to my mind it was Lakatos [5] who gave the strongest, clearest answer. (I shall argue below that it is

incorrect.) Lakatos' answer can be formulated as follows: A method
is rational if it describes an *appropriate* way of progressing towards
a desired end. Using ordinary logical-epistemological reasoning
philosophers have been unable to agree on what is appropriate. However,
all of us agree that scientists are pretty darn successful in advancing
towards their cognitive goals (although we may not agree on exactly
how those goals should be described in philosophical language). Let
us then become anthropologists and study the cognitive methods and
norms which scientists appear to be following.

There were a variety of reactions to Lakatos' metaproposal. One
obvious point is that his metamethodology is very conservative. Using
it we could never hope to make basic improvements in scientific practice,
although we could perhaps help the scientific community make its per-
formance more nearly match the competence which it already possesses.
Also it seemed unwise to rule out logical-epistemological criticism
all together. For example, Lakatos' own MSRP appears to violate the
theorem that e confirms h iff ~e disconfirms h. (As a matter of fact,
at the end of his [5], Lakatos does allow at least minor appeals to
"statue law", but continues to insist that "case law" is more im-
portant.) But although many argued that Lakatos' metacriterion for the
evaluation of theories of scientific method was too strong, I think
everyone agreed that there ought to be some match between our phil-
osophical theory and scientific practice. After all, Newton was no dummy;
as Giere remarked, if there were no correspondence at all between our
philosophical theory and scientific practice, we might well wonder if
we were even talking about science!

Lakatos had assumed that if one stuck to cases drawn from natural
science after 1600, his historico-anthropological method would uncover
a single theory of scientific method which had tacitly guided practice.
However, Laudan (whose earlier work was in the history of philosophy
of science) now argues that not only does the substance of scientific
theories and scientific metaphysics change constantly, so also do the
theories of methodology and standards of rationality which guide
scientific practice! He summarizes as follows: "Every practicing
scientist, past and present, adheres to certain views about how science
should be performed, about what counts as an adequate explanation,
about the use of experimental controls, and the like. *These norms,
which a scientist brings to bear in his assessment of theories, have
been perhaps the single major source for most of the controversies in
the history of science ... .*" ([Italics in the original], p. 58).

It should be emphasized that Laudan's claim is not about changes
in the philosophical theories which scientists sometimes put in memoirs
or introductions to textbooks. Rather it is about their gut-level
responses to scientific problem situations, their "pre-analytic intui-
tions", as Laudan describes them.

So what is to be done? Philosophers working from armchairs do not
agree, so Lakatos suggested the debate be arbitrated through historico-
anthropological research. And now Laudan argues that scientists do

not all practice the same methodological religion. If Laudan is right,
then it looks like we must either give up the Lakatosian metamethod-
ology or else abandon our search for THE theory of rationality of
science.

Laudan tries to wriggle through the horns of the dilemma. He
firmly adheres to a version of Lakatos' metacriterion - that model of
scientific rationality is best which does justice to more of our shared
pre-analytic intuitions about concrete scientific decisions (pp. 160-
161). In short, Laudan believes there is a single unchanging theory
of scientific rationality after all. However, it turns out to have
much less content than what we had expected. As Laudan said in his
comments at San Franciso, he is striving for a theory of scientific
rationality which is epistemologically neutral. I will now explain
what I think he means by that.

To apply any philosophical theory of rationality to a specific
case we must specify certain initial conditions. For example, to
evaluate a scientific theory, on most philosophical accounts one would
need to ask what relevant experimental data were known at the time.
On some philosophical accounts we would also need to know about the
competing theories which were available, and perhaps even about the
metaphysical systems prevalent at the time, before we could say how
the theory should have been evaluated.

Laudan goes even further. On his model one also needs to plug.
in contemporary views about epistemology, scientific method, scientific
explanation, etc. His theory almost boils down to the following:
A scientist is (procedurally or formally) rational if he/she makes
scientific decisions in accordance with the best (substantive) theory
of scientific rationality available in that epoch. Laudan's detailed
account of rationality is a little stronger than the position sketched
above, but not much.

In the rest of this paper I want to raise two issues: (1) Is
Laudan's historical claim about changing standards correct? (2) If it is
correct, should we then be content with very weak procedural con-
ceptions of scientific rationality?

7.1 Have The Basic Norms Implicit In Scientific Practice Changed?

I have argued above that Laudan's retreat to procedural[2] ration-
ality was triggered by his conviction that the basic methods of ap-
praisal used by scientists change from time to time. But is this
historical thesis in fact true?

Laudan's own discussion of the issue is not very convincing. One
example he gives in some detail concerns changing standards in exper-
imental precision and theoretical accuracy. By the time of Van der
Waals, he says, the solutions of traditional kinetic theory to problems
about the pressure-volume relationships of gases were no longer viewed
as adequate (p. 26).

I will grant immediately that what counts as good experimental
accuracy changes over time and from field to field. But this is hardly
a case of fluctuations in a basic standard. The norm in all cases
seems to be the same, perhaps something like the following: the more
accurate the experiment, the better it tests theories; the more precise
a theory, the more testable it becomes; other things being equal, we
prefer theories whose predictions fall within experimental error. I
fail to see why the Van der Waals case should be viewed as an example
of changes in the philosophical "canons for adequacy of problem solu-
tion", as Laudan would have us believe (p. 26).

Technological innovation can change scientific practice without
necessitating a change in norms. It also seems clear that deep meta-
physical differences can lead scientists into fundamental disagreements
even though they are both applying the same basic norms. At least
part of the source of conflict between Skinner and Chomsky, or Bohr
and Einstein, or Newton and Descartes stemmed from their differing
metaphysical hunches about what kind of theory would eventually prove
to be fundamental. Did these antagonists really disagree on the canons
of adequacy for a successful fundamental theory (e.g., that it should
have predictive power, give a unified account of phenomena, etc.)? I
doubt it.

At this point, I would expect Laudan to argue that there were
also significant methodological disagreements in the above cases.
For example, one might point out that Skinner is a methodological
behaviorist - his argument is not so much that Chomsky's ideas are
incorrect, but that they are unscientific, i.e., not subject to direct
empirical verification. A second point is this: Remember we are at
least as interested in the rationality of pursuit as the rationality
of stronger kinds of acceptance. Scientists' decisions about which
theories or research programs to *pursue* are likely to be much more
sensitive to ideas about metaphysics and methodology than are their
decisions about which theories are worthy of technological application.

Although I am not very impressed with Laudan's own examples, I now
think a good case could be made that *some* of the canons of rational
pursuit and perhaps even of rational acceptance have changed. My own
candidates for modifications in the basic norms of science are the
following:

(a) The Discovery of an Empirical Method for Criticizing Ideal
Laws. I have argued elsewhere [3] that before Galileo, one
either imagined away "accidents" (as did Plato and Archimedes)
or tried to give literally correct descriptions of phenomena
(as did Guidobaldo). Galileo showed how to provide *empirical*
support for ideal laws which didn't hold for a single case
which could be realized experimentally (namely by showing the
idealization is approached in the limit).

(b) The Discovery of Methods of Data Averaging and Estimation of
Experimental Error. According to Thoren ([9], [10]), Ptolemy

felt a theory was adequate if it hit *any* member of a cluster of
observations; it was Tycho and Kepler who developed a primitive
methodology for taking experimental error into account. The
development of more sophisticated statistical techniques
by Gauss, Galton, Fisher, etc. is well-known.

(c) The Invention of the Concept of an Explanatory Statistical
Law. Although there is considerable disagreement about exact-
ly how statistical explanations should be understood and when
they become acceptable to practicing scientists, it seems
clear that there have been radical changes in the ways in
which scientists view and evaluate statistical generalizations.

The above examples would need to be spelled out in detail. Very
likely it can be argued that the new methods or concepts were obtained
by a sort of bootstrapping operation using earlier philosophical stand-
ards of rationality. Sometimes the new standard may simply be a more
detailed articulation of a vague slogan. (For example, much of the
theory of error could be viewed as a special case of the canon expressed
by Aristotle in the *Nicomachean Ethics* Bk. I, Ch. 3. "Our discussion
will be adequate if it has as much clearness as the subject-matter
admits of, for precision is not to be sought for alike in all discus-
sions, any more than in all the products of the crafts.") But pointing
out such relationships would not constitute a denial of the claim that
important modifications and extensions have taken place.

So for the purposes of the rest of this paper, let us assume that
Laudan is correct when he claims that there have been important changes
in the basic conceptions of scientific rationality which guide the
actual development of science. We must now ask whether his approach
is the best response to this situation.

7.2 Laudan's Conceptions of Progress and Rationality

Laudan's account of scientific rationality comes very close to that
described by Popper in the Rationality Principle which he claims applies
almost everywhere: "People act appropriately to their perceived situ-
ations." (For references, see [2].) Such a principle is neither empty
nor useless; however, by itself it can hardly serve as a means of
describing the distinctive features of *scientific* rationality.

For Laudan, "...*the chief way of being scientifically reasonable or
rational is to do whatever we can to maximize the progress of scientific
research traditions*." (p. 124). He does not describe in detail how
one might try to maximize progress. To that extent he does not propose
a detailed theory of rational methodology. However, he does offer a
theory of rational appraisal: "...the rational appraisal of a theory
or research tradition necessarily involves an analysis of the empirical
problems which it solves, and the conceptual and anomalous problems
which it generates." (p. 124).

So the invariant part of scientific rationality, according to Laudan, is a general concern with empirical and conceptual adequacy. What changes are the contemporary philosophical accounts of the nature of evidence, criteria for what counts as solving a problem, rules for the relative weighting of empirical and conceptual problems, etc.

Although Laudan's theory of rationality is somewhat stronger than the one found in Popper's Rationality Principle, it is still too weak to demarcate science from a variety of other enterprises such as myth-making, magic, theology, aesthetics, and literary criticism, all of which pay some sort of attention to experience (be it mystical, moral, aesthetic or what have you) and try to be systematic.

I conclude that although Laudan may have given us a partial account of *Wissenschaft,* in its general sense of a systematized area of learning, he has failed to characterize *scientific* progress and *scientific* rationality.

However, the reader may well be thinking, you also admitted that basic scientific norms change! Can you (or anyone else) give a better account of science? Maybe the relativists are right - science is only one *inter pares*.

7.3 Rational Changes in Standards of Rationality

What I wish to do here is sketch (not argue for) an alternative approach to the problem of characterizing scientific rationality. The central flaw in Laudan's approach, as I see it, is that although Laudan emphasizes that standards of scientific rationality *change,* he does not claim that they also *progress*. Once we take note of the fact that there is often a correspondence relationship between successive theories of rationality, then we can claim both (a) that scientific standards change and (b) there is a distinctive progressive rational character to science.

The idea of correspondence between theories is a familiar way of describing the dialectical relationship that typically obtains when a new scientific theory contradicts, yet preserves certain aspects of, the theory which preceded it. Often an approximation to the old theory can be derived as a special limiting case from the new. And by conjoining the new theory with certain boundary conditions describing the domain in which the old theory was applied, we can explain the success of the old incorrect theory - we can show why, though false, it nevertheless worked as well as it did.

I conjecture that similar correspondences obtain between successive theories of scientific rationality. Here is a simple illustration of that sort of situation I have in mind. Tversky and Kahneman [11] describe experiments which reveal a variety of systematic statistical errors which people frequently make. One involves neglecting prior probabilities. It is quite conceivable that such inferences have played a role in science, especially in the past. Using our present statis-

tical theory we can easily prove that the inference is fallacious.
However, we can also show why sometimes the error isn't crucial (per-
haps there is a flat distribution of prior probabilities or enough
evidence is collected so that the priors are swamped.)

My guess is that if we look at the history of either tacit or ex-
plicit theories of scientific rationality we will often find corre-
spondence relationships between theories and their predecessors. Later
methodologies not only modify their predecessors but can also be used
to explain why the imperfect theories of rationality worked as well
as they did.

This brings me at last to the problem of the metacriterion, the
problem which Laudan considers to be the most pressing in philosophy
of science today. I will make three brief points:

1) First of all, it is unreasonable to suggest, as Laudan does,
that the *only* way to judge a philosophical theory is by its "empirical"
success in accounting for historical "facts". Surely we should con-
sider the conceptual problems it generates as well. Why should he
require philosophers to be narrow empiricists while claiming that
scientists aren't?

2) Secondly, in my experience it is unwise to place a priori
restrictions on what will be counted as criticism. New modes of crit-
icism can always be invented and we shouldn't rule them out ahead of
time. (After all, the historical method which Laudan advocates for
philosophy is itself a relatively new idea.) So I conclude that doing
justice to our pre-analytic intuitions or shared value judgements about
historical cases is not a sufficient condition for accepting a phil-
osophical theory.

3) Neither is it a necessary condition. If it is true that the
substantive theories of rationality which have guided science have
evolved, then clearly it is no good looking at past science in order
to arrive at the best possible theory of rationality. Nevertheless,
the history of science *is* relevant to philosophy of science in a slight-
ly weaker sense. Given the success of science through the ages, any
new improved theory of rationality should explain *why* past science
(even with its imperfect norms and methods) was so successful.

Notes

[1] I would like to acknowledge Alberto Coffa's many helpful comments
and probing questions. The Postcript of this paper is a direct result
of discussions with History and Philosophy of Science students in my
seminar on Theories of Scientific Progress, Spring 1979.

[2] The distinction between procedural and substantive rationality
which I have in mind is roughly analogous to the two varieties of due

process recognized within American Constitutional law. Roughly speaking,
one has received *procedural* due process if certain formal safeguards
have been provided, such as prior notice and a hearing before a dis-
interested tribunal. However, one has not received *substantive* due
process unless the laws, methods, and procedures applied are not only
effectively related to some legitimate government purpose but also
constitute the least burdensome means of achieving that end.

Herbert Simon [8] draws a somewhat similar distinction in a paper
on competing research programs in economics.

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