Experiments (1)

Should we study experiments?

Logical positivists, Popper, Kuhn: No.

New experimentalists, Hacking, Galison, Franklin, Pickering: Yes.

The neglect of experiments was fueled by the Quinenean desert aesthetic.

The Theory Dominant Picture of Science

Group	Method of Science	Project	Style	Assumptions	Exemplar	Consequences	Results
Logical Empiricism (Aufbau1928)	Verification/ Confirmation	Demarcation/ Explication	Logical Analysis	CoD Vs CoJ Theory Vs Observation	D.N. Model	Linear Progress/ Rationality	Theory Dominant Picture
Popper (1935/1939)	Falsification/ Corroboration	Same	Logical Analysis	Theory vs Observation (pragmatic)	H.D. Model		Same
Lakatose (1970)	Research Programmes	Similar	Rational Reconstruction	Theory Vs Observation			Same
Kuhn (1962)	Paradigm	Demarcation	Historical	Blurred Boundaries		Nonlinear/Non-rational/ Incommensurable	Same
Feyerabend (1975)	No method	No Demarcation	Historical	Same		Nonlinear/Non-rational/ Incommensurable	No-picture at all

What is peculiar about this movement (up to 1980s) is the notion that science is just theory, and progresses (a "long march") from one theory to another via experiment. Experiments are wholly subservient to theory.

Allan D. Franklin wants us to pay more attention to the epistemology of experiment; the question of what makes us decide an experimental result is valid. (The result of this increase in attention to this & the material culture and practice of experimentation: The autonomy of experimentation.) Theories can change without a change in experimentation practice.

Hacking: There are two senses of theory-laden.

Strong: Everything is theory-laden. (Even our perception of the experiment.)

Weak: Theory-driven, but not determined. (Just our observational terms are theory-based.)

Support for weak: Experiments have happened in the past with no theory (in a loose sense). Example: Microscopes were used before a full explanation of microscopes.

Experiments do an epistemically varied number of things.

Sociologists challenging the purity of science:

1. The anthropologist in the laboratory: "Reality is the *consequence* of the settlement of a dispute rather than its *cause*."

2. Collin's experimental regress: There is no *rational* (formal, epistemological) way to solve a scientific dispute. "A detector is good iff it produces good results. But good results are produced iff the detector is good." It is broken only by consensus.
3. Pickering's dancing agencies:

Another challenge to its purity is strong theory-ladenness.

<u>Experiments (2)</u>

The problem/regress: Successful detection⇔Functioning detector.

Is the discovery of the mystery particle sanctioned/justified by "bad" sociological factors (power, prestige) or "good" epistemic (non-sociological) reasons? Sometimes, the former certainly overcomes scientists. Example: BICEP2.

Calibration: "The use of a surrogate signal to standardize an instrument." Franklin's way to overcome Collins' regress.

Collins says this is misleadingly simple. First of all, we have to assume the standardization is good. Furthermore, what if there aren't any standard surrogate signals for what we want to deal with? Example: Gravitational waves and Weber.

Collins: Sociological factors truncate the regress. Science has no special epistemic status.

Franklin's rebut: Weber's dispute was still ultimately resolved by epistemic criteria. 6 vs 1; his competitors seemed to have better apparatus.

The LIGO team *did* have (arguably appropriate) "fake" surrogate signals.

Now, let us get a more sophisticated account of calibration.

Instrument indications (position of a pointer) v/s measurement outcome (claim inferred from indications + context). Part of this context is calibration.

A more complicated calibrator for a caliper (which accounts for things like temperature, etc.) will give us more accurate results.

Another regress: On what basis have the surrogates been characterized? Applications, other calibrations…and so on.

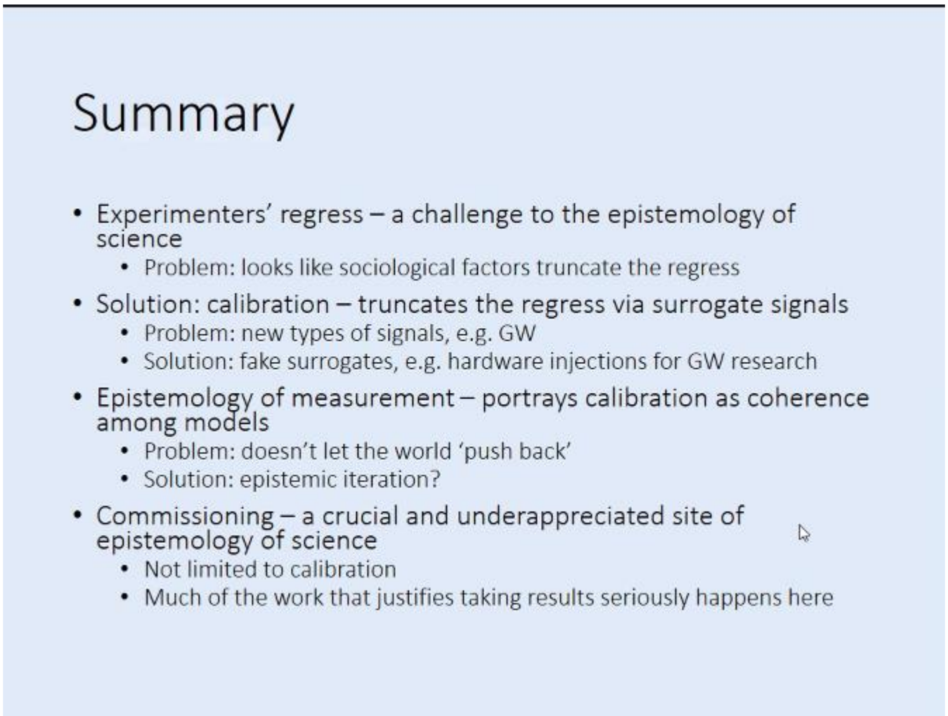
The epistemic problem: We have no access to "true" values. If you hope the regress bottoms out in an epistemically sound way somewhere, you're a foundationalist. Not looking very good. Coherentism: Measurements get epistemic support by associating with one another in the right way. Can this view severe the regress?

Perhaps. But, arguably, this does not do the epistemic face of science. Tal say measurements are just like predictions. But one ends up saying that predictions under one model are the same as those under a previous one. It misses how the world 'pushes back' on us and tells us when our predictions are wrong.

Chang's progressive coherentism: Rather than circles, these loops are spirals.

Questions with this: Is the starting point arbitrary, then? If so, are we to accept strong pluralism? Are there any other non-foundationalist alternatives?

Commissioning: "Preparation of an apparatus for routine performance according to the aims of the research performance." Not exhausted by calibration. Another way out of Collins' regress.



<u>Understanding (1)</u>

It seems to be fairly uncontroversial to say that understanding the world is one of the primary aims of science. Let us investigate the notion of understanding here.

Example: Rayleigh scattering is supposed to make us "understand" why the sky is blue. (Is understanding = explanation?) Understanding contributes to the degree of epistemic certainty in assessments.

A 'crisis of understanding' in computational neuroscience and climate modelling due to the great complexity of their computations. People say we don't understand, only predict (another goal of science).

Newton says: He doesn't have (and doesn't *need* to have) an explanation/understanding of why the earth attracts the sun, but he can describe (and predict) this using his equations. Because of Newton's success, people may have thought understanding = explanation/prediction (what Newton did).

Most philosophy of science in the 20th century considered only the "Eureka!" kind of understanding, taking it to be an irrelevant psychological detail. The epistemology of science shouldn't deal with such things which vary from individual to individual.

Hempel's Deductive-Nomological model:

<u>Explanans</u>

Premise 1: Law of nature

Premise 2: Particular matter of fact

Premise 3: Another particular matter of fact

Explanandum

Conclusion.

What is a law (for Hempel)? The logical empiricists were suspicious of modal notions such as laws and causality. Attempts to cash out laws without relying on a notion of necessity were ultimately unsuccessful.

This explanation provides a certain kind of understanding by making an unexpected phenomenon expected (Hempel's notion of understanding).

Kitcher's refinement of this: Explanation as unification. (Kitcher remains faithful, however, to Hempel's notion of explanation as derivation.) This unification is how science offers understanding.

(Why is it a refinement? The additional constraint of unification accounts for flagpole-esque symmetries.)

An issue: Newtonian mechanics can tell us what happened later just as well as what happened earlier. Both argument patterns are equally unifying, but only the former seems explanatory. Thus, people argue that we need to talk about causation as being central: The causal-mechanical explanation.

But should one think of scientific understanding & explanation as the same philosophical concept to begin with? Not all explanations (arguably) offer understanding (litmus paper example). At any rate, there's a distinction worth taking seriously. Another example in which the two come apart: The four colour theorem.

De Regt: Understanding as *intelligibility*.

1. A theory is intelligible if its qualitative characteristics can be recognized without calculation.
2. A phenomenon is understood iff it has an explanation based on an intelligible theory (which also conforms to the basic epistemic values of empirical adequacy and internal consistency).

Varieties of understanding:

1. Phenomenology of understanding (Eureka!)
2. Understanding a phenomena (Explanation)
3. Understanding a theory (Intelligibility)

Understanding (2)

Idealizations: Assumptions made without regard for their truth. Distinct from idealized representations.

One faction: Idealizations contribute to explanation and understanding. This means explanation and understanding are non-factive. (Rejecting veritism.)

Other faction: Maintain factivism while accommodating idealizations. Motivated by the conviction that there is an objective standard (truth-tropic?) for something to qualify as an epistemic success.

Rejecting veritism: A false posit contributes/is required for an explanation's success.

Endorsing veritism: Only true claims contribute to an explanation's epistemic success. (Or is it enough if the explanation as a whole needs to be approximately true to be successful?)

Issues: Do we evaluate the explanation as a whole or individual posits? What counts as a contribution to the explanation?

Claim: Y can explain X (best) without being entirely true. Example: The ideal gas law can explain a balloon's expansion with respect to its pressure without being perfectly true.

Furthermore: P (posit) can be crucial for Y without being remotely true. Example: There are no intermolecular forces in the gas.

Having rejected veritism, we need a new epistemic standard.

Posits: A posit is *epistemically acceptable* if its divergence from truth is "insignificant", accounting for its role in the representation (the ideal gas law has to be introduced *as an idealization*) and the purpose of that representation (different contexts require different explanations). Example: Electrons following orbits is epistemically acceptable as an idealization in certain aims.

Explanations: A successful explanation can only include epistemically acceptable posits. Furthermore, it must depict a causal pattern that accounts for the *explanandum* (the pattern "embodies" the phenomenon and "matches" the *explanandum*). The pattern must also address the "cognitive needs" of the explanation seekers.

An implication: Science generates different noncompeting varieties of understanding. Western bluebird example.

Veritism also faces the bane of pessimistic meta-induction. If we stop tying the epistemic aim of science to truth and make it understanding (in a context) instead, we have a better picture.

<u>Evidence (1)</u>

How do we pick out one theory over a pool of plausible candidates? A popular model is Bayesianism (prescriptive, not descriptive).

Advantages:

The Dutch book argument: If you violate probability axioms, you're vulnerable to taking a series of bets which will inevitably lose you money.

The convergence theorem: If you reason as a Bayesian would, you can expect to eventually reach the truth. If T is the true theory, then P(E|T) is high for good evidence and low for bad evidence.

Problems:

Vacuity: The theory is flexible to the point of vacuity, because the Bayesian can agree with anything by saying "This is how the ratio should be filled in".

Bayesianism can increase the posterior probability from the prior probability without engaging in inductive argumentation (the example of the coin-flip).

Priors: Where would we get P(T) and the other priors from? Rebuttal: We only need P(E|T), and can work out which theory is better by working out the ratio P(E|T1) and P(E|T2).

Ignorance: Bayesianism cannot express ignorance. You can't always make the options equiprobable. If you try to express your ignorance about A, you end up increasing your confidence in ~A. Additivity may just not be true sometimes.

Another model: Inference to the best explanation. You work out the epistemic virtues of each theory and choose one.

An immediate objection (Hungerford): Many of these epistemic virtues (simplicity, etc.) seem like subjective features.

Another objection (Voltaire): Why suppose the loveliest theory is true? But we're not saying anything about the world by picking out the loveliest/simplest theory.

Evidence (2)

The basic question: How does evidence bear on theories? (Does it support one more strongly than others?)

Confirmation theory: A philosophical theory that attempts to answer the above.

1. Anthropological approach (Descriptive?): Looks at how evidential support works in practice (e.g.: Hempel).
2. Logical approach (Prescriptive?): Tries to reconstruct how evidential support *ought* to work (e.g.: Bayesianism).

Hempel's instantialist theory: A piece of evidence *directly* confirms a theory (All Fs are G) just in case it is an F that is a G or a non-F. (Disconfirms if it is an F that is not a G.) A piece of evidence *indirectly* confirms a hypothesis if it directly confirms another hypothesis that entails it. Example: A black raven seen in USA directly confirms "All ravens are black" (and so indirectly confirms "All Australian ravens are black").

Features:

a) Gives direct criteria for confirmation of any first-order logic statement
b) Solves Jean Nicod's paradox

Limitations:

a) Purely qualitative (no notion of the extent to which an evident supports) (A)
b) Only applies to hypotheses about observable properties

Oddities:

a) A yellow banana confirms "All ravens are black" (related to first limitation?)
b) Ignores "superempirical" virtues (simplicity etc.) (B)
c) Ignores epistemic context (background beliefs etc.: in a place with many ravens, seeing only one black bird/raven disconfirms) (C)

Strevens' thesis (with regards to A, B, C): This is how confirmation *actually works*.

A crucial distinction: Official Scientific Argument vs Private Scientific Reasoning. Each has its own distinct evidential support structure.

PSR: Quantify degree of support, account for superempirical virtues, account for epistemic context.

OSA: Do not quantify degree of support, ignore superempirical virtues, ignore epistemic context.

A) Evidential support is not quantified: Of course, a lot of quantification *does* go on (statistical analysis, etc.). However, they are not by themselves enough (you also need views on, say, the plausibility of the assumptions). The journal contains the evidence and the way it's predicted (by the theory), but how it impacts the theory is left to PSR.

B) Beauty seems to be a virtue of a theory, but a theory is ultimately judged by "the hard facts". It cannot be offered as an argument in a journal.

These "irrational" constraints force/enable OSA to make investigations to a great depth and detail.

Community (1)

Traditional epistemology is concerned with the individual knower (how they acquire and justify knowledge; the individual is its locus); bracketing out social influences in this way leads to an impoverished view.

Contemporary research is more and more often highly collaborative in nature. Their findings often have direct bearings on society. How would a traditional epistemologist make sense of this? How (say) would they justify a non-expert's belief in such research? A response such as "We rely on the consensus of experts" does not take one very far. It needs to be supplemented.

We will not deal with radical breaks from traditional epistemology (such as social constructivism, which deny rationality, etc.).

Mill was a forerunner of social epistemology. His 3 reasons for cognitive diversity:

1. Helps attain truth from partial truth
2. Helps avoid falsehood
3. Helps you do this for the "best possible reasons"

Further, Kuhn says rational deliberation has to be supplemented by the assent of the community (when it comes to paradigm shift.)

The strong program of sociology: An attempt to offer a causal account of why a theory is held, where the causes are sociological.

Division of cognitive labor:

For Kitcher, "The general problem of social epistemology…is to identify the properties of epistemically well-designed social systems." Oft-dismissed motives considered "beyond the pale of scientific decision" can be constructive in certain cases. A community pursuing multiple lines of research is more optimal than just one, even though an unorthodox research line may be irrational for an individual scientist. This cognitive diversity comes about due to the presence of agents motivated by reasons other than the pursuit of truth (sullied agents).

According to Strevens, however, cognitive diversity is to be expected due to the way science offers rewards.

Another thing that can increase cognitive diversity is the way communication may work in a community. Zollman showed that a high communication density can lead communities away from truth (one will end up easily misled). Zollman adds that diversity should also reach a consensus. (However, if you increase the number of nodes or tweak the probabilities, the Zollman effects appears to vanish. In fact, all these models are sensitive to parameters.)

Peer & expert disagreement:

Two ways to react to peer disagreement: Conciliation and steadfastness.

Argument for conciliation: Independence (of the other scientist's peer-hood from the disagreement) and uniqueness (the evidence will confirm exactly one person's proposition). If 1 & 2 hold, the fact of disagreement has to be taken as evidence against one's justification.

Does the fact that experts disagree on some topics make them less trustworthy on the topics they agree on? On the contrary, it can be argued that this should make them more trustworthy. Disagreement tells us they're not merely mimicking one another.

If there is a consensus P, a non-expert could infer P being epistemically justification by seeing that the consensus is best explained by P (or a theory entailing it) being epistemically justified. However, it is also possible that the consensus was merely formed by herd behavior. But if we also knew that experts disagree, the second explanation cannot hold.

Consensus:

Example of an accidental consensus: If a majority happened to prefer Russell in the introduction sheet.

How do we know that there is a consensus? A straightforward way: Consensus reports (e.g., IPCC report). Another way: A qualitative analysis of scientific reports.

When is consensus knowledge-based? Oreskes: We should identify whether the theory exhibits the following virtues (is robust with respect to different accounts of the scientific method):

1. The theory is inductively supported by evidence
2. The theory should make successful predictions
3. The theory should be falsifiable
4. The theory should provide a consilience of evidence
5. The theory should have explanatory success

However, this is purely cognitive in character. It says nothing about the role of biases in interpreting evidence. (Example of secondhand smoke and cancer.)

Thus, Miller says: Consensus is epistemically justified if it [epistemic justification] is the best explanation of consensus. (A mix of the two.)

Knowledge is the best explanation of consensus if we can exclude:

1. Veritic luck
2. Epistemic misfortune
3. Non-cognitive consensus

They can be excluded when we have:

1. Social calibration
2. Apparent consilience of evidence
3. Social diversity (argued that it cannot be reduced to 2)

<u>Community (2)</u>

Disagreements have long been recognized as playing an important epistemic function. However, we expect scientists to reach a consensus. There is a tension.

The peer-disagreement debate in epistemology: What is the adequate doxastic attitude one should hold over *p* upon recognizing that one's peer disagrees with you on *p*? Options: Conciliatory norm & steadfastness norm.

The standard context for disagreement is 'slow science'. However, with things like COVID-19, the backdrop is one of 'fast science'. We will deal with the latter.

The pragma-dialectical approach to disagreement: Disagreement is a discursive practice in terms of giving & asking for reasons.

Commitment: An epistemic obligation via a rule adopted by the community. (If I assert p, I am committed to defend p.)

Entitlement: An epistemic permission. (If p follows logically from accepted premises, I am entitled to assert p.)

Scientists are entitled to maintain their beliefs as long as an opponent doesn't conclusively attack them. Two norms:

Protagonist retraction: Protagonist only obliged to retract standpoint if antagonist attacks conclusively (reformulated: via FOE or HOE).

Antagonist retraction: Antagonist obliged to retract if protagonist successfully defends (reformulated: using FOE or HOE).

(So, a steadfast approach can be warranted.)

What counts as a conclusive attack? Open question for fast science.

Higher-order evidence: Evidence about evidence & evidential relations. Example: A disagreeing peer is HOE that undermines FOE.

Lacey says that when accepting a hypothesis, we need to consider:

1. Available evidence
2. Possible consequences of error, and consequences of not applying if it is true

Claim: HOE enters these evaluations. (In fast science, HOE can be an important proxy for FOE.)

How useful can HOE be in slow science as well, and what is scientifically relevant HOE?

Former question: Maybe not as a proxy, but it can act as a measure against blind spots.

HOE is scientifically relevant (roughly) when:

1. The source is relevant
2. The method of acquisition has good epistemic credentials
3. No biases

etc.

<u>Values (1)</u>

Every inductive law is fallible and thus involves risk. Risk involves moral judgement; therefore, values, are necessary for deciding what to accept in science. In the classical view, values enter the picture only during law-selection. However, Douglas argues that they're crucial during the collection and evaluation of evidence as well. Example: Methodological choices about statistical significance.

Wishful thinking: Hoping a theory is true because it accords with your values. To avoid it, a suggestion is to consider evidence first, and use values only when evidence runs out. Objection to this: Values aren't mere expressions of preferences; they are hypotheses ("Do x and you will like it") and go in a bundle with scientific theories.

Values (2)

Target: To establish why value freedom is worth pursuing, even if it's not a desirable end-state. "Scientists ought to act as if science should be value-free."

It's uncontroversial that values influence science. What is controversial is if they influence scientific reasoning itself. Epistemic values, obviously, do. Do non-epistemic values?

Value-free ideal: Non-epistemic values should not influence internal scientific reasoning. Value-free science seems to be more truth-conducive, democratic and trust-enhancing.

Challenges:

1. Framing problem: A lot of science has value-laden theoretical terms.
2. Methodological critique: Our evaluation of the risks associated with a scientific result can cause differences in our methodology.

These are challenges to the end-state feasibility *and* desirability.