<u>**Underdetermination of Scientific Theory from SEP**</u>

At the heart of the underdetermination of scientific theory by evidence is the simple idea that the evidence available to us at a given time may be insufficient to determine what beliefs we should hold in response to it.
Perhaps the most important division is between what we might call **holist and contrastive forms of underdetermination.**
Holist underdetermination arises whenever **our inability to test hypotheses in isolation leaves us underdetermined in our *response* to a failed prediction or some other piece of disconfirming evidence**: that is, because hypotheses have empirical implications or consequences only when *conjoined* with other hypotheses and/or background beliefs about the world, a failed prediction or falsified empirical consequence typically leaves open to us the possibility of blaming and abandoning one of these background beliefs and/or 'auxiliary' hypotheses rather than the hypothesis we set out to test in the first place.
But **contrastive underdetermination** involves the quite different possibility that **for any body of evidence confirming a theory, there might well be *other theories* that are also well confirmed by that very same body of evidence.**
Moreover, claims of underdetermination of either of these two fundamental varieties can **vary in strength and character** in any number of further ways: one might, for example, suggest that the choice between two theories or two ways of revising our beliefs is ***transiently* underdetermined** simply by the evidence we happen to have *at present*, or instead ***permanently* underdetermined** by *all possible* evidence.
<u>**Holistic Underdetermination**</u>
Duhem's original case for holist underdetermination is, perhaps unsurprisingly, intimately bound up with his arguments for **confirmational holism**: **the claim that theories or hypotheses can only be subjected to empirical testing in groups or collections, never in isolation.** The idea here is that a single scientific hypothesis does not by itself carry any implications about what we should expect to observe in nature; **rather, we can derive empirical consequences from an hypothesis only when it is conjoined with many other beliefs and hypotheses,** including background assumptions about the world, beliefs about how measuring instruments operate, further hypotheses about the interactions between objects in the original hypothesis' field of study and the surrounding environment, etc. For this reason, Duhem argues, **when an empirical prediction turns out to be falsified, we do not know whether the fault lies with the hypothesis we originally sought to test or with one of the many other beliefs and hypotheses that were also needed and used to generate the failed prediction**.
Duhem supports this claim with examples from physical theory, including one designed to illustrate a celebrated further consequence he draws from it. **Holist underdetermination ensures, Duhem argues, that there cannot be any such thing as a "crucial experiment": a single experiment whose outcome is predicted differently by two competing theories and which therefore serves to definitively confirm one and refute the other.** For example, in a famous scientific episode intended to resolve the ongoing heated battle between partisans of the theory that light consists of a stream of particles moving at extremely high speed (the particle or "emission" theory of light) and defenders of the view that light consists instead of waves propagated through a mechanical medium (the wave theory), the physicist Foucault designed an apparatus to test the two theories' competing claims about the speed of transmission of light in different media: the particle theory implied that light would travel faster in water than in air, while the wave theory implied that the reverse was true. Although the outcome of the experiment was taken to show that light travels faster in air than in water,[3] Duhem argues that this is far from a refutation of the hypothesis of emission:

> in fact, what the experiment declares stained with error is the whole group of propositions accepted by Newton, and after him by Laplace and Biot, that is, the whole theory from which we deduce the relation between the index of refraction and the velocity of light in various media. But in condemning this system as a whole by declaring it stained with error, the experiment does not tell us where the error lies. Is it in the fundamental hypothesis that light consists in projectiles thrown out with great speed by luminous bodies? Is it in some other assumption concerning the actions experienced by light corpuscles due to the media in which they move? We know nothing about that. It would be rash to believe, as Arago seems to have thought, that Foucault's experiment condemns once and for all the very hypothesis of emission, i.e., the assimilation of a ray of light to a swarm of projectiles. If physicists had attached some value to this task, they would undoubtedly have succeeded in founding on this assumption a system of optics that would agree with Foucault's experiment. ([1914] 1954, p. 187)

From this and similar examples, Duhem drew the quite general conclusion that our response to the experimental or observational falsification of a theory is always underdetermined in this way. **When the world does not live up to our theory-grounded expectations, we must give up *something*, but because no hypothesis is ever tested in isolation, no experiment ever tells us precisely which belief it is that we must revise or give up as mistaken.**
The predicament Duhem here identifies is no rainy day puzzle for philosophers of science, but a methodological challenge that constantly arises in the course of scientific practice itself. It is simply not true that for practical purposes and in concrete contexts a single revision of our beliefs in response to disconfirming evidence is always obviously correct, or the most promising, or the only or even most sensible avenue to pursue. To cite a **classic example, when Newton's celestial mechanics failed to correctly predict the orbit of Uranus, scientists at the time did not simply abandon the theory but protected it from refutation by instead challenging the background assumption that the solar system contained only seven planets.** This strategy bore fruit, notwithstanding the falsity of Newton's theory: by calculating the location of a hypothetical eighth planet influencing the orbit of Uranus, the astronomers Adams and Leverrier were eventually led to **discover Neptune in 1846**. But **the very same strategy failed when used to try to explain the advance of the perihelion in Mercury's orbit by postulating the existence of "Vulcan",** an additional planet located between Mercury and the sun, and this phenomenon would resist satisfactory explanation until the arrival of Einstein's theory of general relativity. So it seems that Duhem was right to suggest not only that hypotheses must be tested as a group or a collection, but also that it is by no means a foregone conclusion which member of such a collection should be abandoned or revised in response to a failed empirical test or false implication. Indeed, this very example illustrates why Duhem's own rather hopeful appeal to the 'good sense' of scientists themselves in deciding when a given hypothesis ought to be abandoned promises very little if any relief from the general predicament of holist underdetermination.
In essence, holist underdetermination ensures that **the evidence alone cannot do the work of picking out a single response to such conflicting evidence, thus something else must step in to do the job**, and sociologists of scientific knowledge, feminist critics of science, and other interest-driven theorists of science each have their favored suggestions close to hand.

Contrastive Underdetermination

Contrastive underdetermination is so-called because it questions the ability of the evidence to confirm any given hypothesis *against alternatives.*
Quine recognized, of course, that ***many of the logically possible ways of revising our beliefs that remain open to us in response to recalcitrant experiences strike us as ad hoc, perfectly ridiculous, or worse.*** *He argues that our actual revisions of the web of belief seek to* ***maximize the theoretical "virtues" of simplicity, familiarity, scope, and fecundity, along with conformity to experience****, and elsewhere suggests that we typically seek to resolve conflicts between the web of our beliefs and our sensory experiences in accordance with a principle of* ***"conservatism"****, that is, by making the smallest possible number of changes to the least central beliefs we can that will suffice to reconcile the web with experience. That is, Quine recognized that* ***when we encounter recalcitrant experience we are not usually at a loss to decide which of our beliefs to revise in response to it, but he claimed that this is simply because we are strongly disposed as a matter of fundamental psychology to prefer whatever revision requires the most minimal mutilation of the existing web of beliefs and/or maximizes virtues that he explicitly characterizes as pragmatic.***
Indeed, it would seem that on Quine's view the very notion of a belief being more central or peripheral or in lesser or greater "proximity" to sense experience should be cashed out simply as a measure of our willingness to revise it in response to recalcitrant experience. That is, it would seem that what it means *for one belief to be located "closer" to the sensory periphery of the web than another is simply that we are more likely to revise the first than the second if doing so would enable us to bring the web as a whole into conformity with otherwise recalcitrant sense experience.*
One way to see why not is to consider an analogy that champions of contrastive underdetermination have sometimes used to support their case. If we consider any finite group of data points, an elementary proof reveals that there are an infinite number of distinct mathematical functions describing different curves that will pass through all of them. As we add further data to our initial set we will definitively eliminate functions describing curves which no longer capture all of the data points in the new, larger set, but no matter how much data we accumulate, the proof guarantees that there will always be an infinite number of functions *remaining* that define curves including all the data points in the new set and which would therefore seem to be equally well supported by the empirical evidence. *No* finite amount of data will *ever* be able to narrow the possibilities down to just a single function or indeed, any finite number of candidate functions, from which the distribution of data points we have might have

been generated. Each new data point we gather eliminates an infinite number of curves that *previously* fit all the data (so the problem here is not the holist's challenge that we do not know which beliefs to give up in response to failed predictions or disconfirming evidence), but also leaves an infinite number still in contention.

But Bas van Fraassen has offered an extremely influential line of argument intended to show that such contrastive underdetermination is a serious concern for scientific theorizing more generally. In *The Scientific Image* (1980), van Fraassen uses a now-classic example to illustrate the possibility that even our best scientific theories might have ***empirical equivalents*****: that is, alternative theories making the very same empirical predictions, and which therefore cannot be better or worse supported by any *possible* body of evidence.** Consider Newton's cosmology, with its laws of motion and gravitational attraction. As Newton himself realized, van Fraassen points out, exactly the same predictions are made by the theory whether we assume that the entire universe is at rest or assume instead that it is moving with some constant velocity in any given direction: from our position within it, we have no way to detect constant, absolute motion by the universe as a whole. **Thus, van Fraassen argues, we are here faced with empirically equivalent scientific theories: Newtonian mechanics and gravitation conjoined either with the fundamental assumption that the universe is at absolute rest (as Newton himself believed), or with any one of an infinite variety of alternative assumptions about the constant velocity with which the universe is moving in some particular direction.** All of these theories make all and only the same empirical predictions, so no evidence will ever permit us to decide between them on empirical grounds.