HOW TO DO THINGS WITH AN INFINITE REGRESS*

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Abstract

Scientific method may be viewed either as an argument justifying a conclusion or as a procedure for finding the right answer to some question. Both conceptions occasion the problem of empirical regresses. According to the former approach, it is hard to say what the point of a regress is. According to the latter, we can solve for the strongest sense of single-method performance that could be covered from a regress of procedures. Several types of regresses are solved in this sense. Some of the solutions are shown to have sufficient power to deal with Duhem's problem.

1 CONFIRMATION AND NATURALISM

Here is a familiar way to think about the philosophy of science. Our empirical claims must be justified. Usually, evidence does not and never will entail them, so they must be justified some weaker way. Thus, there must be a relation of partial support or *confirmation* falling short of full (deductive) support that justifies them. The principal task of the philosophy of science is to explicate the concept of confirmation from practice and historical examples. Any feature of scientific method or procedure that is not addressed to the nature of this relation is extraneous to the philosophy of science *per se*, although it may be of tangential psychological, sociological, or purely computational interest. Thus virtues such as confirmation, explanation, simplicity, and testing are relevant, but the logic of discovery (procedures for inventing new hypotheses) and computational efficiency are extraneous (e.g., Laudan 1980).

After the justifying relation is explicated from historical examples, the obvious question is why it *should* be that relation rather than another. It is no longer stylish to seek an *a priori* answer to this question; one responds, instead, with the *naturalistic* view that if scientific standards are to be justified, that justification must itself be scientific. The next question is how scientific reasoning can justify itself. Somehow, circles are more fashionable than regresses, but without a clear picture of what justification is supposed to accomplish it is hard

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to see why regresses would be any worse than circles or what would make one regress better or worse than another.

2 THE PROCEDURAL PARADIGM

Consider an alternative paradigm for the philosophy of science. Suppose that scientific methods are procedures aimed at converging to correct answers rather than concepts of short-run justification that apply to incorrect as well as to correct hypotheses. This shift turns everything on its head. For now the object is to justify methods for finding correct hypotheses rather than to justify possible mistakes. Computational considerations are no longer extraneous but central, for procedures are justified insofar as they find correct answers both reliably and efficiently. The logic of discovery is no longer peripheral because the concept of convergence to a correct answer applies as much to methods producing hypotheses as to methods assessing given hypotheses. If confirmation has any relevance to justification, it is relevant only in the derivative sense of serving as a cog in a larger procedure for finding correct answers. This is the tacit philosophy of *computational learning theory*, a computation-theoretic approach to inductive inference initially proposed by Hilary Putnam (1963, 1965) and computer scientist E. M. Gold (1965, 1967).[1]

Here is a more precise formulation of the idea. Empirical methods are procedures or dispositions that take in successive inputs from nature and that output guesses in response. Like computational procedures, inductive methods may be judged as solutions to problems. A *problem* is not a particular situation but a *range* of possible cases in which the method must succeed (analogously, a multiplication procedure is expected to work for each instance of $n \times k$, not just for 2×2). In the case of empirical problems, the range of possible cases is a range of possibilities over which the method is to be held accountable. Such possibilities may be called *serious possibilities* (relative to the problem at hand). Success in a possibility means converging to a correct answer on the stream of inputs that would be received if that possibility were actual. Correctness may be truth or something weaker, such as empirical adequacy. It may also involve pragmatic components, such as being a potential answer to a given question. Or following Thomas Kuhn, it might be something like future problem solving effectiveness. The precise choice of the correctness relation is not the crucial matter. What is crucial is that all of these notions transcend any finite amount of data, and therefore occasion the problem of induction, unlike confirmation, support, personal confidence, and other such "local" substitutes.

There are many different senses of convergent success, some of which are more stringent than others (cf. Kelly 1996). Let a hypothesis be given. It would be wonderful if we could expect a scientific procedure to eventually halt with acceptance or rejection just in case the hypothesis is respectively correct or incorrect. Call this notion of success *decision with certainty*. But the hypothesis may only be *verifiable with certainty* (halt with acceptance if and only if the hypothesis is correct) or *refutable with certainty* (halt with rejection if and

[1]For book-length reviews of the technical literature, cf. (Osherson et al. 1986), (Jain et al. 1999). For sustained attempts to relate the ideas to the philosophy of science, cf. (Kelly 1996), (Martin and Osherson 1998) . For my most recent attempt at a general philosophical motivation, cf. (Kelly 2000).

only if the hypothesis is false). Popper's philosophy of science begins with the idea that universal laws are refutable but not verifiable. Less stringently, we might require only that the method *decide the hypothesis in the limit*, meaning that it eventually stabilizes to acceptance if the hypothesis is correct and to rejection otherwise. More leniently still, we could insist that the method stabilize to acceptance if and only if the hypothesis is correct (*verification in the limit*) or stabilize to rejection if and only if the hypothesis is incorrect (*refutation in the limit*). Between decision in the limit and verification and refutation with certainty, we may refine the notion of success by asking how many *retractions* are necessary prior to convergence. Kuhn and others have emphasized the tremendous social cost of retracting fundamental theories, but the number of retractions required prior to convergence may be viewed as a concept of complexity that bridges the concepts of certainty and limiting convergence with an infinite sequence of refined complexity concepts. Similar ideas can be applied when the problem is to discover a correct hypothesis, rather than to assess a given one. *Identification with certainty* requires that the method halt with a correct answer. *Identification in the limit* requires convergence to a correct answer and in between we can count the number of retractions prior to convergence.

A given empirical problem (whether of test or discovery) may be solvable in one of the above senses but not in another. The best sense in which it is solvable may be said to be its characteristic *complexity*. This is parallel to what goes on in the theory of computability and computational complexity; in fact, the complexity classes so defined are already familiar objects in analysis and computability theory. In the philosophy of science we speak vaguely of *underdetermination* of theory by evidence. I have proposed that underdetermination is just complexity, so degrees of complexity correspond to degrees of underdetermination (Kelly 1996, 2000b). This way of thinking yields a comprehensive framework for comparing and understanding different kinds of inquiry drawn from different contexts, as well as for providing a unified perspective on formal and empirical inquiry (Kelly 1996, chapters 6, 7, 8, and 10), something that has bedeviled the confirmation-theoretic approach from the beginning.

All of that sounds rather abstract. But many methodological ideas familiar to philosophers of science drop out of the framework in a natural way. Given the relevant auxiliary hypotheses, a universal hypothesis is refutable with certainty from a data stream that eventually presents every instance. The problem of choosing a correct universal hypothesis from a finite set of alternatives (assuming that one is correct) requires at most as many retractions as there are hypotheses in the set, as when one needs to isolate a quantum number conservation law to account for collisions among a finite set of observable particles (Schulte 2000). If we drop the assumption that all the particles have been observed, the answer may only be identifiable in the limit (we may be surprised by new particles any finite number of times).

Return to the problem of assessing an individual universal hypothesis. If we drop the relevant auxiliary assumptions, the hypothesis may fail even to be decidable in the limit. The auxiliaries may also fail to be individually refutable. It may only be the entire theory involving the hypothesis and the auxiliaries that is refutable. Assuming only that each conjunction of H with a set of auxiliaries is refutable with certainty, we can enumerate the possible systems of auxiliaries we have thought of and accept H so long as the currently adopted set of auxiliaries is not refuted in conjunction with H. Each time the conjunction is refuted we move to the next set of auxiliaries and reject H. When we think of new systems of auxiliaries, we add them to the end of the queue of auxiliaries we have thought of. This verifies H in the limit so long as our "creative intuition" produces systems of auxiliaries covering all relevant possibilities admitted by H. So verifiability in the limit corresponds to the intuitive epistemic difficulty occasioned by Duhem's problem and paradigm choice. That is important, because most issues in the philosophy of science (realism, conventionalism, observability, theory-ladenness, and paradigms) cluster around Duhem's problem.

Think of a "puzzle" as an empirical problem of low complexity (refutable or decidable with bounded retractions) whose crisp, stepwise solvability is due to the constraints on serious possibility afforded by the protective environment of the paradigm (as in the conservation law inference problem). Think of extraordinary science as arising when the constraining paradigm can no longer be assumed.[2] Now it is necessary to run through the possible alternative auxiliary hypotheses, so inquiry appears more subjective and arbitrary in the short run as different investigators fill in the blanks in different ways. Here is a bold idea: the historicist distinction between "normal" and "extraordinary" science is just the distinction between low learning theoretic complexity given the assumptions of the paradigm and high learning theoretic complexity when the paradigm is no longer assumed .

Limiting verifiability can arise even within normal science if the question is sufficiently complex, as in the case of hypotheses concerning trends. Such questions are intuitively difficult, because any evidence for the trend could be a local fluctuation around an unknown equilibrium. This sort of complexity was apparent in the debate between uniformitarian and progressionist geologists in the nineteenth century (cf. Ruse 1979). Progressionists held that geological history exhibits progress due to the classical, exponential cooling of the Earth from its primordial, hot state, whereas Lyell reinterpreted all apparent trends as local fluctuations on an immensely expanded time scale. Assuming a particular schedule of progress, progressionism is refutable with certainty: just wait for a fossil to come in that appears ahead of schedule. In fact, Lyell claimed to have refuted progressionism when the Stonesfield mammals were found in Jurassic strata, prior to progressionist expectations. But without such an auxiliary assumption, the progressionists can revise their schedule. To verify progressionism in the limit, accept so long as the current schedule for progress fits the facts and reject whenever a new schedule has to be adopted. Uniformitarianism is refutable with certainty: it looks good when progressionism looks bad. Global warming (Berger and Labeyrie 1987) provides a more recent example of this kind (is the current warming trend a chaotic spike or a genuine effect of the undeniable increase in carbon dioxide in the atmosphere?). Other hypotheses that are verifiable in the limit include the computability of human cognition (Kelly 1996) and the hypothesis that an empirical time series is generated by a chaotic system (Harrell 2000).

[2] Much more can be said about this (Kelly 2000b). For example, one can also provide a naturalistic account of theory-laden data in learning theoretic terms.

3 PROCEDURAL REGRESSES

The procedural outlook just described is subject to its own empirical regress problem. No empirical problem is solvable, even in the limit, without *some* empirical, auxiliary assumptions. If such assumptions are necessary for success, how can we know that they are satisfied? By invoking another method with its own battery of assumptions? And what about those? Appealing to "problems" doesn't help, for how do we know that the actual world is counted among the range of possibilities specified by the problem?

I used to think that I had no interesting answer to this challenge, because if some background knowledge is necessary to "prime the pump" of successful inquiry, it cannot be successfully investigated in the *same sense* of success, else it wouldn't have been necessary to begin with. But this argument assumes that a method that investigates the background assumption and a method that presupposes the background assumption to investigate H could be chained together to form a method that succeeds in the same sense on H without the background assumption. The assumption is plausible but wrong. Chaining methods that succeed in stronger senses can solve problems solvable only in weaker senses. Therein lies the germ of a nontrivial, learning theoretic analysis of infinite epistemic regresses. The basic idea is a methodological *no free lunch* principle, which states that the value of a regress can be no greater than the best single-method performance that could be achieved by looking at the outputs of the methods in the regress rather than at the data themselves. If this performance is much worse than what could be achieved by looking at the data directly, we may say that it is methodologically vicious. If this performance is the best possible, then the regress is *optimal*. This idea imposes the full discipline of computational complexity on empirical epistemic regresses.

4 FOUNDED REGRESSES

For simplicity, let's focus on the problem of assessing a given hypothesis H_0. Fix a given sense of success (e.g., refutation with certainty, verifiability in the limit, etc.) The *empirical presupposition* H_1 of a method is just the set of all serious possibilities over which it succeeds. Since the presupposition of method M_0 is a set of worlds, we may think of it as the proposition "method M_0 will succeed", which is a nontrivial empirical hypothesis in its own right. So let M_1 be charged with assessing H_1. Method M_1 has its own presupposition H_2, which is assessed by M_2, and so forth. Start with the case of a finite (i.e., *founded*) regress whose last method is M_n. In that case, we say that the regress succeeds with respect to H_0 in a given sense (e.g., refutation with certainty) just in case

1. H_{i+1} is the presupposition of M_i with respect to H_i according to the given sense of success and
2. M_n really does succeed on H_n.

A *regress reduction* is an empirical method that gets to watch all of the outputs of all the methods in the regress up to the current stage of inquiry and that produces a single output for that stage. Similarly, a *regress production* is a rule that looks at the outputs of a single method up to the present stage and that specifies the current output of each method in a regress at that stage. A sense of regressive success R is *methodologically reducible* to a concept of single-method success S if and only if there exists a regress reduction that turns each regressive solution in sense R into a single-method solution in sense S.[3] Regressive success concept R is *methodologically producible* from a given sense S of single-method success just in case there is a regress production that turns each method succeeding in sense S into a regress succeeding in sense R. A sense R of regressive success is *methodologically equivalent* to a sense S of single-method success just in case R is reducible to and producible from S.

Suppose we have a regress of two methods such that the outer one refutes the proposition that the inner one refutes the given hypothesis H_0 with certainty. Applying the no free lunch principle, what kind of single-method performance is this kind of success methodologically reducible to? None; for without further qualifications, the regress might erase all of the information in the data. For example, M_0 might ignore the data and mindlessly vacillate between rejection and acceptance, so that the proposition that M_0 succeeds is the empty set. Then M_1 could refute H_1 with certainty by rejecting it *a priori*. This shows that regresses do not have to be infinite to be vicious. The triviality can be overcome, however, if we add the qualification that each method in the chain *pretends* to succeed in the sense specified in its presupposition. By this I mean that the pattern of outputs produced by the method is consistent with the intended sense of success. For example, if M pretends to refute some hypothesis with certainty, M starts out accepting and then retracts to rejection just once, resolving to stick with the rejection forever after. A method that pretends to decide a hypothesis in the limit is not permitted to vacillate between acceptance and rejection forever. In this connection, it is interesting that Karl Popper's philosophy was based on pretending to refute what is not really refutable. Even though auxiliaries could always be tinkered with, Popper recommended that we eschew this "conventionalist stratagem" and formulate in advance conditions under which the hypothesis must be dropped for good. In other words, we should "pretend", in the sense under consideration, that the non-refutable hypothesis is refutable. Popper also appealed to regresses: the pretense of refutability was to be adopted afresh every time a presupposition came under challenge. So Popper's philosophy naturally invites the question of what can be accomplished with a regress of pretending refuters!

Let's try again. From now on, say that a finite regress of length n succeeds in a given sense S of single-method success just in case

1. H_{i+1} is the presupposition of M_i with respect to H_i according to success criterion S and
2. M_i pretends to succeed in sense S with respect to H_i and
3. M_n really does succeed in sense S on H_n.

[3]Notice that notions of success with respect to a given problem are being reduced here, whereas in the theory of computability it is usually problems that are reduced with respect to a given notion of success (e.g.,Turing computability). But one could just as well say that, for example, the problem of computing a function is inter-reducible to the problem of deciding its graph since a procedure of either kind can be modified effectively to produce a procedure of the other kind.

This sort of regress is no longer worthless. For example, a regress of two methods that pretend to refute with certainty is methodologically equivalent to one method that retracts at most twice prior to convergence and that starts out accepting.[4] More generally, the following are methodologically equivalent:[5]

- A finite regress $(M_0, \ldots, M_n)$ in which each method pretends to succeed with some finite number of retractions.
- A single method that succeeds with the sum of the retractions required by $(M_0, \ldots, M_n)$ starting with rejection if an even number of the M_i reject and starting with acceptance otherwise.

What if the methods in the regress succeed only in the limit? It is easy to see that any finite regress of limiting decision procedures is equivalent to a limiting decision procedure: just accept if an even number of the methods in the sequence reject and reject otherwise. Regresses involving one-sided limiting methods are not reducible to any of our notions of success and may be thought of as a natural way to build methodological success criteria applicable to more complex hypotheses. The situation simplifies when all of the presuppositions of methods in the regress are entailed by H_0 or by its complement. Then we may speak of an H_0-entailed or co-H_0-entailed regress, respectively. Then we have the following, simplified equivalence:

- An H_0-entailed regress (M_0, M_1) such that M_0 pretends to refute [verify] in the limit and M_1 refutes [verifies] the presupposition H_1 of M_0 in the limit.
- A single method M that refutes [verifies] H_0 in the limit.

Similarly, the following are equivalent:

- A co-H_0-entailed regress (M_0, M_1) such that M_0 pretends to refute [verify] in the limit and M_1 verifies [refutes] the presupposition H_1 of M_0 in the limit.
- A single method M that verifies [refutes] H_0 in the limit.

5 INFINITE REGRESSES

Suppose we require that every challenged presupposition be tested. If the game were to continue without an arbitrary stopping point, one would respond with a

[4]Here's the trick. Both methods start out accepting, since both pretend to refute. Let the constructed method M accept because M_1 will succeed and M_1 currently says that M_0 will succeed and M_0 now accepts. If M_1 ever rejects, then let M disagree with what M_0 says because M_1 discovered that M_0 will fail and M_1 cannot fail by retracting too often since M_0 pretends to refute. At worst, both retract and M retracts once each time. So M retracts at most twice, starting with acceptance. Methodological equivalence requires that we can also produce a regress of two refuters M_0, M_1, from an arbitrary method M that succeeds with two retractions starting with acceptance. Here's how to do it. Let M_0 accept until M retracts once and reject thereafter. Let M_1 accept until M retracts twice and reject thereafter. Let H_1 be the proposition that M_0 successfully refutes H_0 with certainty. That is true just in case M retracts at most once. Thus, M_1 really succeeds in refuting H_1 with certainty, as required.

[5]The proofs of all the propositions may be found in (Kelly 2000a).

potentially infinite sequence of methods testing the assumptions of methods.... What is accomplished thereby, other than to hold up one's position in an ongoing, rhetorical game? In a more Kantian mode, what *could* be accomplished thereby? We can try to use the concept of methodological equivalence to solve for the best sort of single-method performance such a regress would be equivalent to.

Without *some* sort of reliability constraint on the methods in the infinite regress it might be completely worthless: all of the methods might ignore the data and do something arbitrary. Even though every method has presuppositions, we may require, at least, that later methods have *weaker* presuppositions than the presuppositions they check so that later methods are more reliable than their predecessors. Say that such a regress is *directed*.[6] Then the following are methodologically equivalent:

- An infinite, directed regress $(M_0, \ldots, M_n, \ldots)$ of pretending refuters.
- A single method M that decides H_0 with at most two retractions, starting with acceptance, over the disjunction $(H_1 \vee \ldots \vee H_n \vee \ldots)$ of all the presuppositions of the methods in the regress.

More specifically, if the regress of pretending refuters is H_0-entailed, then the whole regress is equivalent to a single method that refutes H_0 with certainty. More generally, if M_0 succeeds with n retractions, the regress is equivalent to a single method that succeeds with one more retraction, starting with the same initial conjecture as M_0.

Several points should be emphasized. First, the methodological reduction constructed to prove the result operates in a *local* manner, looking at more and more of the outputs of more and more methods in the regress. So the equivalences hold even if the infinite regress is built up through time in response to specific challenges instead of being given all at once. Second, no method in the regress has a presupposition as weak as the presupposition of the regress itself, so appealing to a regress is a way to weaken presuppositions. Third, although such regresses yield greater reliability, they are feasible only for hypotheses that are decidable with only two retractions, which falls far short of Popper's ambition to address Duhem's problem by means of arbitrary regresses of pretending refuters (recall that Duhem's problem gives rise to hypotheses that are only verifiable or refutable in the limit).

The corresponding result for an infinite regress of pretending verifiers is quite different, as the following are methodologically equivalent:

- An infinite, directed regress $(M_0, \ldots, M_n, \ldots)$ of methods that pretend to verify with certainty, decide with at least one retraction starting with 1, decide in the limit, or refute in the limit.
- A regress (M_0, M) such that M refutes the presupposition H_1 of M_0 *in the limit* over the disjunction $(H_2 \vee \ldots \vee H_n \vee \ldots)$ of all the other presuppositions in the regress.

Recall that regresses of limiting methods are irreducible to simpler success criteria. If the regress is H_0-entailed, however, then we have the following equivalence.

[6] This does not imply that H_0 entails H_1, since H_0 is not a presupposition.

- An infinite, H_0-entailed, directed regress $(M_0, \ldots, M_n, \ldots)$ of methods that pretend to verify with certainty, decide with at least one retraction starting with 1, decide in the limit, or refute in the limit.

- A single method M such that M refutes H_0 in the limit over the disjunction $(H_2 \vee \ldots \vee H_n \vee \ldots)$ of all the other presuppositions in the regress.

If the regress is co-H_0-entailed the following are equivalent:

- An infinite, not-H_0-entailed, directed regress $(M_0, \ldots, M_n, \ldots)$ of methods that pretend to verify with certainty, decide with at least one retraction starting with 1, decide in the limit, or refute in the limit.

- A single method M such that M verifies H_0 in the limit over the disjunction $(H_2 \vee \ldots \vee H_n \vee \ldots)$ of all the other presuppositions in the regress.

Thus, infinite directed regresses of pretending verifiers vastly transcend the power of verification with certainty. They reach the level of complexity occasioned by Duhem's problem, which is what Popper wished to address with regresses of refuters. So, perhaps he should have been a regressive *verificationist* after all![7]

To illustrate this result, let H_0 denote Lyell's uniformitarian hypothesis. After the Stonesfield mammals were discovered in the Jurassic strata, Lyell declared victory for H_0.[8] His method M_0 was something like: don't declare victory for uniformitarianism[9] until the current progressionist (auxiliary) hypothesis A_1 about the particular schedule of progress is refuted (e.g., by an advanced form like a mammal appearing too early). Then halt and accept uniformitarianism. The presupposition H_1 of this method is, obviously, that uniformitarianism is correct if A_1 is not (i.e., $H_1 = H_0 \vee A_1$). Indeed, the progressionists responded by challenging precisely this presupposition. They revised their schedule to a new schedule A_2. Let's continue the game. Lyell can respond with a meta-method M_1 that tests his initial presupposition as follows. If the original schedule A_1 is never refuted, M_0 was right to reject uniformitarianism, so accept H_1. If the current schedule A_1 is refuted but the new schedule A_2 never is, then M_0 was wrong to halt with acceptance of H_0, so reject H_1. Finally, M_1 presupposes that no new schedule is correct (i.e., $H_2 = H_0 \vee A_1 \vee A_2$) and assumes that M_0 was right to reject if the second schedule is refuted. In general, M_n accepts H_n just in case the nth conjectured schedule of progress is not refuted, rejects if it is, and accepts when the $n+1$th schedule is refuted, thereby presupposing $H_n = H_0 \vee A_1 \vee \ldots A_n$. This is an H_0-entailed directed regress of methods pretending to decide with at most two retractions starting with acceptance, and hence is equivalent to a single limiting refutation procedure M for H_0 that succeeds over the disjunction of the presuppositions, which is equivalent to the disjunction of the two competing paradigms (i.e., uniformitarianism $\vee$ progressionism). Here is how to construct M in this particular case. Method M maintains a queue of the methods added to the regress so far. Each time a new

[7] There is an escape from this argument. Popper never said, to my knowledge, that the regress had to be directed.

[8] Of course, I oversimplify. He declared victory for a tangle of reasons that would defy any elegant logical representation.

[9] Actually, Lyell was already committed to uniformitarianism. In this case, the method M_0 models his rule for announcing victory rather than his actual convictions.

method is added to the regress, it gets added to the end of the queue (the regress is only "potentially" infinite). If the method at the head of the queue accepts, it is placed at the end of the queue (ahead of any new methods added at that stage). Each time the method at the head of the queue is shuffled to the back, M accepts. Otherwise, M rejects. Suppose that the presupposition of some method in the regress is satisfied. Let H_n be the first such. Suppose that $n > 0$. Then H_0 is false since H_0 entails H_n. By directedness, each preceding presupposition is wrong. So M_n converges correctly rejection. Each preceding method converges incorrectly to acceptance and each succeeding method converges correctly to acceptance, so M_n is the unique method that converges to rejection. So eventually M_n comes to the head of the queue after it has converged to rejection and M converges correctly to rejection at that stage, as required. Now suppose that $n = 0$. Then all of the methods converge to acceptance so M accepts infinitely often, as required. Observe how the reduction in this example unravels the rhetorical game of responding to challenges with methods that second-guess presuppositions into an ongoing game of inquiry against nature whose object is finding a correct answer. This is a model, perhaps, of how rhetorical and reliabilist conceptions of science can be reconciled.

6 CONCLUSION

Scientific method may be conceived as a justifying argument or as a procedure aimed at finding a correct answer. Both conceptions raise a question about the propriety of infinite empirical regresses, whether of reasons or of methods checking methods checking methods. Since it is hard to say what evidential justification is for, it is hard to bring the notion of infinite regresses of reasons under firm theoretical control. The procedural concept of methodological equivalence, on the other hand, allows one to "solve" for the best single-method performance that a given kind of regress is equivalent to. Without extra constraints, both finite and infinite regresses can be worthless in terms of equivalent single-method performance. However, some motivated conditions on regresses can result in nontrivial regresses that achieve sufficient power to address Duhem's problem. Other interesting sorts of regresses might be brought under the same, complexity-theoretic discipline.

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