Rationally warranted promise: the virtue-economic account of pursuit-worthiness

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Abstract

Pursuing a scientific idea is often justified by the promise associated with it. Philosophers of science have proposed a variety of approaches to such promise, including more specific indicators. Economic models in particular emphasise the trade-off between an idea's benefits and its costs. Taking up this Peirce-inspired idea, we spell out the metaphor of such a cost-benefit analysis of scientific ideas. We show that it fruitfully urges a set of salient meta-methodological questions that accounts of scientific pursuit-worthiness ought to address. In line with such a meta-methodological framework, we articulate and explore an appealing and auspicious concretisation—what we shall dub "the virtue-economic account of pursuit-worthiness": cognitive benefits and costs of an idea, we suggest, should be characterised in terms of an idea's theoretical virtues, such as empirical adequacy, explanatory power, or coherence. Assessments of pursuit-worthiness are deliberative judgements in which scientifically competent evaluators weigh and compare the prospects of such virtues, subject to certain rationality constraints that ensure historical and contemporary scientific circumspection, coherence and systematicity. The virtue-economic account, we show, sheds new light on the normativity of scientific pursuit, methodological pluralism in science, and the rationality of historical science.

Keywords: *pursuit-worthiness, research heuristics, theory choice, theoretical virtues, reflective equilibrium, Laudan, Kuhn*

I. Introduction

Scientific promise causes much head-scratching for practising researchers. Frequently, they must decide what hypotheses, models, research programmes, etc. to pursue: what ideas to work on when they aren't sufficiently developed yet, and/or lack conclusive evidence? For example, physicists have mooted a variety of theories beyond the Standard Model of particle physics, extensions of our currently best fundamental theory of matter. To date, none of these theories has accrued cogent evidence. Nonetheless, many firmly held (and still hold) that inquiry into these theories was (and still is) worth the effort. Are such convictions regarding their pursuit-worthiness justified? To what extent do they go beyond pious hopes?

Several criteria for an idea's rational promise (or pursuit-worthiness—the term we'll henceforth use) have been proposed in the philosophical literature. The main ones include the rate at which the idea solves scientific problems (Laudan 1977), the idea's empirical fertility and conceptual viability (Whitt 1992), certain epistemic values (Douglas 2013) and the idea's potential coherence (Šešelja & Straßer 2014). On which basis should we assess such criteria of pursuit-worthiness?

Principled adjudication and legitimation of criteria for pursuit call for *meta*-methodological reflections; we need a more abstract evaluative framework for judging the adequacy of proposals for pursuit-worthiness (cf. Nola & Sankey, 2007, Ch. 4, limited to the distinct, and more traditional, context of evaluation, associated with "*acceptance*", see below **§II**). Hitherto, explicit meta-methodological considerations are rare in the literature on pursuit-worthiness. As a result, it's difficult to systematically and transparently fathom progress.[1]

The present study will try to overcome this stalemate. An appealing and natural meta-methodological framework will form our starting point—one, in fact, that finds plenty of fruitful applications in other areas (and walks of life!). We thereby hope to advance the debate on pursuit-worthiness. The framework also suggests a promising new approach towards criteria of pursuit-worthiness that the bulk of the paper will unravel: what we'll dub the "virtue-economic account of pursuit-worthiness". Synthesising ideas by Peirce, Laudan and Kuhn, it concretises the economic framework, by centrally invoking theory virtues.

Our meta-methodological framework heeds an "economy of research", as forcefully urged by Peirce (Rescher, 1976; McKaughan, 2006). Consider the analogous case of *health* economics (e.g. Guiness & Wiseman, 2011). Its models deal with recommendations for decision-making in the medical sector: to which healthcare programmes or projects should one allocate (inevitably scarce) resources and funding, in a way that creates the greatest benefit for the targeted group of recipients (op.cit., Introduction & Ch.1)? The task has its counterpart in the decision-making that *scientists* face when musing about which ideas to pursue. In both cases, a course of action must be fixed on which deploys resources most efficiently to achieve the desired outcomes.

The perspective from an economy of research seems eminently fecund when it comes to pursuit-worthiness. First, it programmatically urges a bunch of relevant questions, both philosophical-epistemological *and practical*: what are potential cognitive-epistemic gains in

[1] The absence of meta-methodological reflections is as regrettable as it is surprising. Laudan (as we'll see in **§II**) was one of the first philosophers of science to draw attention to the peculiarities of assessments of pursuit-worthiness—rather than assessments of acceptance (or rational assertability). At the same time, Laudan pellucidly underscored the importance of meta-methodology for principled assessments of methodological proposals, primarily within his so-called Normative Naturalism (especially, 1996; Donovan et al., 1992). The latter in fact involved explicit meta-methodological tenets. To the best of our knowledge, however, Laudan never applied Normative Naturalism's meta-methodology to the context of *pursuit*; it remained limited to the context of acceptance.

scientific pursuit? What are the relevant costs? How to trade off those benefits and costs? How to factor in considerations of risk and uncertainty of research outcomes? From whose perspective are such assessments supposed to be made? From the vantage point of meta-methodology, the economic ansatz zeros in on issues which *any* methodology of pursuit-worthiness ought to address—issues that most of the extant proposals skirt.

Secondly, the framework has a naturally built-in link to *rational* advice for action-taking. Extant accounts of pursuit-worthiness tend to piggy-back on their proponents' more specific further methodological and philosophical views. By contrast, an ansatz for pursuit-worthiness that conforms to an economic meta-methodology—an ansatz, that is, that fleshes out the preceding questions—can plausibly by-pass such strong commitments. It realises, or instantiates, a fairly *standard* decision-theoretic tool, with sundry applications in health policy or political science (see e.g. Allingham, 2002).

Thirdly, we'll showcase the research-economic framework's fertility. To this end, our paper's main focus will lie on exploring a natural *concretisation* of it, our *virtue-economic account* of scientific pursuit-worthiness. Its basic idea is to cash out benefits and costs, in an *idealised cognitive/scientific* sense: as the actual, or the prospect of, cognitive theory virtues (such as explanatory power, coherence, and simplicity) that an idea or hypothesis may reasonably be expected to instantiate. To competently assess those costs and benefits and their cost/benefit trade-off, evaluators must be scientifically knowledgeable and skilled, as well as exhibit certain intellectual-moral qualities. Such assessors' "cognitive utility estimate" then consists in the reasoned weighing of cognitive costs and benefits of the various ideas to be pursued: they compare (rank) the various theory virtues and how they trade off against each other. The trade-off judgements are supposed to respect deliberative rationality by implementing reflective equilibrium (see Brun, 2020): far from being concocted whimsically, the abstract trade-off scheme—the ranking or ordering of virtues—that the assessor applies must be such that the ordering or preference structure matches her judgements in comparisons of other cases (typically taken to be paradigmatic).

The virtue-economic account allows exciting interactions with the flourishing literature on theory virtues—Kuhn's (1977) stance on value judgments, in particular (whilst, thanks to the demand for deliberative rationality, forestalling potential misgivings about arbitrariness and a-rational subjectivism (allegedly) inherent in Kuhn's position, cf. Laudan, 1996, pp.89). The account furthermore sheds fresh light on a series of issues surrounding scientific promise: the normativity of scientific pursuit-worthiness, methodological pluralism in science, and the normative standards that can facilitate historiographical analysis.

Our **plan for the paper** is as follows. **§II** revisits and refines Laudan's distinction between two modes of theory appraisal: the context of *acceptance*, concerned with an idea's epistemic accomplishments, and a distinct one, the context of *pursuit*, concerned with assaying an idea's promise. As an abstract scheme for assessing questions of pursuit-worthiness, we'll then, in **§III**, introduce the economic framework of pursuit-worthiness. For the specific purposes of theory appraisal, as they arise in traditional philosophy of science, **§IV** propounds a concretisation of that framework, the *virtue-economic* account of pursuit–worthiness. In **§V**, we'll demarcate our account from Kuhn's, to which it bears some prima facie resemblance**.** **§VI** analyses further merits of our account. We'll summarise our findings and conclude in **§VII**.

II. Context of pursuit—context of acceptance

Prior to articulating specific criteria justifying (or dissuading) an idea's pursuit in subsequent sections, here we'll hone in on different types of theory appraisal. For this, a reminder of Laudan's taxonomy of cognitive stances will be rewarding (**§II.1**). Our paper's focus will be on the context of "pursuit". **§II.2** clarifies some of the characteristic features of theory appraisal in this context.

II.1. Cognitive stances and theory choice

The fundamental problem of methodology is theory choice: which theory/assumption/hypothesis[2] should scientists adopt? As Laudan (1996, p.77) stresses "(t)here is a broad spectrum of cognitive stances which scientists take toward theories, including accepting, rejecting, pursuing, and entertaining." These kinds of theory appraisal involve "distinct stances that a community or an individual scientist can take towards a theory" (Barseghyan & Shaw, 2017, p.3). One should "(distinguish) sharply between the rules of appraisal governing acceptance" and the "rules or constraints that should govern 'pursuit' or 'employment'" (Laudan, 1996, p.111).

Adopting the attitude of acceptance one is preoccupied with "warranted assertibility" (Laudan, 1977, p.110). Considerations of theory acceptance revolve around questions of evidence, confirmation, support, etc.: does the theory show indications that it's likely to be true (or at least that scientists are licensed, or perhaps even ought, "to treat it as if it were true", op.cit., p.108)? This kind of appraisal has been the predominant, and in fact often exclusive, focus of

[2] Following widespread practice in the philosophy of science literature, we'll limit ourselves to what henceforth we'll subsume under "ideas" as the objects of methodological appraisal: theories, research programmes, hypotheses, models, etc. An extension to experiments, measurements, observational missions, etc. lies outside of the present paper's ambit.

much of traditional philosophy of science—the domain of Reichenbach's (1938) "context of justification".

By contrast, the context of pursuit scrutinises questions of further investigations and rationally warranted *promise*: does a theory or, more loosely, an idea deserve further development, and study? Should future research efforts be spent on it? "To consider a theory worthy of pursuit amounts to believing that it is reasonable to work on its elaboration, on applying it to other relevant phenomena, on reformulating some of its tenets" (Barseghyan and Shaw 2017, p.3).

Considerations of—and criteria for—acceptability and for pursuit often come apart. "Many, if not most, theories deal with ideal cases. Scientists neither believe such theories nor accept them as true. But neither does 'disbelief' or 'rejection' correctly characterize scientists' attitudes towards such theories" (Laudan, 1996, p.82).[3] Moreover, while certain features of a theory, such as its simplicity or unificatory power, may not be sufficient to accept it, they furnish good reasons for further investigation. Or so we shall argue in **§IV** (extending ideas in e.g. Nyrup, 2015; Wolf & Duerr, 2024; Fischer, 2024a).

II.2 The context of pursuit

The notion of pursuit itself calls for illumination. Achinstein (1990, p.195, our emphasis) offers a helpful first pass: "(b)y 'pursue' H, I mean to include a host of things scientists and many others typically do when they *work out* their ideas, including formulating H as precisely as possible, relating it to other hypotheses, applying it to new areas, drawing out consequences and testing them. What I mean to exclude is taking some epistemic stand with respect to it, such as believing it, or believing that it is probable, or believing that it is more probable than it was before considering competitors." The goal behind pursuit is explorative: when pursuing an idea (including a highly speculative, or an inchoate one), one hopes to learn more about and develop/refine it. In this, one isn't necessarily committed to it epistemically sensu stricto. That is, one needn't believe it to be true or the best available explanation.

Assessments of pursuit-worthiness aren't intended as *rivalling*—let alone, replacing—other forms of theory appraisal: they don't compete with assessments of truth (or adequacy) or epistemic warrant.[4] Each figures in different *stages* (or phases) of research (see also Nickles,

[3] A similar case concerns toy-models (Wolf & Duerr, 2023, fn.21), such as the Ising Model of ferromagnetism or Schelling's model of social segregation. They are *known* to be "false" in that they grossly and deliberately distort their target systems. Despite their forlorn epistemic credentials that render them irredeemably unacceptable, their exploration often yields valuable scientific insights; they are widely considered pursuit-worthy.

[4] We *don't* regard appraisal of pursuit-worthiness as a form of (or even akin to) meta-empirical theory *confirmation* (as envisaged by e.g. Dawid, 2013, 2019; cf. Cabrera, 2021 for a similar critique).

2006, pp.164). As Peirce underlined (see e.g. Rescher, 1976, sect.1; McKaughan, 2008), practical and theoretical limitations force upon science a division of inquisitive labour. Early on, scientists need “guidance through the *embarras de richesses* of alternative possibilities to determine priorities”. This stage of research has “to do with the elaboration of possibilities and the provision of possible explanations and hypotheses for the solution of scientific problems” (op.cit., p.72). Considerations of pursuit-worthiness prevail in—and are apt for—here. Considerations within the context of acceptance, and tests in particular,can follow suit. The subsequent stage, accordingly, is “concerned with the narrowing of this range of alternative possibilities in an endeavor to determine which is in fact correct (or at any rate is the most promising candidate for correctness in the epistemic circumstances at hand)” (ibid.). Fulfilling different functions in distinct modi operandi of science, evaluations of pursuit-worthiness and of acceptability differ. Three regards stand out (see Nickles, 2006 for a detailed discussion). First, within the context of pursuit, forms of reasoning are regularly utilised that would be deemed suspect, if not fallacious, for acceptance: analogical reasoning, inspiration from similarities, heuristic rules-of-thumbs, etc. “These are notoriously weak modes of reasoning when it comes to justifying theory acceptance, yet they can provide invaluable ‘intuition pumps’ in contexts of innovation and [pursuit] and legitimate modes of persuasion in making research choices” (op.cit., p.166). As far as rigour is concerned, the standards of reasoning in the context of pursuit are usually lower than those for epistemic-evidential considerations (Whitt, 1990, Franklin, 1999, Ch.6). Given the different goals in the two phases, this comes as no surprise: for appraising pursuit-worthiness, one prioritises the rough-and-ready pre-selection of auspicious, stimulating ideas—a process eo ipso not obeying austere rules and criteria of rigour. Frequently, no evidence is even available yet. Decisions to further pursue an idea are then made *with the hope* of future tests whose details are precisely what further inquiry should reveal. The context of pursuit summons scientific creativity and imagination to aid researchers’ vision beyond the theory’s *present* accomplishments, and to probe its *prospects* (see also Sánchez-Dorado, 2020, 2023).[5]

Secondly, epistemic considerations often *bear on*—and co-determine—considerations of pursuit-worthiness (without the latter being reducible to the former, see Nickels, 2006, sect.3). After all, researchers usually hanker after empirically-evidentially *successful* hypotheses. Hence, an idea’s *preliminary* empirical-evidential success can legitimately spur researchers

[5] Such laxity in standards of reasoning seems inevitable if one wants to solve what Laudan & Laudan (1989) call the “innovation problem”: “Why should scientists ever abandon an accomplished theory with a strong record of explanatory and predictive success in favor of an upstart model that so far has little empirical support and that may suffer from conceptual problems as well” (Nickles, 2006, p.172).

on to further pursue it.[6] Although *empirical-evidential* demands for pursuit are typically lower than for acceptance, it would be rash to conclude that criteria for pursuit-worthiness *in general* are just watered-down versions of those for acceptance. Some criteria for pursuit arguably play no straightforward, uncontroversial role in the context of acceptance, super-empirical considerations in particular. In this respect, considerations for pursuit can be *more* demanding than those for acceptance: their promise must enthral scientists—often ”in defiance of the evidence” (Kuhn, 1996, p.158). Qualms about invoking theory virtues (such as simplicity, explanatory scope, etc.) as reasons for acceptance are legion in the context of acceptance (e.g. van Fraassen, 1980, esp.Ch.4.4; McMullin, 2013; Ivanova, forth., pace e.g. Schindler, 2018). In the context of pursuit they provide guidance for theory choice in a much less controversial way—not seldom *faute de mieux*.

A third difference concerns pluralism. The context of pursuit tends (and ought, see **§VI.3**) to be more congenial to it than the context of justification (see Nickles, 2006, pp.161). This is a corollary of the already mentioned less strict standards for evidential credentials, and the different modes of reasoning. Such differences in permissiveness reflect the chief goals in the two phases of research. In the context of pursuit, the primary aim is to foster innovation and exploration, rather than more definitive epistemic appraisal. By itself, such an aim isn’t per se exclusivist: two—not yet evidentially-epistemically established—theories can peacefully coexist. Their promise may, for instance, lie in different areas. In fact, in the context of pursuit pluralism, “the method of multiple working hypotheses” (Chamberlin, as cited in Laudan, 1980) has indeed been argued to especially enhance the development of science (ibid.; Chang, 2012, Ch.5). By contrast, the context of justification is less permissive: the co-existence of empirical-evidentially underdetermined rival theories spells a quandary for the quest of identifying the *best* account available (see e.g. Stanford, 2023).

In summary, while assessments of pursuit-worthiness tend to lower the bar for traditional epistemic-evidential standards and are more congenial to pluralism, they *raise* it in other regards. Our account for assessing pursuit-worthiness retains these distinctive features. It also naturally *explains* them and their underlying rationality through the norms of theory-choice in the context of pursuit. With these promissory notes, it’s time now to turn to our account. We commence with a general *framework*.

[6] This is plausibly reflected in the significance scientists tend to attribute to predictive novelty (see e.g. Douglas & Magnus, 2013; Schindler, 2018, Ch.3): novel predictive successes are taken to be (tentative) indicators of *further* empirical successes, and hence boost a theory’s pursuit-worthiness.

III The economic framework: a natural decision-theoretic ansatz for pursuit

This section will present the (meta-methodological) framework that will shape our subsequent (methodological) discussion in **§IV**. Its main idea is borrowed from economics: decisions of whether or not to pursue scientific ideas should be adjudicated on the basis of estimated costs and benefits.

We'll take our cue from Peirce: "(p)roposals for hypotheses inundate us in an overwhelming flood, while the process of verification to which each one must be subjected before it can count as at all an item, even of likely knowledge, is so very costly in time, energy, and money" (cited in McKaughan, 2008, p.456 This suggests that questions of pursuit-worthiness can, and should be, treated akin to economic decisions involving investments under uncertainty: in both cases, we strive to optimise resource allocation—to get the biggest bang for our buck.

Within such an economic framework one would trade off the expected epistemic gain or output of a research project against the likely costs associated with it. The expected epistemic gain, in turn, depends on assumptions about how valuable the project's potential outcomes are and how likely the project achieves them. For example, finding and potentially confirming a new theory of Beyond the Standard Model Physics (BSM) may be valued highly by a community of researchers. But in order to evaluate the overall pursuit-worthiness of a research project associated with that theory one also has to factor in how likely the search for the theory will succeed, and how large the expected efforts or costs will be.[7]

The framework may be applied in one of two ways. The first is to recommend scientific pursuit simpliciter iff the expected epistemic gains stand in a particularly favourable relation to the costs. This yields a *more absolute* sense of pursuit-worthiness: is the idea pursuit-worthy at all? For another, *comparative* sense, one must decide whether one project P is more pursuit-worthy than another project, Q: for a pool of projects and limited resources, how to spend the latter on the most promising project(s)? For such comparisons, the directive would be: pursue P rather than Q iff the trade-off relation is more favourable for P than for Q. Here we can distinguish three cases. First, suppose that P and Q are associated with the same (prima facie) efforts. Then P is more pursuit-worthy iff it has the higher expected epistemic gain. Secondly, suppose P and Q have the same associated expected epistemic gain. Then P will be strictly more pursuit-worthy iff it achieves the epistemic gain with less efforts. Thirdly (and most

[7] The framework is open for a qualitative, as well as a more quantitative interpretation. In what follows, we'll keep our discussion to a qualitative understanding of costs and benefits, as seems adequate in the context of theory evaluation. Quantitative considerations may come in, e.g., at the stage of experiment planning. But even then, it's difficult or even impossible, we submit, to relate epistemic benefits to financial costs in a straightforwardly quantitative way, especially in foundational research.

commonly), both the costs and the expected epistemic benefits of P and Q differ. In such situations the framework requires a more detailed analysis of the individual gains and costs (see the discussion in Sec. **IV**).

Note that the framework acts at the level of meta-methodology. On its own, it doesn't issue any specific criteria for pursuit-worthiness. Instead, it concerns questions of how such criteria are to be justified and to which (more concrete) methodologies of pursuit must conform. To show the fertility and plausibility of such economic constraints/considerations, let's briefly glance at two extant approaches to pursuit-worthiness.

Consider Laudan's "rate of progress" criterion. Laudan deserves credit for drawing a sharp distinction between pursuit and acceptance as distinct cognitive attitudes for theory choice. That the two contexts call for distinct criteria is explicitly built into his proposal. Assessments of acceptability are predicated on a theory's past record of achievements: for Laudan, the total number of successfully solved (and suitably–by a research tradition's inherent standards–weighted) problems minus the number of unsolved ones and plus potentially newly incurred ones. The indicator of pursuit-worthiness, by contrast, for Laudan, is the *rate* of problem-solving, i.e. the theory's most recent achievements per time unit.

Two critical comments on Laudan's proposal highlight advantages of the economic perspective. First, Laudan's "rate of progress"-criterion presupposes a *linear* extrapolation. In order to draw conclusions for the present pursuit-worthiness one would have to assume that the past trend continues. This assumption is rarely adequate. The checkered history of, say, the corpuscular theory of light, or in more recent times, of string theory, attest to that.

Secondly, the rate of progress passes over a prima facie relevant factor: some research projects are pursuit-worthy *despite* low rate of progress because *too few* researchers are working on them. For instance, General Relativity enjoyed an early phase (from 1915 until the late 1920s) of intensive research efforts that bore stately fruits. That decade of blossoming was followed by a "low-tide" between 1925-1955, when general-relativistic physics stagnated, and was even shoved outside the physics mainstream (Eisenstaedt, 1986, 2003)—to be resurrected triumphantly, both in terms of community size and scientific output, in the mid/late 1950s. If rate-of-progress is to be seen as a necessary criterion for pursuit-worthiness, then General Relativity would have to be judged non-pursuit-worthy during that low-tide period. From the perspective of the economic framework, a low rate of progress needn't entail low pursuit-worthiness: General Relativity had merely accrued too little attention. According to the economic framework, had more researchers invested their efforts, it likely would have progressed faster.

The kind of cost-benefit analysis we are suggesting is, we submit, a more reliable indicator than Laudan's rate-of-progress criterion. The former even offers a deeper explanation for the latter. Whenever pursuit-worthiness correlates with rate of progress, a fast rate of progress suggests that high epistemic benefits can be reaped with relatively low additional efforts. For A to be more pursuit-worthy than B on the basis of rate of progress it suffices if costs are roughly similar but rate of progress of A is higher.

It's useful to juxtapose the economic framework and DiMarco and Khalifa's (2022) recent "apocritic" proposal. It too can be understood as a meta-methodological framework for studying pursuit-worthiness considerations. DiMarco and Khalifa distinguish between "apocritic" obligations and prohibitions. An apocritic obligation has the following structure: "If a question Q about object of inquiry x has feature F, then some scientists with capability C should pursue question Q about x." Moreover, there are apocritic prohibitions: "If a question Q about object of inquiry x has bug B, then no scientists should pursue question Q about x." It should be noted that DiMarco and Khalifa's approach has a broader scope than our framework, including obligations and prohibitions that aren't purely epistemic. We admit that a strict separation between epistemic and non-epistemic obligations and prohibitions isn't always strictly possible (see e.g. Longino, 1996). Nonetheless, our focus will be on those values that tend towards the more epistemic end of the spectrum.

In a few regards, our framework complements DiMarco and Khalifa's approach. First, their proposal, whilst citing obligations and prohibitions, doesn't spell out the concrete directives that follow from them. In particular, they don't provide a systematic framework for weighing obligations and prohibitions against each other. But this is needed for criticisms of specific scientific pursuits. Most research projects are stained by drawbacks (e.g. the need for animal experimentation, or opportunity costs). The clincher is whether the epistemic payoffs *outweigh* (and thus potentially justify) such drawbacks. The advantage of the economic framework is that it builds in the comparison from the start.

Relatedly, DiMarco and Khalifa's approach seems to be geared towards absolute/binary evaluations: whether a question should be pursued or not. Our framework, by contrast, allows *graded* judgements of pursuit-worthiness. This is an advantage. For example, funding bodies aren't always interested in whether a research proposal is pursuit-worthy or not. Instead, they peruse a pool of proposals; they then decide which of the most promising ones to fund with the resources available. An advantage of our economic framework is that it provides a basis for such comparisons from the get-go.

Finally, DiMarco and Khalifa relativise obligations and prohibitions to scientists' capabilities. This, however, is somewhat a red herring: what seems to matter for pursuit are costs, material (say, money for requisite equipment or training) or intellectual (say, cerebral efforts). They are lowered by infrastructure already in place, or existing experience and expertise on the researchers' side, respectively. While DiMarco & Khalifa rightly emphasise capabilities—as researchers' abilities, background knowledge, and skills—the economic framework puts the finger on the *more basic*, and general, component of pursuit-worthiness (viz. costs) and how researchers' background knowledge and abilities are related to it (viz. by lowering costs).

The economic scheme outlined in this section meshes with the guiding thought employed in modelling for decision-making under scarce "resources" and uncertainty commonplace in virtually any decision that needs to address the public's financial matters, such as tertiary education and medical care. But it instantly also prompts iffy questions:

A. What exactly are the epistemic gains in question (e.g. contributions to GDP, resulting from technological applications of the theory, or some loftier outputs of science, such as truth)?
B. What would correspond to the "costs" in question (e.g. public money)?
C. Who are supposed to be the decision-makers? And relatedly: might different agents (e.g. different funding bodies) not hope for different epistemic gains, and incurred different costs?
D. What is the common measure that allows a comparison not only amongst the epistemic gains but also between gains and costs?

In what follows, we'll explore the viability and appeal of one set of answers to these questions. It forms what we'll call "the virtue-economic account of pursuit-worthiness". It pivots on a cost-benefit analysis for the cognitive goods and costs in an ideal science; those goods and costs are plausibly cashed out in terms of the instantiation (or non-instantiation) of theory virtues (cognitive values). This focus on cognitive aspects of an ideal science helps clarify the idea of a cost-benefit analysis as it naturally finds its home in the actual practice of science, historical and contemporary.

IV The virtue-economic account of pursuit-worthiness

This section will unpack the economic framework in a version that directly links it to the aims of science, traditionally identified in the philosophy of science literature. Building on suggestions by Kuhn (1996, postscript; 1977), Whitt (1992), and Lichtenstein (2021), we'll flesh out the idea of a cost/benefit analysis in terms of theory virtues (or cognitive values): the surmised instantiation of differentially ranked theory virtues, we propose, functions as an index of pursuit-worthiness. Constraints on the deliberation process through which the virtues are weighed ensure rationality in a robustly objective sense (*absent*, for instance, in Kuhn, cf. Laudan, 1996, pp.98; Nola & Sankey, 2000, sect.8).

To adumbrate our following elaboration, let's list, in broad brush strokes, our answers to the above questions that the economic ansatz urges (**§III**)—the upshot of our virtue-economic account of pursuit-worthiness:

> Ad A. Pursuit aims at the attainment of theories that one has reason to expect will achieve science's cognitive *goals*: powerful explanations, understanding, and empirical adequacy. The likely prospects of an idea exhibiting empirical or super-empirical theory virtues, such as explanatory or unificatory power, or coherence, indicate the realisation of those goals; we therefore propose theory virtues as indices of pursuit-worthiness.
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> Ad B. Pursuing scientific ideas dissipates research efforts: pursuing a project, one expends time, mental and material resources—the costs for investigating it. While the *costs* for real individuals (or groups) vary, useful (albeit idealised) objective proxy indicators are again certain—more pragmatic—theory virtues (e.g. simplicity and familiarity).
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> Ad C. Ultimately it's individual researchers who must decide (or assent) to actually pursue an idea. Yet, an idea's pursuit-worthiness can be appraised by any person (or group of persons), insofar as we have reason to believe that she's scientifically competent and displays certain intellectual virtues (such as impartiality, and probity).
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> Ad D. Our account works even in the absence of a universal common measure: epistemic gains and costs are weighed by assessors (meeting the foregoing criteria), with empirical theory virtues (as per our response to A) typically given especially strong weights. Assessments of pursuit-worthiness are—like many other important decisions in science—deliberative judgements. This explicitly allows for rational disagreement.

In several respects, our proposal for evaluating pursuit-worthiness is unabashedly idealised. For instance, the actual goals of actual decision-makers (including scientists!) may—and typically do—deviate from the idealised, "purely cognitive" ones that our account traffics in. Nonetheless, the idealisation doesn't detract from our account's value. Focusing on somewhat idealised agents and cognitive aspects is a natural restriction for (normative) philosophy of science. Insofar as methodological evaluations operate in the abstract (as they traditionally do), our account is no worse off than what is customary for methodological proposals. Should one covet a *more* (psychologically and sociologically) *realistic* model of the decision-making situation, one must include many more factors. *Amongst* those factors within such a de-idealised model will be—suitably weighted, depending on the involved concrete agents' preferences—cognitive costs and benefits (see e.g. Kitcher, 2001; cf. Shaw, 2021). Even for such more complex, real-life decision-theoretic questions, our idealised account will be useful: the account will *inform* them.

The subsequent subsections will successively expand on our above sketched answers, which form the core tenets of our account of pursuit-worthiness: what the benefits (**§IV.1**) and costs (**§IV.2**) are, who qualifies as a competent evaluator (**§IV.3**), and finally how she is supposed combine costs and benefits in a utility estimate (**§IV.4**).

IV.1 Cognitive benefits

According to our account, an idea's positive pursuit-worthiness derives from its cognitive benefits. These we identify with the idea's expected instantiation of certain cognitive (or theory) virtues.

As stressed previously, the virtue-economic account proffers an evaluative framework for gauging the pursuit-worthiness of ideas in its cognitive, *inherently* scientific dimensions.[8] We propose to equate the payoffs with attainment of the epistemic/cognitive aims of science: an idea counts as a cognitive benefit, iff it realises a cognitive/epistemic value, such as empirical accuracy, explanatory, predictive and unificatory power, and understanding (see e.g. Nola & Sankey, 2007. Ch.2). About those cognitive goals we opt for pluralist permissiveness.[9]

Specifically, ideas qualifying as cognitive benefits encompass hypotheses, assumptions, theories, interpretations, models, classification systems, theoretical frameworks, etc. that

[8] In the terminology of Fleisher (2022), we limit our considerations of "inquisitive reasons" to "promise reasons", bracketing "*social* inquisitive reasons" and idiosyncratic-personal ones.

[9] This pluralism of aims—an attitude congenial to our overarching economic perspective, which also in other areas recognises the plurality of goals—ameliorates another aspect of Laudan's (1977) account of pursuit-worthiness in terms of problem-solving rate. What *precisely* Laudan means by "problems" and their solution remains vague (cf. Nickles, 1981). It's therefore difficult to judge which elements of the broad array of commonly adduced aims of science (deep and comprehensive explanations, coherence, understanding, etc.) count as "problem-solving". Insofar as they don't, Laudan owes us an explanation why they ought to be discarded as aims. Absent that, it would seem desirable for any account of pursuit-worthiness to do justice to the plurality of prima facie legitimate aims of science. Moreover, it would behoove such an account to heed the peculiarities of the context of pursuit (see **§II**). In particular, Laudan's proposal "does not quite capture what scientists mean by 'promise'. It neglects the assessing of the resources of the theory for further development both of problems and of solutions, as well as of the likelihood that it will be able to incorporate all or most of the results that earlier theories successfully explained. 'Promise' is a matter of estimating what lies ahead; though the rate of past problem-solving success is undeniably relevant to it, a measure of this latter alone is only one of the clues that such an estimate would have to rely on" (McMullin, 1979, p.638).

achieve the cognitive aims of scientific inquiry: the formulation of predictively and explanatorily powerful theories, handy, versatile and adequate models (cf. Parker, 2010, 2020), the application of theories to new domains, the proof of substantial theorems, or informative and coherent classification/taxonomic systems (cf. Schindler, 2018, Ch.3.5).

Cognitive benefits can be parsed into intrinsic (or direct) and extrinsic (or indirect) ones. The former denote cognitive payoffs that would be gained *directly* by the idea-to-be-pursued itself (if the hopes pinned on the idea pan out). An empirically well-corroborated theory that satisfactorily explains motley phenomena, is a case in point (say, Darwinian evolution). *Extrinsic* cognitive benefits, by contradistinction, denote cognitive benefits not *directly* resulting from the idea-to-be pursued. Rather, extrinsic benefits are spin-offs (e.g. better understanding of certain measurement or calculational techniques) that are sparked off *as a by-product* of pursuing the idea, irrespective of its *ultimate* success. Toy models—gross simplifications or distortions (as in Schelling's segregation model in sociology), occasionally even counterfactual/counternomic possibilities (as in the 2-dimensional Ising model in statistical mechanics)—typically fall into this category (see e.g. Reutlinger et al., 2018).

Our account attributes cognitive values a pivotal role: the prospect of their instantiation figures as our preferred positive index of pursuit-worthiness. Let's inspect those values more closely (see also McMullin, 1982, 1996; Laudan, 2004; Nola & Sankey, 2007, Ch. 2.2; Douglas, 2009, 2013; Schindler, 2018). Which ones in particular are relevant? And why should we elevate them to indices of pursuit-worthiness?

In the main, we concur with Kuhn (1977, for differences, see **§V**), or Keas (2018, for an extended list and taxonomy), on the most important cognitive[10] values :

- accuracy: the fit with empirical evidence
- unificatory power: the ability to connect hitherto disparate phenomena
- explanatory power and explanatory depth
- consistency
- internal coherence: the organic and harmonic order of basic principles, in virtue of which the elements hang together

[10] We acknowledge that the distinction between cognitive/non-cognitive values can occasionally be blurry; borderline cases may exist whose status varies across disciplines (say, immunology or climate science vs. gamma-ray astronomy or geochemistry). Our choice is tailored to physics and the exact sciences. Assimilating McMullin's (1982, pp.18) proposal (so as to match our pluralist stance towards the aims of science), we demarcate cognitive from non-cognitive value through their *function*: "(w)hen no sufficient case can be made for saying that the imposition of a particular value on the process of theory choice is likely to improve the [cognitive status of the theory]". Conversely, cognitive values constitute, or are conducive to, the realisation of the aims of science; they circumscribe the "internal standards" of scientific inquiry.

- external coherence: compatibility, and ideally coherence, with other parts of our knowledge
- fertility and heuristic power: the resources for generating further innovation, for example for expanding the idea's scope or giving rise to novel predictions
- simplicity (syntactic, or ontological)

Our key claim is (taking up a suggestion by Douglas, 2013) that these virtues (listed non-exhaustively) define the standards for theory choice for pursuit (as well as *to some extent* to acceptance). But then how does assessing pursuit-worthiness in terms of auspicious instantiation of cognitive virtues differ from considerations of cognitive virtues in the context of acceptance? How to delimit criteria of theory acceptance from those for theory pursuit, if both contexts invoke cognitive virtues? Three differences stand out.

The first concerns the *modality* of the virtues' instantiation. An assessment of pursuit-worthiness often involves *not yet* actually—or at least not manifestly—instantiated virtues, only the likely prospect thereof. By contrast, theory assessment in the context of acceptance requires an idea's *actual* achievements. The allure of Common Origin Inferences (Janssen, 2002; forth.) illustrates the point. These are scientific hypotheses that "(trace) some striking coincidences back to a common origin (typically some causal structure or mechanism)" (op.cit., p.458). Darwin and Einstein, for instance, traced a variety of phenomena (life on earth, and contractions and other coincidences in 19th-century ether theory, respectively) to a common origin (a common ancestor, and the new space-time structure of Special Relativity, respectively). The *prospect* of those ideas' success, on the virtue-economic account justified their pursuit. In line with the historical attitudes towards the two ideas in the scientific community, acceptance demands more stringent evidential standards.[11]

This segues into the second key difference: the staple canon of values for acceptance is usually small. Besides consistency and a modicum of internal coherence (non-adhocness), it primarily contains the "evidential-empirical" ones: external coherence, empirical accuracy, and explanatory power of the phenomena presumed to be the most salient ones. Their application is relatively strict: little tolerance is condoned for shortcomings on any of those values. Whether super-empirical virtues (e.g. fertility or simplicity) may *legitimately* enter theory appraisal in the context of acceptance requires substantial arguments. Affirmative views (such as Schindler's (2018))—as opposed to those that regard them as merely pragmatic (e.g. van

[11] A more recent example that sparked heated scientific controversy (see Parsons, 2003) before being accepted, after the discovery of the Chicxulub Crater, is Alvarez and Alvarez's observation, in the 1980s, of unusually high concentration of Iridium in thin geological layers all around the globe. To account for this, they postulated a large asteroid that hit the Earth 65 million years ago, causing the mysteriously sudden extinction of the dinosaurs.

Fraassen, 1980, esp. Ch. 4.1; Worrall, 2000), or as eliminable altogether (Norton, 2021, Ch.5)—are notoriously controversial.

By contrast, appeal to cognitive virtues when appraising pursuit-worthiness is marked by opportunism. On the one hand, the range of pertinent virtues is broader: super-empirical ones are warmly welcomed. On the other hand, standards are lower. This lenience and benevolence, in working with an idea's fortes (rather than the eagle-eyed readiness to leap on its weaknesses), express the willingness to give fledgling ideas a chance. It's owed to the epistemic precariousness, characteristic of the research phase in which questions of pursuit arise.

The third, and arguably most important, difference concerns the *kinds* of values that are prized. Recall the different priorities in the contexts of acceptance and pursuit (**§II**). In the former, one is interested in assaying an idea's epistemic-evidential credentials: does it live up to standards for belief, empirical adequacy, etc.? In short, does it *constitute* a cut-and-dry epistemic achievement? In the context of pursuit, we want to press on scientifically: to expand our horizons, to augment and to ameliorate our knowledge. Hence, when assessing an idea, we wonder: does it have the *potential* for promoting the aims of scientific inquiry (cf. Fleisher, 2022, pp.18)?

The differences in priorities percolate to differences in emphases of germane cognitive virtues.[12] Those that enjoy pride of place in the context of pursuit oftentimes don't, in any obvious way at least, indicate truth, compelling epistemic warrant, empirical adequacy, etc. Yet, they plausibly squarely promote the aims of scientific inquiry (see also Laudan, 2004; Douglas, 2013[13]). This is our main reason for including them amongst the indicators of an idea's promise (alongside the evidential-empirical virtues): the extent to which they qualify as *constituting* cognitive achievements is controversial; much less controversially, they are *instrumental* to realising those achievements. Fertility, testability (i.e. ease and informativeness of tests), unificatory power, or simplicity are subservient to the *explorative*

[12] We don't claim that all theory virtues can be dichotomised. We acknowledge that some straddle considerations of acceptance *and* pursuit. Predictive novelty is arguably a case in point (Douglas & Magnus, 2013; Carrier, 2014; Schindler, 2018, Ch.3; Wolf & Duerr, 2024, sect.7).

[13] We reject Douglas' *ranking* of the cognitive values in terms of minimal criteria versus mere desiderata for two reasons. First, it hinges on a contentious—and problematically narrow—view on the aims of science: the attainment of *truth*. Secondly, her reasoning is restricted to the context of acceptance. It doesn't automatically carry over to the context of pursuit. Considerations that Douglas adduces in our arguments are rarely available in the context of pursuit. Researchers must typically make do with much less: clues, hints, indications, rules of thumb, hunches of what looks promising etc. This makes the context of pursuit much more opportunistic and pluralistic—as Douglas (p.801) seems to acknowledge. We refrain from any *a priori*, fixed ranking of cognitive virtues—a matter better left to the competent judgement of individual scientists (subject to the constraints in **§IV.3** and **IV.4**).

thrust prevalent in the context of pursuit. In no way does this imply that evidential-empirical considerations are spurned. Insofar as intimations of them are available, they are usually hailed as encouraging hints that one seems epistemically-evidentially on the right track. For assessing pursuit-worthiness, we therefore treat the theory virtues listed above as indicators of promise. Here, we needn't take a stance on whether their implementation by itself constitutes an epistemic achievement sensu stricto.

Having identified theories and models instantiating virtues as the cognitive benefits, we can discern two dimensions of such a benefit's value, of its *cognitive quality*. One is the number, and variety, of different virtues it (plausibly) instantiates. The second dimension pertains to the *degree or extent* and likelihood to which the result instantiates (or contributes to the instantiation of) the theory virtue(s) in question. For instance, coherence—or non-adhocness—comes in degrees (Schindler, 2018, Ch.5). Even consistency is a property that a theory seldom instantiates *in toto* (e.g. Nickles, 2002). Conversely, shortcomings with respect to its instantiation of theory virtues diminish the value of an idea. Explanatory losses ("Kuhn losses"), for instance, are widely deplored as curtailing a theory's appeal.

The issue generalises in the manner adverted to (but arguably overdramatised (cf. Laudan, 1984, pp. 90) by Kuhn (1977)). First, theory virtues exhibit some interpretative ambiguity. They admit of leeway for how to construe them: different scientists may understand them differently. Simplicity is a notorious example (see e.g. Bunge, 1963). For instance, the Copernican model of the solar system is much simpler in explaining the qualitative motions of the planets than is geocentrism. In terms of the simplicity (or difficulty) of making quantitative predictions, however, the Copernican model and the geocentric model "proved substantially equivalent" (Kuhn, 1977, p. 358). Secondly, scientists tend to rank (or weight) the importance of theory virtues differently; they needn't hold all virtues on a par. The debate between Einstein and Bohr over the status of Quantum Mechanics exemplifies this. Both agreed on its predictive accuracy. Einstein's repudiation of the theory rested on the (in his view) lack of consistency with the rest of physics, and a defective internal coherence—supposed vices that Bohr disputed (McMullin, 1982, pp.16; Howard, 2007). In **§IV.3-4**, we'll place suitable rationality constraints on the weighting process to forestall apprehensions about arbitrariness and "radical individualism" (Laudan, 1984, pp.88).

Both the ambiguity of virtues and the disagreement regarding virtue ranking bear upon the nature of cognitive benefits in our account. Its *application*—that is, the appraisal of an idea's pursuit-worthiness through an *actual* agent on the basis of our account's principles—has objective components (i.e. pertaining to the idea-to-be-pursued itself), *alongside* agent-dependent ones. The latter are rooted in the agent's exercise of deliberative judgement (see also McMullin, 1982, sect.1). Whereas the instantiation of the virtues belongs to the objective side, the ambiguity and ranking issue belong to the more agent-relative side—albeit subject to constraints (**§IV.3-4**).[14]

IV.2 Costs

The costs that the virtue-economic account budgets for assessing an idea's pursuit-worthiness are its cognitive (as opposed to material) ones: the mental efforts of the ideal scientist. As indices for cognitive costs, we again propose the prospect (or actual) non-instantiation (or deficient instantiation) of cognitive virtues. For opportunity costs—as the cognitive benefits of *neglected alternative* ideas one could pursue—this is straightforward. Applying our proposal from **§IV.1**, we can identify them with those alternative ideas' prospects of instantiating (or contributing to the instantiation of) cognitive virtues.

The intrinsic cognitive costs express the sense of inherent knottiness of the research to be performed; some ideas are more difficult and laborious to pursue than others. Certain cognitive virtues (or lack thereof) encode this.[15] In part, they lower mental costs by allowing researchers to tap already existing resources and results; in part, they are related to more inherent tractability and "user-friendliness".

- *Coherence and familiarity/conservatism*. An idea hanging together with other parts of more established science allows one to import insights for the idea's further elaboration. One thereby needn't invent or produce whatever is necessary for this development. The more and the stronger the inferential links with other parts of knowledge (cf. Šešelja & Straßer, 2014), the more one can draw on them to facilitate and expedite the idea's further pursuit. The modern synthesis in evolutionary biology is a case in point. Bringing together genetics, zoology, population biology, and palaeontology, it opened up rich and multifarious sources of further inquiry for researchers from different areas (e.g. Mayr, 2001). Similar synergies fuelled (and fuel!) the pursuit of relativistic astrophysics, and astroparticle physics in particular (e.g. Falkenburg & Rhode, 2012).

[14] There is empirical reason to think that the *actual* disagreement (by scientists as agents, who embody the ideal scientist, of course, to varying degrees) tends to be much less than is occasionally suggested (see e.g. Schindler, 2022).

[15] Some virtues (especially simplicity and heuristic power) *double* in both the assessment of cognitive gains and costs of an idea. The same virtue often fulfils different functions. Heuristic power, for instance, is associated with on the one hand the prospect of extending a theory's scope—clearly an epistemic aim, cognitively valuable per se—while on the other hand, it also functions as a means towards research: its suggestiveness *facilitates* pursuit, making it thereby a feature weighing in on the side of cognitive costs.

- *Simplicity*. The simpler an idea in its mathematical, conceptual-logical/syntactic form, the more tractable it is. We have to spend fewer resources to work with it.[16] The quartic, so-called φ^4 theory is a case in point, a prototypical model of quantum field theory. Because of its mathematical simplicity, it's widely studied for applications in statistical mechanics, particle physics or critical phenomena. In the same vein, the standard (or ΛCDM) model of cosmology is pursued for primarily pragmatic reasons (Wolf & Duerr, 2024): "(w)ith some simple assumptions, [the ΛCDM] model fits a wide range of data, with just six (or seven) free parameters" (Scott, 2018, p.1).

- *Powerful positive heuristic.* The thought is neatly captured by Lakatos (1989, passim): some ideas—especially when they come in the form of broader frameworks or families of theories—come equipped with a blueprint for elaboration. This research agenda contains a set of tentative and natural directives which paths to pursue (and which to avoid), "a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research-programme, how to modify, sophisticate, the 'protective belt'" (p.50). An idea with a powerful positive heuristic is thus easier to pursue than one that requires creative leaps and tinkering from scratch at every turn. The paradigmatic example here is Newtonian celestial mechanics (Smith, 2014 for details). Thanks to its heuristic, it successfully "digested" (Lakatos) initially unaccounted for phenomena. Thereby, it produced ever more refined models of the solar system (an especially celebrated triumph being, of course, the prediction of Neptune). More recent examples of such heuristics include guiding principles such as the correspondence principle or the naturalness principle that has been invoked in the context of particle physics (Fischer, 2023, 2024b).

- *The existence of analogies and similarities* with other areas where one has already garnered expertise allow the transfer of insights (see Nyrup, 2020). Potentially useful tools for pursuing an idea are thus readily available (and don't have to be cost-intensively created). Examples of how such cognitive transfer is routinely lunged for include the gauge theoretic structure in particle physics, or renormalisation group methods (with copious applications in solid state physics, cosmology, or high-energy physics).

IV.3 The evaluator

Neither need the "evaluator" and the "pursuer" be the same person(s); nor do we claim that this appraisal ought to translate directly into decisions of actual pursuit (or sponsor pursuit). If those who evaluate an idea's pursuit-worthiness (say, scientifically savvy philosophers of

[16] This is, of course, precisely the idea behind classifying simplicity as a *pragmatic* virtue (see e.g. Worrall, 2000): its appeal lies in convenience, rather than truth-conduciveness.

science) needn't coincide with scientists who might pursue it, *whom* does our account presume to undertake the assessments of pursuit-worthiness? We propose it's the *ideal* scientist, who strives, to the best of her scientific knowledge and judgement, to realise the aims of science (rather than her own individual aims).

To cross paths with an ideal scientist (IS) is, admittedly, a rather quixotic occasion. The notion, however, encapsulates a regulative ideal. Accordingly, we stipulate, an idea's pursuit-worthiness should be appraised by an evaluator (a person or a group) *insofar* as one has reasons to assume that the evaluator lives up to, and avows that ideal. It's characterised by three features:

> **(IS-KNOW)** The ideal scientist has perfect access to the relevant scientific knowledge, available at a point in time.
>
> **(IS-AIMS)** Her goals are those inherent to science (which we'll specify further below); she doesn't aspire to other aims, aims extraneous to science.
>
> **(IS-RAT)** In pursuing those goals, and given the scientific knowledge of her time, she displays perfect rationality: against the background knowledge of her time, she invariably chooses the best means to achieve those goals.

The idealised nature of **(IS-KNOW)**, **(IS-AIMS)**, and **(IS-RAT)** is manifest.[17] No researcher—or group of researchers—possesses perfect knowledge of the scientific community. (Even the scientific community *en entier* doesn't have perfect access to all its knowledge.) By the same token, no researcher solely aspires to the scientific-communal goals. The foibles of the human mind—our notorious failures of rationality—are legion (Kahnemann, 2011). Different individuals (or groups of individuals) can embody the regulative ideal to different extents.[18]

We won't have much more to say about **(IS-RAT)** and **(IS-KNOW)**. But **(IS-AIMS)** deserves a comment. What are the goals in question? Following Popper (1972, Ch. 5), we take them to

[17] At first sight, the ideal may seem remote. Nonetheless, it arguably plays a substantive role for understanding *actual* science: it's plausible to regard the scientific community as a whole as a group agent in the sense of Pettit (2009, 2014, 2023), a self-organised system with intentional states and rationality, that arises from the complex network amongst individual scientists (without being reducible to a mere aggregate of the latter). That is, to ascribe to the scientific community as a whole knowledge states, goals and rationality—approximating the three characteristic posits characterising the ideal scientist—has explanatory power.The ideal scientist, in other words, captures a "real pattern" in the sense of Dennett (1991, 2009). We cannot pursue this line of thought here.

[18] One needn't be entirely pessimistic, though, that such an ideal departs too drastically from reality. Both at the individual level (through the internalised values of the "ethos of science", see Merton, 1973, Ch.13), as well as through the communal-institutional level (e.g. education/training, publication practices, or the reward system of science) constraints are in place to ensure that the deviations at least at the group level don't become egregious (cf. op.cit., see also Surowiecki, 2004).

include first and foremost (but *not* exclusively, see below) explanations of increasing depth, precision, and scope. We'll not embroil ourselves in what counts as a satisfactory explanation. At this juncture, we remain permissive, allowing for a broad array of types of explanations, and construals of explanatory dimensions (see e.g. Bartelborth, 2007), as well as epistemic aims more generally, including well-confirmed knowledge, accommodation, problem-solving (see e.g. Laudan, 1977; Nickles, 1981), understanding (see e.g. Elgin, 2007; de Regt, 2020).

An *actual* evaluator's appraisal of pursuit-worthiness carries rational weight to the extent that she approximates the ideal: the more we have reason to regard her as having up-to-date scientific knowledge and understanding (as per **(IS-KNOW)**), and as aligning her interests with the aims of scientific inquiry (as per **(IS-AIMS)**), the more seriously we ought to take her appraisal.

This translates into two requirements on a concrete agent serving as an evaluator: on her intellectual and epistemic faculties and her trustworthy character, respectively. First, we must have reason to believe that the evaluator has expertise: rather than being an otherworldly bureaucrat, she's required to possess substantive scientific knowledge, as well as scientific know-how (an understanding of how science works).

Secondly, for the alignment clause we need assurance that the evaluator qualifies as a "juge impartial et loyal" (Duhem 1906, p. 332). Following a suggestion by Sankey (2020)—but transferring it to the context of appraising pursuit-worthiness—we propose that in order for her to make competent deliberative judgements, it's imperative that she "adopt an attitude of detached neutrality with respect to personal interests and theoretical commitments. In appropriately performing the role of impartial judge, the scientist [in our case: the evaluator, *our addition*] behaves in a virtuous way. The virtue involved in performing as an impartial judge is not just a virtue that is cognitive in nature. It has a moral dimension as well" (op.cit., p.16). We must have reasons to believe in an actual evaluator's epistemic virtuousness: open-minded, intellectually courageous, tempered with intellectual sobriety and humility, faithful, integer, disinterested, honest, and impartial (Patternotte & Ivanova, 2017, pp.1791). This requirement adds "a further element to the objectivity of the decision-making process" (Sankey, 2020, p.17). Those epistemic virtues ensure—of course, fallibly, i.e. with no guarantee—that the cognitive value judgements entering the cognitive utility estimate "are rigorously and correctly applied" (ibid.): that the evaluator "whose judgement is appropriately guided by the epistemic virtues is one whose deliberations are honestly and conscientiously conducted. Their judgement is based solely on appropriate considerations of an epistemically relevant kind rather than being subject to the influence of personal interest, political ideology, or other forms of bias" (ibid.).

Both requirements are non-trivial; not every scientist satisfies them. Fortunately, these requirements *can* plausibly be satisfied (or at least satisficed). They in fact reflect scientific practice: they are sought, and—if everything goes well—satisfactorily realised in the selection of expert referees for funding agencies, hiring committees, book proposals, etc.

IV.4 Cognitive utility estimate

Having clarified the notions of costs and benefits, and the requirements on a judicious evaluator, let's finally address the virtue-economic account's utility estimate. To appraise the overall pursuit-worthiness, an evaluator must exercise her judgement to weigh the costs and benefits—a matter of skill- and reason-based deliberation (cf. Brown, 2017): to the best of her knowledge and abilities, she arbitrates which of the ideas under consideration strikes the best balance between potential cognitive benefits and costs.

How exactly is she supposed to do that? With too much laissez-fair, two objections immediately loom. The first is Laudan's remonstrance about "radical individualism", as he scathes it in Kuhn (1977): "every scientist has his own set of reasons for theory preferences and thus that there is no real consensus whatever with respect to the grounds for theory preference" (Laudan, 1996, p.89). Theory appraisal thereby degenerates, Laudan educes, into an a-rational—arbitrary and subjective—affair: "[Kuhn's] view entails, among other things, that it is a category mistake to ask (say) why physicists think Einstein's theories are better than Newton's; for, on Kuhn's view, there must be as many different answers to that question as there are physicists" (op.cit., p.90). A second challenge targets proposals for theory choice on the basis of theory virtues more generally: Kuhn's list of salient theory virtues "is assembled ad hoc. One might easily add further criteria or delete others." (Carrier, 2008, p.284). Virtue-based proposals are obliged to "(identify) [features of excellence, i.e. theory virtues] from a unified point of view. It gives a systematic and coherent account of methodological distinction and thus provides a rationale as to why these features and not others are to be preferred" (ibid.).

To alleviate both concerns, we demand that judgements be obtained through a process of reflective equilibrium (Baumberger & Brun, 2021; Beisbart & Brun, 2024). Rather than relying on spontaneous, or dogmatically clung to, intuitions—let alone capricious ad-hockery—an evaluator should plump for a circumspectly meditated ranking of theory virtues: for appraising pursuit-worthiness, she must employ a *systematic* preference structure amongst virtues, an ordinal ranking. It should respect two constraints.

The first insists on coherence with other (historical and/or present-day) cases. The preference structure ought to have broad scope with respect to paradigmatically pursuit-worthy cases; the ranking amongst virtues should be consonant with judgements of *other* hypotheses or models whose pursuit-worthiness the evaluator deems exemplary (see also Carrier, 1986; cf. Sankey's 2018 characterisation of Chisholmian particularism). Their pursuit-worthiness should be preserved when applying the preference structure to those "touchstone episodes" (Sankey) (for reasonable, in particularly historically sensitive, assessments of pertinent background knowledge). This "testing procedure" is especially useful when the exemplars were confronted with rivals; this allows a direct matching of preference structure for the virtues in question.

A second clause complements attention to other ideas in science: we need some kind of assurance of an evaluator's *philosophical/meta-theoretical* conscientiousness. Her commitments that are supposed to be in equilibrium not only encompass paradigmatic scientific instances of pursuit-worthiness; they also involve reflection on science—especially regarding the cognitive-epistemic scientific achievements of those paradigmatic exemplars, and the more general significance of key virtues (e.g. predictive novelty). In other words, the evaluator's judgements, with their preference structure amongst virtues, must not only cohere with her background knowledge and exemplars of scientific pursuit-worthiness. They must also cohere with her wider philosophical/meta-theoretical outlook. The latter in turn also likely influences her choice of exemplars.

The two requirements defuse Laudan's and Carrier's concerns. The first imposes a substantial constraint—in itself, as well as intersubjectively. The list of bona fide pursuit-worthy ideas most evaluators would deem exemplary will, we believe, be manageable and fairly uncontroversial (including e.g. Bohr's model of the atom, the modern evolutionary synthesis, special relativity, particle Dark Matter, etc.). We take this near-unanimity to be a sociological-historical fact. Accommodating such paradigmatic cases as "test instances" induces systematicity amongst virtue rankings, as Carrier enjoins. The requirement of philosophical conscientiousness—or rather: the observation that any evaluator is perforce also steeped in philosophical commitments, of which we demand reflectively equilibrated harmony—further reigns in the latitude for arbitrariness, at least in most cases. We herein don't deny the occasional luminary with outré philosophical ideas (say, Dirac about mathematical beauty, see e.g. Ivanova, 2017). Nor do we brandish such scholars as necessarily deluded or irrational. Rather, our point is that most evaluators won't share these scholars' background beliefs. Therefore, such commitments will, in the main, drop out of the systematising effect of the reflective equilibrium process that their judgements should exhibit. Our goal isn't to rule out rational disagreement—

to the contrary; rather, it's to dispel fears about "*radical* individualism", impervious to rational scrutiny.

The two constraints vouchsafe a sense of rationality, apposite to deliberative judgements in science and philosophy of science (see also Elgin, 1996, 2012, 2017, 2018). To preempt misunderstandings of such deliberative rationality, let's spell out two implications. First, the reliance on judgements isn't an "algorithmic decision procedure" (Kuhn, 1977, p.439), "able to dictate rational, unanimous choice" (ibid.). Judgements don't obey fixed—hard-and-fast and context-independent—rules of a "rational calculus" (as envisaged by Laudan, 1977. p.162). Such variability is, of course, typical of rationality inherent in the exchange and assessment of arguments and reasons (see Rescher, 1988, 1993; Elgin, 2022). Nonetheless, this doesn't imply that it's arbitrary or random—let alone a-rational or even irrational. The evaluator's judgements must be responsive to reasons: her utility estimate is supposed to be the result of careful, context-sensitive deliberation (see also McMullin, 1982).

The judgements should also incorporate another aspect of pursuit-worthiness considerations to which the economic framework drew attention: the *likelihood of succes*s upon pursuing an idea. On the one hand, construing likelihood as *probability* sensu stricto that an idea turns out to be true or successful", one runs into quandaries. Does such a probability make sense? It's not obvious that a meaningful measure could always be objectively defined. Even if it is, how exactly do we acquire knowledge of it? Our account is compatible with, say, Bayesian assessments of likelihood, (see Nyrup, 2017, Ch. 2 & 3 for details), but doesn't require them. For deliberative rationality, it suffices if more qualitative judgements of likelihood enter an idea's cognitive utility estimate.[19] Such considerations of likelihood need be no more—but also shouldn't be less—scrupulous and thorough than what is customarily expected (and realised), whenever scientists and funding panels judiciously decide on which projects to choose.

A second implication of our reliance on deliberative rationality is that reasons underwriting a cognitive utility estimate generically allow for *rational disagreement* (as indicated in the above-quoted passage by Kuhn, and Sankey, 2020, p.18). Nothing per se mandates that different evaluators arrive at the *same* outcome: within the bounds of deliberative rationality, it's possible—and, in fact, not rare—for judgements to diverge (see also Elgin, 1996, 2010,

[19] Illustrations of such considerations of likely gains and costs, at a sophisticated level, are given by so-called "No-Lose Theorems" (Fischer, 2024c). These are arguments to the effect that scientific efforts will provide substantial epistemic gain, irrespective of their scientific outcomes. For instance, since the 1980s up to the discovery of the Higgs boson in 2012 collision experiments at the LHC were taken to yield substantial epistemic gain even if they should exclude the Higgs boson.

2018).[20] Such permissiveness seems appropriate for the pluralism-friendly context of pursuit (see also **§VI.3**).[21]

V Demarcation from Kuhn

Kuhn's account of pursuit-worthiness differs from ours in three respects. In each, the virtue-economic account has a distinct advantage.

First, Kuhn doesn't clearly distinguish between pursuit and acceptance. Kuhn's notion of a paradigm welds aspects of both (Šešelja & Straßer, 2013). During normal science, the commitment to a paradigm is marked by belief in its adequacy (its truth or status as the best explanatory description, i.e. superior problem-solving power). At the same time, paradigms circumscribe the framework for *new* problems: normal-scientific pursuit is governed by imitation of the original paradigm ("exemplar") and its elaboration and further development ("disciplinary matrix"). Where revolutionary science stands with respect to pursuit/acceptance is more elusive to classify. During revolutions problem-solving and evidential considerations are said to give way to "faith that the new paradigm will succeed with many large problems that confront it, knowing only that the older paradigm has failed with a few" (1996, p.158), to the paradigm's "future promise" "that a few scientists feel" (ibid.). According to Kuhn, "(a) decision of that kind [viz. "which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely", p.157, our insertion] can only be made on faith" (p.158). It's unclear whether evaluations of acceptance are merely (temporarily) *suspended*, or—as the repeated religious metaphor, with its doxastic and practical connotations, indeed suggests—even *subordinated*.

Kuhn's insufficient differentiation between pursuit and acceptance isn't merely lamentable imprecision (and has arguably led to misunderstandings with Feyerabend, see Shaw & Barseghyan, 2017, esp. sect. 4.1). It also makes it difficult to assess the normative adequacy of Kuhn's evaluative stances during the two phases that he postulates. In particular, the *rationality* of scientific revolutions has been a notorious bone of contention (cf. Lakatos, 1989, p.91). By contrast, the distinction between pursuit/acceptance is explicitly built into the virtue-economic account ab initio. Furthermore, as we'll argue in greater detail (**§VI.3**), our account licences pluralistic pursuit also of non-mainstream ideas.

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[20] One shouldn't contrariwise overestimate the *actual* extent of disagreement (a point that Kuhn, with his assertion of paradigm *monopoly*, arguably overblew). By *all* reasonable standards, for instance, Newtonian celestial mechanics in the 18th and 19th century outperformed any rivals (Smith, 2014).

[21] Straßer et al. (2015) rightly stress that epistemic tolerance is the appropriate cognitive attitude vis-à-vis typical scientific disagreement amongst peers. The reason, from our perspective, is precisely that deliberative rationality allows for *rational* disagreement.

Secondly, Kuhn's criteria for pursuit-worthiness depend on the mode/phase of inquiry. During normal science, research is marshalled by the prevailing paradigm: paradigms set the research agenda, the kinds of problems that must be solved, with the appropriate methods (mathematical, modelling, etc.), together with methodological and meta-theoretical constraints. An idea's pursuit-worthiness in normal science is thus determined by two key factors. The first is a more conservative moment: coherence with the established background knowledge and aspects of the ruling paradigm. The second, related, and arguably more fundamental factor is *similarity* with exemplary works that embody the paradigm's matrix (see Bird, 2001), "one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice" (Kuhn, 1962, p.10). The more an idea resembles such past scientific achievements, the more pursuit-worthy it is: imitability of those exemplary achievements, for Kuhn, grounds reasons for further investigation during normal science. The upheavals of revolutionary science require different criteria for pursuit-worthiness for here the paradigms themselves undergo radical change. (What precisely they are Kuhn only vaguely gestures at.)

The virtue-economic economic account partially subsumes Kuhn's criteria for pursuit during normal science: conservatism and coherence with established knowledge are indices of pursuit-worthiness that the account recognises. But the latter allows for a wider spectrum of virtues; by no means is it wedded to such conservatism. The virtue-economic account thus offers a more *unified* set of standards than Kuhn (and doesn't rely on Kuhn's questionable (cf. Feyerabend, 1970) two-phase distinction): cognitive virtues remain the (context-dependent and reasons-sensitive) indices of pursuit-worthiness, throughout. As stressed, across time and evaluators, assessments of those indices' instantiation (and weighted aggregation) may vary. This view comes close to Kuhn's later views—leading us to the third difference.

Finally, as we saw with Kuhn's invocation of "faith" and "conversion" (p.158) (also: "transfer of allegiance", p.151, during revolutions), in *Structure*, Kuhn is groping for an articulation of those criteria (and, a fortiori, their rationality). The difficulty, Kuhn (1962, p.156) notes, lies in the fact that "that decision [between an old and a new paradigm] must be based less on past achievements than on future promise. The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving". Regrettably, Kuhn doesn't further elaborate this sense of promise or potential on which such pursuit-worthiness pivots (see also Haufe, 2024). The cageyness is doubly unfortunate since also the pursuit-worthiness during normal science seems to emanate from this source: "(n)ormal science consists in the actualization of that promise […]" (op.cit., p.24).

As a solution, Kuhn eventually (1996, *Postscript*; 1977; 1993, p.338) settles on value-judgements: theory virtues (empirical adequacy, consistency (internal/external), simplicity, scope, and fruitfulness) provide evaluative standards for theory choice, universally shared by scientists. According to Kuhn, they function as *trans*-paradigmatic criteria for *both* pursuing and accepting research paradigms. More specifically, he regards them as constitutive of scientific rationality: they define what it means for scientists to act rationally in theory choice; not orienting theory choice on their basis, one ceases to play the game of science.

The virtue-economic account concurs with—and is overtly indebted to—Kuhn, as far as the importance of theory virtues is concerned: as indices of pursuit-worthiness. We underline, however, three differences. First, in line with our paper's focus on the context of pursuit, we refrain from any claims about criteria for theory acceptance—the link to which is crucial for Kuhn (1977, p.322). Secondly, for Kuhn theory choice on the basis of theory virtues delimits the very rules of scientific rationality: to practise science is to adhere to those rules; they are constitutive for science. We refrain from a strong claim. All our account needs—and hopes to purvey—is a suitable strategy for optimising the attainment of science's cognitive goals (see also below, **§VI.1** for our account's connection to normativity). A third difference vis-à-vis Kuhn concerns the nature of the value judgements, underlying theory choice: for Kuhn, they boil down to irreducibly subjective preferences. In the final analysis, as Kuhn's critics were quick to castigate, the sense in which such judgements still count as rational is opaque. By contrast, as elucidated in **§IV.4**, our account remedies this defect through rationality constraints imposed on the deliberative process (absent in Kuhn).

VI Merits of the Virtue-Economic Account

Here, we'll expound the core merits of the virtue-economic account: a clear source of normativity (**§VI.1**), its middle path between flexibility and substantive prescriptive content (**§VI.2**), and its both complementary and supportive link to epistemic pluralism (**§VI.3**).

VI.1 Issues of normativity

The virtue-economic account pronounces those ideas pursuit-worthy that are judged to strike the best balance between cognitive costs and benefits. Let's zoom in on the source of its normative force: what grounds the account's normativity? We submit that it flows directly from considerations of commonsense means/end considerations. It's a garden-variety instrumental rationality (encoded in the economic, standard decision-theoretic framework) that undergirds the virtue-economic account's normative maxim: if one covets scientifically valuable theories, one should pursue those ideas, provided that (i) fallible and tentative indications exist that they'll lead to (either directly or indirectly) scientifically valuable theories or insights, and (ii) the pursuit comes at reasonable cognitive costs.

Our account needn't invoke a particularly controversial source of normativity—nor of rationality (cf. Šešelja et al., 2012). Fairly run-of-the-mill means/ends considerations govern the overarching strategy to pursue projects which are judged to optimise cognitive utilities; those judgements in turn involve likewise fairly standard constraints on rational deliberations, as they routinely figure in jurisprudence, philosophy, historiography, or sociology. A side glance to two other prominent views illustrates that one can't take such an advantage for granted. First, Laudan (1977) *defines* scientific rationality in terms of progress: "he takes rationality to be derivative, instead of the primary element it has usually been assumed to be" (McMullin, 1979, p.623). We dispense with such an assumption. Furthermore, whether rationally warranted pursuit *eventually* results in scientific progress, rather than a dead end, is a distinct question. We are well-advised to also keep the questions separate—not least because the very notion of scientific progress (and whether we ought to define it following Laudan) is the subject of on-going controversy (see, e.g., Shan, 2023; or Niinuluoto, 2024).

Secondly, consider Friedman's (2001, 2010, 2011) neo-Kantian account of the history of science. As such, it also purports to encompass the dynamics of scientific pursuit. A crucial element is a philosophical—including metaphysical and methodological—discourse at a meta-level. It's supposed to accompany scientific discussions (more narrowly construed); within Friedman's model, reasoning at this level brings about, and steers, the large-scale dynamics of science, major theory shifts ("paradigms"). In the final analysis, it grounds the rationality of science: the "dynamics of reason", for Friedman, is the motor for the dynamics of science. Explicitly (2001, pp.53) distinguishing (and distancing) it from instrumental rationality, Friedman identifies this rationality as communicative/discursive rationality in the sense of Habermas (1981). Friedman's reliance on such communicative rationality is externalist (cf. Dimitrakos, 2017, 2023, fn.9): it seeks to explain theory change by means, often portrayed as external to science (see e.g. Arabatzis, 1994).

We sympathise with Friedman's emphasis on communicative rationality in science through philosophical deliberation. We reject, however, his externalism: rational discourse and deliberation regarding theory choice can't be meaningfully *severed* from science. Rather than anything extraneous to science, on our account, deliberative elements are *integral to* science. By the same token, we baulk at Friedman's opposition between instrumental and discursive rationality: in science and scientific reasoning, both are inextricably entangled. More generally, we contest a clear-cut distinction between philosophy and science (see e.g. Buchdahl, 1970 for a historical illustration, and Ellis, 2006 for a contemporary one). Accordingly, we rebuff their segregation into different levels, and a fortiori the hierarchy of cognitive authority underlying Friedman's account, the idea of a sovereign realm of reasons at a distinct, higher level that is supposed to guide science through the darkness of history.

VI.2 Via media between flexibility and stringency

A second key merit of the virtue-economic account is that it strikes the right balance between flexibility and permissiveness on the one hand, and stringency and specificity, on the other. As a corollary, the account gets extra mileage in terms of fertility for historical analyses.

Meta-methodologically, we deem it vital that *any* methodological view be sufficiently flexible and permissive to do justice to the complexity and variability of actual science. Pluralism and disagreement are enduring realities in science, past and present (e.g. Chang, 2010, 2012; Lopez-Corredoira & Marmet, 2022; Ćirovič & Perović, 2024 for historical examples). A realistic methodological proposal thus shouldn't shrug off lightly the plurality of expert opinions, nor of ideas seriously explored; instead, it ought to make sense of it. With pluralism having been defended on independent grounds also normatively (a topic we'll return to in **§VI.3**), *rational* disagreement must be allowed for. Through its reliance on deliberative rationality as exercised in the formation of judgements, this desideratum is built into the virtue-economic account from the get-go.

If flexibility and permissiveness are desirable, so is specificity: methodological criteria should be sharp enough to recommend or condemn *something*. The rationality constraints that our account imposes on the deliberative process safeguard this. The twofold reflective equilibrium (with respect to both scientific and philosophical commitments) through which an evaluator forms her judgements, and the resulting coherence of reasoning, are demanding (see e.g. Currie, 2017; Currie & Sterelny, 2017; and Elgin, 1996, 2005).

Some situations in the history of science our account (on most plausible rankings of virtues) fairly unequivocally certifies pursuit-worthiness: (at least) in certain phases, cosmic inflation (Wolf & Duerr, 2024), Supersymmetry (Fischer, 2024a), Continental Drift (Šešelja & Weber, 2012), Special Relativity (Janssen, 2002). One may view such episodes as "test cases" for descriptive adequacy. They allow us to check whether the virtue-economic account's recommendation for pursuit chimes with (putatively) paradigmatic historical examples (cf. Schindler, 2018, Ch.7). Two scenarios typify such (ideal) "testing" cases:

1. Vis-à-vis alternative ideas, the cognitive benefits of idea A are largely estimated high, with cognitive costs estimated largely low (with no intervening material, or societal/moral obstacles).

2. Conversely, the cognitive benefits of an idea B are largely deemed low (or at most, equal those of competitor ideas), and cognitive costs are largely deemed high.

If descriptively adequate, our account yields straightforward predictions. For (1), it clearly judges A to be pursuit-worthy; one should therefore expect its large-scale pursuit through the scientific community (assuming the latter's by-and-large rationality). For (2), the virtue-economic account dismisses B's pursuit-worthiness: if the account is descriptively accurate, one therefore shouldn't expect large-scale pursuit of B through the scientific community. Especially poignant instances where the account intuitively passes these two—of course, stylised—"tests" occur when "orthodox"/mainstream and "heterodox" ideas compete. For example, various alternative theories of gravity (say, rivals to General Relativity, such as Massive Gravity or so-called gauge theories of gravity, or also rivals to the Dark Matter (*cum* General Relativity) hypothesis, such as TeVeS, see Clifton et al., 2012), for the most part, necessitate an inordinate level of mathematical complication to even achieve empirical adequacy. Hence, it comes as no surprise that they are only pursued as minority research programmes—in concurrence with the virtue-economic account's prediction.

In fact, and more boldly, we believe that our account has the potential for incisive critical bite: it can directly contradict prevailing opinions. Two examples spring to mind. One is Pitts' (2011, 2016) plea for the superior pursuit-worthiness of an alternative to General Relativity until the late 1910s on the basis of simplicity and conservatism. The other is Bell's (2004) plea for the pursuit-worthiness of alternatives to standard quantum mechanics (see also Cushing, 1994). (While not explicitly couched in terms of our virtue-economic analysis, it's straightforward to read the arguments in those analyses as such.)

One appealing consequence of our account's balance between flexibility and specificity is its fertility for historiographical practice: the account delimits a concrete, rich and versatile evaluative agenda for assessing historical questions of pursuit-worthiness (as it were "ex-post", rather than "ex-ante", Fischer, 2024a). The account's normative tenets afford epistemological standards of rationality against which historical agents' pursuit becomes intelligible (and/or assessable): a cognitive utility estimate (with historical actors' background knowledge and assumptions) explicates the rationality (or its failure) for the episodes in question. By dint of it, we can craft coherent narratives that spotlight reasons and commonsensical, decision-theoretic standards of rationality (cf. Currie & Sterelny, 2017; Currie, 2023). An investigation of historical episodes in terms of virtues thus confers understanding of them as episodes in the history of *science*, a paradigmatically rational enterprise, through properly historicised "internal history" (Nanay, 2010, 2017; Arabatzis, 2017; Dimitrakos, 2021).

Examples of fruitful ex-post virtue-economic reasoning include Kuhn's (1957) analysis of the rivalry between Copernican and Ptolemaic astronomy, or Chang's (2012ab) analysis of the Chemical Revolution. While, for obvious reasons, not explicitly framed in terms of the virtue-economic account, their accounts can be naturally read as applying its principles.

VI.3 Affinity with pluralism

Our virtue-economic account is both complementary and congenial to scientific pluralism. Pluralists advocate the proliferation of multiple lines of research in any given field (see Laudan, 1980; Chang, 2012, 2021). Rather than "an idle pronouncement to 'let a hundred flowers bloom'", pluralism emboldens the "effort of *actively* cultivating the other 99 flowers" (op.cit., p.260).

It's often demurred that scientific pluralism ducks a critical practical problem: "it may sound fine to cultivate a hundred flowers, but how do you keep the weeds out?" (op.cit., p.262). Chang counters that "pluralism is a doctrine about how many places we should have at the table; it cannot be expected to answer a wholly different question, which is about the guest list" (ibid.). The virtue-economic approach of pursuit worthiness is therefore best seen as a naturally *complementing* Chang's pluralism: by deciding who makes it to that guest list.

It may do so either by agreeing upon a benchmark. To meet it earns an idea the invitation suite. Beyond that, entry isn't restricted; pluralism is confined, though, to ideas above the threshold. Alternatively, one may introduce a hierarchy of pursuit-worthy ideas into an overriding pluralism: pluralists would then prioritise several projects according to the rankings of pursuit-worthiness that an evaluator, on our account, would issue.

The virtue-economic account not only complements, but also *supports* pluralism. First, it expressly allows for rational disagreement (**§IV.4**): different evaluators whom we have reasons to regard as equally competent can reach different verdicts on an idea's pursuit-worthiness. It therefore behoves us to treat their verdicts equally seriously; pluralism naturally ensues—as the attitude of encouraging the further exploration of rationally warranted projects. Secondly, an intuitively compelling argument for pluralism stems from risk-spreading (e.g. op.cit., pp.270): we hedge our bets on research projects by not putting all the proverbial eggs in one basket, but instead pursuing several projects simultaneously. The underlying rationale is precisely that of the economic framework (**§III**).

VII Conclusion and outlook

We began our paper's project by presenting the economic model as a meta-methodological framework for evaluating considerations of pursuit-worthiness. It urges a set of questions for more concrete accounts: what are relevant benefits and costs? Who evaluates benefits and costs? How is an overarching utility estimate to be achieved? As a natural and appealing way of putting flesh on the bones of the economic framework, we developed our virtue-economic account of pursuit-worthiness. It cashes out benefits and costs in terms of theoretical virtues that are to be evaluated from the perspective of an ideal scientist. Utility estimates are a matter of deliberative judgments, rather than calculus. Nonetheless, they must conform to demanding rationality constraints.

The virtue-economic account comes with considerable idealisations. This is a natural restriction for (normative) philosophy of science. Undoubtedly, it would be worthwhile exploring whether the economic framework can be extended to less idealised evaluations of pursuit-worthiness, as they figure in down-to-Earth, actual decision-making In particular one may wish to include not only purely epistemic benefits (such as technological spin-offs), and more material costs (such as funds for experimental equipment, or lab management).

Such considerations will inevitably complicate cost-benefit analysis in several regards. For example, costs will have to be assessed from different perspectives. Resources at the individual level encompass scientific abilities and talents, prior experience and training or background, available equipment and facilities (including, for instance, computation or observing time) and time available for research tout court. The availability of such resources varies: one scientist may have different infrastructure available than another. Moreover, additional questions arise regarding the utility estimate: how are material costs to be traded off against epistemic benefits, specifically benefits far from concrete applications (in foundational research)? We believe that the economic framework can be a useful tool for approaching such questions in future work.

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