Cosmic conundrums, common origins, and omnivorous constraints

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Abstract:

The paper revisits Janssen's seminal proposal of Common Origin Inferences (COIs), a powerful and scientifically fruitful inference pattern that (causally) traces striking coincidences back to a common origin. According to Janssen, COIs are a decisive engine for rational theory change across disciplines and eras. After a careful reconstruction of Janssen's central tenets, we critically assess them, highlighting three key shortcomings: its strong realist and ontological commitments, its restriction to (or strong penchant for) causal/ontic explanations, and its intended employment for conferring evidential-epistemic status. To remedy these shortcomings, we moot a natural generalisation and amelioration of Janssen's original conception—COI*s: Constraint-Omnivorous Inferences. COI*s warrant inference to pursuit-worthy hypotheses: it's rational to further study, work on, elaborate/refine or test hypotheses that account for multiple constraints in one fell swoop. As a demonstration of the utility of COI*-reasoning, we finally show how it sheds light on, and dovetails, the three most significant breakthroughs in recent cosmology: the Dark Matter hypothesis, the Dark Energy postulate, and the theory of cosmic inflation.

Key words: *Common Origins, IBE, pursuit, heuristic reasoning, theory choice, cosmology*

I. Introduction

If a renowned historian and philosopher of science opines that "(s)cientists such as Copernicus, Kepler, Newton, Darwin and Einstein all availed themselves of a pattern of reasoning" (Janssen, 2002, p.458), philosophers of science are bound to prick up their ears. They will want to learn more about this pattern. And provided that the historian's case sways them[1], they'll be eager to extract methodological lessons from this (perhaps to that scholar's chagrin, p.512).

We are lucky to find ourselves in that situation. Janssen (2002) "(draws) attention to one specific way in which scientists in disparate fields, periods, and locales have used the explanatory power of their ideas as evidence" (p.458): "common origin inferences" (COIs), as he calls them, denote an omnipresent inference pattern that "traces some striking coincidence back to a common origin (typically some causal structure or mechanism)" (ibid.). A "powerful engine for theory change" (p.470), and a promising competitor to Kuhn's (1996) anomaly-driven model, it provides "some of the strongest evidence ever produced by science on how to cut nature at the joints" (p.465). The examples that Janssen investigates in some detail couldn't have been picked more aptly to further pique our curiosity: the heliocentric revolution, special relativity's Minkowksian spacetime structure, plate tectonics, the primordial universe, Darwinian

[1] The bar for this proviso will, for some, doubtless be high (see e.g. Barseghyan, 2015; McAllister, 2018; Dimitrakos, 2020; cf. however Schindler, 2018, Ch. 7 for a more sanguine approach, to which we're sympathetic).

evolution, the equality of inertial and gravitational mass on Einstein's path to general relativity, the extinction of the dinosaurs, and an intriguing case from recent art history.

Janssen's claims only become more intriguing, when he adverts to earlier and vocal advocates of COI reasoning (or close cognates thereof): inter alios, Newton (p.464), Whewell (pp.486), or Darwin (pp.494).

On the one hand, according to Janssen, "*COI*s are exceedingly common in science as well as in everyday life" (p.464). On the other hand, Janssen is careful to note that "COIs only capture *one* aspect of scientific methodology" (p.458). Interestingly, however, eminent authors have championed the stronger thesis of COIs as, at bottom, *the* method of the so-called historical sciences (e.g. the geosciences, paleontology or archeology): Cleland (2013, p.7, see also 2011; Tucker, 2011, p.20), for instance, asserts that "(t)he dominant form of explanation in the historical natural sciences is common cause explanation. The basic idea is to attribute a puzzling collection of traces to a common cause". Such claims—whatever our ultimate verdict on them (see Currie, 2018, esp. Ch.6&12 for a persuasive counter)—heighten the relevance of Janssen's proposal.[2]

In sum, we certainly agree that, as a methodological precept, "what was good enough for Darwin [...], should be good enough for us" (Janssen, 2002, p.512): COIs represent "an important pattern of scientific reasoning that [...] deserves further attention from both historians and philosophers of science" (p.513). This we shall undertake in the present paper—primarily from the latter vantage point.

The **plan** of the paper is as follows. **§II** will review, and give a concise formulation of what we take to be the gist of, Janssen's original COI account. In **§III**, we'll critically examine it. Our reflections will allow us to clarify some incongruities, and conversely, pinpoint the essential insights. Based on that analysis, we'll then (**§IV**) articulate our proposed modification of Janssen's account, our COI* account. It suitably generalises the former, and overcomes its key defects. **§V** will illustrate COI and COI* reasoning in three case studies from contemporary cosmology, further strengthening Janssen's diagnosed prevalence of this inference pattern.

II. Revisiting COIs: Janssen's account

[2] Regrettably, to the best of our knowledge, neither the authors participating in that debate over the methods of the historical sciences, nor Janssen engage with each other's work.

This section will briefly recapitulate Janssen's (2002) original account of Common Origin Inference (COI). He defines them as inference patterns for hypotheses that explain multiple phenomena as having a common origin (a "COI story" in Janssen's appellation).

More precisely, in a COI, given empirical phenomena a, b, c, etc., and X—"a statement, a model or an idea, no matter how well or how poorly articulated" (p.464)—one "traces a number of otherwise puzzling coincidences" a, b, c, … "to a common origin" (ibid.), as purported by X. A COI, in other words, is an ampliative/inductive (i.e. non-deductive, p.458, fn.1) inference rule of the form:

> "If it were the case that X, then that would explain observations/phenomena a, b, c, …" → "It is, in fact, the case that X" (p.464).

Complementing this inference rule, Janssen's account of COIs and COI-based reasoning encompasses four further tenets.

> **(CAUSAL EXPLANATION)** COIs, at least provisionally, posit some causal structure or mechanism as their central explanantia. COI stories provide causal explanations: they explain by exhibiting how the phenomena fit into a larger causal nexus. Typically, and for "the most interesting cases" (p.467) COI stories postulate common causes.
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> **(SUPREME EXPLANATORY POWER)** The explanatory power of COIs is so formidable that it counts as compelling evidence. In fact, "the main *COI*s examined in (Janssen's) paper have provided some of the strongest evidence ever produced by science on how to cut nature at the joints (p.465).
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> **(WEAK REALISM)** COIs "provide exceptionally strong warrant for the conclusion that the phenomena they tie together are due to the same structure or mechanism" (ibid.; also p.512), circumscribing "phenomena kinds" (p.465), i.e. natural groupings.
>
> COIs *don't* warrant, however, realist commitments to any specific details of their explanatorily operant structure/mechanism, nor to the "ontological status of theoretical entities" (p.468) more generally.
>
> **(COMMITMENT)** COIs exact a twofold commitment towards them.[3] The first is a corollary of (WEAK REALISM): they establish firm grounds (sometimes even "beyond the shadow of a doubt", p.492, 512) for *believing* a COI story's distinctive causal structure or mechanism to correspond to something real. In addition to that, COIs also commit us to *prospective* "forward-engagement" (p.476) as gripping "promissory (notes)" (ibid.) for further work on them: they instil rationally warranted trust in their

[3] Interestingly, we find this *double*-commitment also in Kuhn (Šešelja & Straßer, 2013) (as well as in van Fraassen, 1980, pp.210, and elaborated in his 1984). It's also prevalent in many scientists' philosophical musings on science (e.g. Peebles, 2024, as well as Turner, whom Janssen cites). As one of our main corrections of Janssen's account, **§III.3** will argue that, first and foremost, the *prospective* dimension of this commitment, the one related to promise and trust-worthy potential, is what COIs licence.

> implementation into future research as “important constraints” (p.465), and in their fruitful further elaboration.

Indubitably, Janssen has made a stimulating proposal. He touts COIs as “non-trivial elements of scientific methodology that are common to many traditions across disciplines, locales, and periods” (p.458). “(Ubiquitously used) in scientific practice, past and present” (ibid.), they suggest an “alternative to the Kuhnian mechanism of theory change” (Janssen, 2002, p.458)—an alternative that, Janssen contends, vouchsafes “an element of rationality” (p.513, 501).

The wonderful richness of Janssen’s—avowedly “programmatic” (p.469)—paper isn’t without some lacunae, and incongruities, as we’ll see below. Our objective for the remainder of this paper is to systematically evaluate some of his philosophically juiciest claims. The analytic paraphernalia of recent philosophy of science will prove handy in sharpening the profile of Janssen’s account, and refining it, with occasional, friendly rectifications.

III. Analysis: re-examining COIs, and the case for COI*s

Having reviewed the gist of Janssen’s COI account, this section will critically examine it. We’ll suggest some improvements, encapsulated in our subsequently mooted *COI** account. **§III.1** will inspect facets of explanation, germane to COIs. **§III.2** will zoom in on the kinds of inferences COI reasoning licences.

III.1 Causal explanations vs. accounting for constraints

Here, we’ll push back against Janssen’s focus on common *causes* (i.e. the view that COIs causally explain several phenomena), as per (CAUSAL EXPLANATIONS) (**§II**). We’ll argue for relaxing this condition in favour of more permissively construed common origins that *account for multiple constraints*.

Janssen (2002, p.513) primarily envisions COI stories as *causal explanations* for multiple phenomena. “(M)ost interesting COIs will be CCIs, Common-Cause Inferences“ (op.cit., p.467). In fact, common causes shape Janssen’s hunches on *how* COIs explain. Janssen states upfront his subscription to Salmon’s (1984, p.276) model of explanations. According to it, “we explain events by showing how they fit into the causal nexus”. “Salmon’s so-called ‘ontic conception of explanation’ provides a natural framework for the analysis of *COI*s and *CCI*s: ‘The ontic conception sees explanations as exhibitions of the ways in which what is to be explained fits into natural patterns or regularities. This view . . . usually takes the patterns and regularities to be causal' [...]. This seems to be an accurate characterization of common-origin explanations" (Janssen, 2002, p. 467). "In scientific practice", Janssen continues (p.468), "[causal structures or networks] (are) much more common" than "causally efficacious events of substances". Despite not categorically dismissing other accounts of explanation, Janssen hews to a causal construal of "networks, structures, or mechanism"—along Salmon's terms (rather than, say, "the unification account of Kitcher and Friedman", p.513).

Tying COIs to causal explanations strikes us as problematic—gratuitously so. Three objections militate against it. First, *at the heart* of Salmon's account lies the idea of causal processes (Salmon, 1984, Ch.5&6; 1985, pp.297): they continuously propagate, or transmit, a mark. Salmon's proposal suffers from numerous intrinsic difficulties, well-rehearsed in the literature (see e.g. Kitcher, 1989; Dowe, 2007; Hüttemann, 2018, Ch.7). (They led Salmon himself to abandon his model!) Hence, it would seem bizarre to foreground the salient achievement of COIs, as "an important pattern of scientific reasoning that is used in research traditions across disciplines, locales, and periods" (Janssen, 2002, p.515) in terms of a grossly inadequate model of causality.[4]

Secondly, and more specifically, some of Janssen's own paradigmatic examples of COI reasoning rub up against a characterisation in causal terms. In later works, Janssen (2002*, 2009) himself admits that in Special Relativity's COI story about, say, length contraction and time contraction "the sense of explanation I invoke is certainly not causal" (Janssen, 2009, p.49, ditto, 2002, p.501; see e.g. Dorato & Fellini, 2010 for further arguments).[5] It would likewise be somewhat misleading to depict Einstein's COI reasoning, in the genesis of General Relativity, regarding the equality of gravitational and inertial mass (Janssen, 2002, pp.507) as causal. Gravitational mass is explained away: it's reduced to inertial mass, rather than explained "causally".[6] The pertinent COI-reasoning is more perspicuously characterised as an *eliminative* explanation (Weatherall, 2011). In the same vein, as far as evolutionary explanations in biology are concerned,

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[4] Note in particular one of the challenges to Salmon's conception: its inherent physicalism (as Salmon, 1984, p.204 frankly admits): its applicability to disciplines other than physics seems doubtful (especially to Janssen's (2002, fn.23; p.460, fn.5) example of COI-reasoning in recent art history). This clearly contravenes the cross-disciplinary pervasiveness of COIs that Janssen otherwise stresses.

[5] Janssen's (2002, pp.497) explicitly discusses Einstein's COI reasoning in the form of a symmetry argument. Such arguments have in fact been adduced as plausible candidates for non-causal explanations in general (Saatsi & French, 2018).

[6] Analogously to how identity theorists seek to explain away and reduce mental states to physical states (see e.g. Beckermann, 2008, Ch. 8).

reservations about categorising them as causal explanations (Janssen, 2002, p.480) can point to long-standing—and by no means settled—debates (see, e.g., Sober, 1983).[7]

Thirdly, and finally, the demand for causal explanations stands in tension with Janssen's (2002, p.468, fn.21) agnosticism about causes (as also (WEAK REALISM) strongly suggests): "one cannot draw one any conclusions about the ontological status of theoretical entities" (ibid.) from COIs. This is "contrary to the spirit of the ontic approach—concerned, as it is, with the causal mechanisms that produce the facts-to-be-explained—to allow agnosticism of that sort" (Salmon, 1984, p.238).[8] "For the record", Janssen moreover professes commitment to "structural or Kantian realism" (2002, p.468, fn.21). We interpret this as an endorsement of *epistemic* structural realism (cf. Ladyman, 2023, esp. sect.3): knowledge can only be garnered about relations amongst entities—how they depend on each other; the entities and their intrinsic properties themselves forever elude us epistemically. We may believe the "structural content" of our best scientific descriptions: the relations in which (unobservable) objects stand.[9] Judgements about the latters' nature, however, ought to be suspended.

Suppose, then, that epistemic structural realism is indeed Janssen's overarching commitment. With its ontological reticence about entities and their nature, this mandates metaphysically deflating the sense of causality as it may figure in COIs. We'd be hard-pressed to discern how this wouldn't morph causal locutions into those of a regularity theory of sorts (see Ladyman et al., 2007, Ch.7 for an elaboration). Epistemic structural realism marshals us *away from* metaphysically strong conceptions of causality. Instead, it lures us towards ones in which

[7] It would even be hasty to parade the Copernican and Keplerian cases of celestial mechanics as clear-cut examples for causal explanations. Neither Copernicus nor Kepler obviously stated their COI reason in causal terms; arguably that would involve gravity as a key explanans. Even if we allow for the later description in terms of Newtonian gravity, the appropriateness of causal terminology isn't straightforward (owing to the action-at-a-distance nature of Newtonian Gravity, Salmon, 1984, pp.209, p.242)—the linchpin of e.g. Russell's and others' anti-causalism (as cited by Salmon, op.cit., p.136, see Hüttemann, 2018, Ch.2 for a broader historical review of anti-causal sentiments amongst 19th and early 20th century physicists). The general-relativistic perspective on gravity as a causal explanans is even more controversial (e.g. Curiel, 2000, Vasallo, 2019).

[8] Salmon (1984, p.238) is lucid about the requisite ontological status for causal explanantia: "(c)onsider, for example, the causal interactions involved in Brownian motion. According to Einstein's theoretical account of this phenomenon, the microscopic particle undergoes many collisions with the molecules of the fluid in which it is suspended. If there are no such things as molecules, then we have a radically inaccurate account of the mechanism. [...] For the ontic approach, any causal mechanism that is invoked for explanatory purposes must be taken to be real. If we are not prepared to assert its existence, we cannot attribute explanatory force either to that mechanism or to any theory that involves it. [...] The tooth fairy does not explain anything"

[9] Effective ontic structural realism, as outlined by Ladyman & Lorenzetti (2023), seems to be especially close to what Janssen has in mind.

structural relations, as enshrined in law-like regularities[10] (irrespective of any classification as causal), shoulder the explanatory work. In line with Janssen's urged primacy of scientific practice, this chimes with prevalent attitudes amongst scientists, with their customary reluctance to have research stymied by metaphysical strictures (see Scheibe, 2006, Ch.VI for a historical, and Norton 2003, 2008 for a sustained philosophical argument).

The cure to the preceding queries is to heed Janssen's (2002, p.458) own warnings against "putting episodes in the history of science on the Procrustean bed of one's preconceived philosophical categories"—and scrap the insistence on causal explanations tout court as what COI stories afford. What matters is that a COI story *account for* the explananda. No a priori restrictions should be imposed on the kind of explanation,[11] or the posits/explanantia accomplishing the explanation.[12] A COI story's explanantia may comprise:

- concrete-material common causes (as one would intuitively and conventionally classify them), such as the Chicxulub asteroid impact (Janssen, 2002, p.469, fn.23), or Dark Matter and Energy (see below, §V.1-§V.2));
- or common "underlying" processes, such as in natural selection (Janssen, 2002, pp.485) or plate tectonics (op.cit., p.469, fn.23);
- law-like claims/generalisations under which phenomena are subsumed as instantiations, as in the case of Copernican, Keplerian or Newtonian celestial mechanics (op.cit., pp.471), or Einstein's light quantum hypothesis[13] (cf. op.cit., p.512, fn.67); or
- more abstract, mathematical principles, such as the Pauli Principle (as a symmetry postulate for many-particle quantum systems) or Einstein's Relativity Principle.

Attenuating the reliance on causal explanations in COIs might elicit worries about *too much* leeway. It would threaten to run counter to Janssen's accentuation of their beguiling

[10] For concreteness, Bartelborth's (2007, Ch.VI) model of "nomic patterns" or Woodward's (2003) invariant generalisations may serve as plausible, and prima facie metaphysically sufficiently thin ways of spelling out the notion of regularity here.

[11] Permissiveness and liberalism as far as apriori/philosophical constraints on explanations are concerned seem prudent not only vis-à-vis Janssen's primacy of scientific practice (to which we are largely sympathetic, see also Ben-Menahem, 2018), but also vis-à-vis the proliferation of models of explanation and forms of explanatory reasoning (including non-causal explanations), flourishing in the philosophy of science literature.

[12] This liberalism dovetails Janssen's comment elsewhere (2002, p.458 fn.2): he seems to recognise the context-sensitivity of what counts as an explanation. Following van Fraassen (1980, Ch.5.2.8), he characterises explanations as answers to a 'why'-question in a given context of inquiry. This liberalism is also congenial to our subsequent extension of explanations to coherence relations.

[13] In the introduction of one of his annus mirabilis papers, Einstein (1905, p.368) writes: "(i)t seems to me that the observations associated with blackbody radiation, fluorescence, the production of cathode rays by ultraviolet light, and other related phenomena connected with the emission or transformation of light are more readily understood if one assumes that the energy of light is discontinuously distributed in space".

explanatory power. What is *minimally* required of a COI-based explanation—especially vis-à-vis (*SUPREME* EXPLANATORY POWER)?

Before providing an answer, let's drop yet another of Janssen's assumptions: that the explananda in a COI are "observations/phenomena" (Janssen, 2002, p. 464). Why limit ab initio the targets of COIs to *observational* facts? Reasons for quibbles with this are a little trite: two of the core tenets of 20th century philosophy of science, theory-ladenness and a blurry/non-absolute theoretical/observable distinction. Incorporating this received wisdom pushes us to a natural extension of what COIs can target: *any* fact in our background knowledge (be it of a more theoretical or a more empirical stripe). We'll henceforth refer to those targets as "constraints". This is intended to underline the constraining role of such facts for further theory development and model-building.

With this extension of targets in place, we can state our generalisation of COIs, COI*-reasoning: as an inference pattern to hypotheses that account for constraints. Paralleling Janssen's original definition (2002, p. 464), we stipulate that COI*-reasoning takes the form:

> "If it were the case that *X,* then that would account for the constraints *a,b,c, ….* " → adopt X.

What "adopting X" amounts to, will preoccupy us later on (§III.2). Here, our aim is a better grasp of what it means to account for constraints. Meshing with scientific practice, this isn't exhausted by explaining them sensu stricto (i.e. via logically entailing them, as in, e.g., in Salmon's or Kitcher's (1989) models of explanation). A hypothesis also accounts for constraints, when it *accommodates* them (see e.g. Barnes, 2022): when the constraints can, more or less coherently, be integrated into the wider web of scientific beliefs of which the hypothesis forms a part. Examples include cases of data accommodation through suitably adjusting free parameters or functions within a given theoretical framework (e.g. Newtonian mechanics), or taxonomic groupings (e.g. stellar classifications, see e.g. Ruphy, 2013). Integration constitutes a non-trivial cognitive-epistemic achievement. Scientists themselves expressly and routinely cherish it. For instance, despite otherwise voicing scepticism about it, Scott (2018, p.7; similarly, Ellis, 2018) praises the modern standard model of cosmology in such terms: "it requires just a few simplifying assumptions and seven free parameters to fit this huge amount of information–quite a remarkable achievement". Philosophically, such achievements are typically explicated in terms of coherence or understanding (Elgin, 1996, 2017, esp. Ch.3&4; Dellsén, 2020; Duerr & Dellsén, forth.): thanks to the systematisation they provide, we can "make sense of" things (and frequently venture further inquiry, as epitomised by, e.g. Mendeleev's periodic table, see Schindler, 2018, Ch.3.5).

*In COI*s*, the ability to trace back the satisfaction of multiple constraints to one origin X supersedes Janssen's emphasis on a common causal explanation of multiple phenomena: COI* reasoning infers from X's ability to account for a multitude of constraints, to the recommendation of adopting X. We'll dub this ability, a COI*'s "constraint-omnivory".[14] As the generalisation of Janssen's original Common Origins Inferences (COIs), we propound Constraint Omniverous Inferences (COI*s). The "surprising coincidences" that COIs explain become the surprisingly consilient suite of constraints that COI*s account for; COI*s tie these constraints together, "weaving" them into our web of beliefs (Elgin, forth.).

III.2 COI*s as guides to warranted pursuit

Our second amendment is to confine use of COIs to the context of *pursuit*; contra Janssen, we *don't* extend it to the context of acceptance. Furthermore, to imbue COIs with normative force as a methodological prescription, we demonstrate how to flesh out the pursuit-worthiness of COI- (and COI*-) based reasoning in two recent accounts of pursuit-worthiness.

Let's begin with a principal objection to Janssen's presentation of COIs. As an inference pattern, he regards COIs as a special case of IBE. This opens the gates to well-rehearsed challenges (e.g. van Fraassen, 1980, Ch. 2; 1989, Ch. 6.4; Cabrera, 2023). Of course, Janssen (2002, p.459) is fully aware of them. Bewilderingly, though, he declines to offer rebuttals. Instead, Janssen gestures at the weight of historical facts: "an important subsidiary goal of (his) paper is to show how strongly this view [scepticism about the inference to truth or knowledge] is at odds with scientific practice" (ibid.). It's difficult not to read this as a quasi-inductive argument for the efficacy of COIs qua IBEs, buttressed by the (alleged) strength of the historical record.

Were the case for COIs to rest on such historical induction, philosophers would rightly be disappointed. It wouldn't be unfair to decry it as fairly *naïve* enumerative induction (see also McAllister, 2018, sect. 4&5).[15] The inductive basis is manifestly meagre. No information about

[14] The term is modelled after Currie's (2015, 2017, 2018) notion of "methodological omnivory", the practice of historical scientists (paleobiologists, archaeologists, etc.) to "(utilise) multiple and disparate methods to generate evidence streams" by "opportunistically exploiting what resources are available" (Currie, 2015, p.198) or "(squeezing) all the empirical juice they can from whatever sources are available" (Currie, 2018, p.297).
[15] The quasi-inductive argument for COIs shares key difficulties with those of Laudan's "normative naturalism" (as discussed by Nola & Sankey, 2000, sect. 11).

a base rate is moreover provided. Even "(i)f one sees philosophy of science primarily as a descriptive enterprise" or "is interested primarily in elucidating scientific practice", "as (Janssen does)" (Janssen, 2002, p.463), the inductive case for COIs remains unpersuasive.[16]

In fact, Janssen doesn't wholesale renounce normative claims. Without qualms he speaks of "good explanations", "good evidence" (ibid.), a COI's "fruitfulness" (p.466), "legitimate COIs" (p.507), some COIs' "exceptionally strong warrant" (p. 465) or COIs as "an element of *rationality* in theory choice" (p.512, our emphasis). These passages feature *thick* concepts (i.e. notions with normative-epistemological dimensions, welded with descriptive ones). More generally, historians of science arguably can't dispense with all aspects of normative epistemology (Nanay, 2010, 2017; Dimitrakos, 2020). Such intimations, alongside the classification of COIs a subspecies of IBE (which is invariably discussed as a potentially *valid*—rather than a merely conventionally or habitually adopted—rule of inference) strongly suggest that COIs are supposed to be methodologically sound prescriptions: they should be endowed with (at least a modicum of) normative force.[17]

How to substantiate the normative force of COIs as a form of IBE—its methodologically prescriptive propriety—then? Janssen tries to sidestep the question (2002, p.471), thereby implicitly conceding that he lacks a satisfactory answer.

Apart from the precarious quasi-inductive basis for the reliability of COIs (cf. McAllister, 2018), another fundamental obstacle impedes an answer. As Ben-Menahem (1990, see also Norton, 2021, Ch. 8&9; Currie, 2018, esp. pp.145) stresses, we can trust IBE only insofar as we possess background knowledge about the domain to which we apply it. For an IBE to likely guide us towards true theories (or however we spell out "rational assertibility", appropriate for acceptance/belief), we must have prior experience with plausible standards of explanatory merit, "on the basis of broad empirical considerations: how crimes are usually committed, what seems to be the nature of physical interactions, what factors dominate intellectual development, etc." (Ben-Menahem, 1990, p.323). IBE, she insists, isn't a "formal inferential scheme" (i.e. a perhaps fallible, but nonetheless algorithm-like rule that can be applied mechanically). IBE is rather—in Norton's (2021) terminology—a "material" inference scheme

[16] To be sure, one may still regard it as an auspicious working hypothesis—a reading that Janssen's self-characterisation of his paper as "programmatic (p.469) invites, cf. McAllister, 2018, esp. sect. 2.

[17] Such exegetical issues aside, Janssen explicitly (p.471) considers it a natural follow-up question to what extent COIs "yield normative prescriptions for current scientific practice—or even criteria for funding decisions".

(i.e. one that depends on our knowledge of domain-specific, background facts). "Our standards for explanatory power cannot […] be separated from judgements of credibility" (Ben-Menahem, 1990, p.324).[18] Such background knowledge is typically absent. Thus "a rational application of IBE" (ibid.) to COIs as a universal scheme is blocked. Paradigmatic examples of COIs span motley areas. Claims about the methodological propriety of COIs would, on Ben-Menahem's line of thought, have to operate on quixotic levels of generality. But what would be the commonalities of (to take Janssen's examples) late Baroque paintings, celestial mechanics, bird's beaks on the Pacific Islands, and radioactive sediments from an asteroid impact 66 million years ago!), with which we are supposed to be familiar?

We short-circuit the notorious objections to IBE by simply restricting our use of COIs (McKaughan, 2008; Nyrup, 2015): rather than inferences from Common Origins to hypotheses which we regard as sufficiently well-supported to earn credibility, COIs ought to viewed as inferences to certain kinds of commitments or cognitive attitudes, *different from* those traditionally associated with warranted belief, or likelihood to be true.

We take our cue directly from Janssen: the kinds of commitments licenced by a COI, in his opinion, are "particularly strong in what I shall call the 'context of pursuit'" (2002, p.466). Let's first spell out this hint. In a second step, we'll revert to—and reject—Janssen's employment of COIs also beyond the context of pursuit.

Janssen's usage of the term—advice on "what to work on in the absence of 'hard' evidence (such as the data from Galileo's telescope)" (Janssen, 2002, p.480)—tallies with Laudan's (1977, pp.108). Laudan differentiates between two "modalities of appraisal". They lie on a "spectrum of cognitive stances which scientists take toward theories, including accepting, rejecting, pursuing, and entertaining" (Laudan, 1996, p.77). It's vital to "(distinguish) sharply between the rules of appraisal governing *acceptance*" and the "rules or constraints that should govern *'pursuit'* […]" (op.cit., p.111; see also Barseghyan & Shaw, 2017). In the context of acceptance one is preoccupied with "warranted assertibility" (Laudan, 1977, p.110), "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))?

[18] It goes without saying that, like all non-deductive inferences, even rational applications of IBE involve inductive risk. No non-deductive inference rule has a guarantee of truth (Ben-Menahem, 1990, p.232).

By contrast, the context of pursuit is devoted to ascertaining whether a hypothesis deserves further development, and study. “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 & Shaw 2017, p.3). The goal behind pursuit is to *explore* preliminary, perhaps embryonic or inchoate, ideas: to learn more about, test or develop/refine them. In other words, one gauges an idea’s *promise*—without necessarily regarding it as the best description available, with shining epistemic-evidential credentials (in contrast to what considerations in the context of acceptance home in on).

Janssen embraces use of COIs for underwriting inferences to a commitment to pursue hypotheses about common origins (2002, p.484, see also Kao, 2015; 2019 for further historical examples). Kepler’s and Galileo’s arguments for heliocentrism—extolled as paradigm COIs—“clearly illustrate the importance of COIs in the context of pursuit” (p.480). “An important feature of COIs” is their future-directed (“forward-engaged”, p. 476)—but typically *not yet* redeemed—promise or potential: “Copernicus’ model, in a sense, is nothing but a promissory note. As it stands, it is too preliminary and faces too many problems to be a viable alternative to the Ptolemaic-Aristotelian theory” (ibid., see also p. 495, p.501 for other episodes).

Janssen acknowledges the “(temptation) to use the example of Galileo to argue that COIs belong only in the context of pursuit” (Janssen, 2002, p.480). But he staunchly resists it (op.cit., p.481): COIs, for him, are legitimate *also* in the context of acceptance. Presumably, (SUPREME EXPLANATORY POWER) and (WEAK REALISM) drive his—as we’ll see: specious—resistance. Janssen is adamant that “that explanatory considerations *have evidentiary value*“ (p.465, our emphasis) in COIs. Philosophers, Janssen (p.459) avers, are wont to distinguish sharply between evidence/confirmation and explanation. What Janssen chiefly objects to in his opposition to restricting COIs to the context of pursuit, boils down to the demur that this would deprive COIs’ explanatory power of any evidential clout. COIs would then “*only* play a role [...] to help scientists decide what to work on in the absence of ‘hard’ evidence” (p.480, our emphasis). This, however, is a non-sequitur. Moreover, Janssen exaggerates the distance between explanation and confirmation in philosophers’ attempt to shed light on their conceptual distinction (see e.g. Barnes, 2022). Exploiting COIs solely for judgements of pursuit-worthiness *doesn’t* imply that we deny explanation any evidential-empirical significance. The relationship between explanation and evidence is simply a distinct matter.

An idea which explains multiple phenomena in one fell swoop is arguably supported by as good evidence as one may reasonably *hope for* in early stages of theory development—according to Janssen, the "typical and most interesting cases" (p.465) of COI reasoning. In the context of pursuit, evidential hints are of course prized—incomplete and tentative as they may be: they reassure scientists that scientists are "on the right track" (p.504) (see Wolf & Duerr, 2024, sect. 7), rather than "being led down a garden path" (Janssen, 2002, p.466). Pace Janssen's (pp.459) misgivings, such tentative evidence qualifies as "epistemic" in a bona fide, substantive sense—rather than merely "pragmatic" (i.e. a matter of convenience).

More mature theories tend to admit of further tests. They'll also enlarge their stock of explanations. This likely enables a more independent, and solid grip on the theory's explanatory apparatus (including those parts responsible for the common origin explanation). Standard theory evaluation in terms of confirmation and evidence—including and arguably inextricably entangled with assessments of explanatory and predictive achievements in particular—become possible, and apposite; considerations of pursuit, with their aim at further development, now fade into the background. In either stage of theory development—for both the context of acceptance and that of pursuit—explanatory power *can* be granted evidential weight in a genuine epistemic sense.

The same applies to unification (as a dimension of explanation): restricting COIs to pursuit is compatible with assigning unification (as usually exemplified by COIs) evidential weight. Whether and when (and to what extent) one should ascribe unification evidential weight is just a separate question, in need of an *independent* argument (see e.g. Castellani et al., 2025 for a persuasive one). Whether it's the "dominant view in modern philosophy of science" that explanation and confirmation/evidence are "two completely separate things" (Janssen, 2002, p.459) is doubtful as a descriptive claim (see e.g. Schindler, 2018, Ch.3; Crupi, 2021; Dellsén, forth.). In no way is modern philosophy of science inherently wedded to that view. In short, the restriction of COIs to the context of pursuit, we believe, *ought to be* palatable to Janssen—malgré lui.[19]

We thus arrive at our final task: how does a COI or a COI* acquire rational warrant? What *justifies* the pursuit of such an idea, such that it *deserves* further inquiry?

[19] In recent presentations of his work, Janssen himself has adopted this view (cf., e.g., Kao, 2019, p.3266, fn.3).

As the further (sufficient and necessary) condition for this normative statement, we can plump for one of two approaches. Both centre on theory virtues (e.g. Kuhn, 1977; McMullin, 1982, 1996; Douglas, 2013; Carrier, 2013; Schindler, 2018; Keas, 2018) as indicators of pursuit-worthiness. The first—for reasons that will become transparent shortly—we'll moniker the "coherentist approach". It cashes out pursuit-worthiness in terms of plausible, but to-date merely *potential* evidential-epistemic warrant (Šešelja & Weber, 2012; Šešelja & Straßer, 2014): an idea counts as pursuit-worthy, if it plausibly promises the *prospect* of satisfying evidential criteria. The approach is couched in a coherentist epistemology (e.g. Bonjour, 1984): scientific claims receive epistemic justification when they cohere with (or can be harmoniously integrated into) our wider web of scientific belief. The manifold facets of this potential coherence are encoded in the instantiation of what we'll call the "coherentist-evidential theory virtues":

- internal consistency: absence of logical contradictions
- explanatory power, with its dimensions of scope (or unifying power), and fit[20]
- coherence in the narrow sense: the ability to form a harmonious whole (non-adhocness) links to other areas (minimally: consistency).
- programmatic character: "methodological and heuristic means to tackle [existing] problems in [...] further development" (Šešelja & Straßer, 2014, p.3123).

The coherentist rationale for pursuit-worthiness is commonsensical (see Šešelja & Weber, 2012; Šešelja et al., 2012): we may deem an idea promising/pursuit-worthy if we expect it to earn evidential merits—if we expect its seeds of coherentist-epistemic justification (i.e. potential coherence) to come to fruition upon further pursuit.

In virtue of their explanatory power, amply underscored by Janssen, COIs fit the bill. Let's, for simplicity, assume no egregious *defects* on the above other aspects of coherence.[21] The eponymous capacity of COIs for explaining otherwise puzzling "striking coincidences" is

[20] Note that no particular account of explanation is stipulated (Šešelja & Straßer, 2014, p.3126). "Explanation" should here be construed broadly. It ought to include also the form of conceptual integration, usually labelled "accommodation" (e.g. Douglas & Magnus, 2013; Barnes, 2022; Dellsén, forth.). We side with Janssen: caution and non-presumptuousness counsel maximal permissiveness, especially vis-à-vis scientific practice.

[21] An example of a COI that *doesn't* count as pursuit-worthy because it purchases some gain in explanatory-unificatory coherence by sacrificing *substantial* coherence in other areas (in this case: the factual and theoretical background knowledge of relativistic physics and cosmology) is MOND, an alternative to the Dark Matter hypothesis (§IV.1). See Duerr & Wolf (2023) for a detailed analysis. Another example from cosmology is the Steady State model of the universe in the aftermath of the 1964 discovery of the Cosmic Microwave Background radiation (see e.g. Kragh, 2019 for details): any claims to empirical adequacy would be purchased by significant ad-hocness liabilities in a coherentist sense (Schindler, 2018, Ch.5). A similar case (op.cit., pp.142), of special interest—last but not least since Janssen himself discusses it (2002, pp.497; 2002*)—is Lorentz's ether contraction hypothesis.

naturally interpreted as auspicious prospect of impressive potential coherence. Bringing together disparate phenomena, COIs frequently also display a programmatic thrust: they facilitate the transfer and applications of ideas—the "(embedment) in a theoretical and methodological framework which allows for the further research of the system to proceed in spite of the encountered problems, and towards their systematic resolution" (Šešelja & Straßer, 2014, p.3131). In short, the coherentist rationale pronounces COIs pursuit-worthy because they can typically—ceteris paribus—point to potential, but plausible, coherentist evidence.

We obtain same result for our generalised COIs, COI*s. Recall that they relax the "striking coincidence" data (observational or theoretical) that bear on them to constraints more broadly. COI*s licence inferences to pursuit because they promise potential coherence with respect to those constraints (see Šešelja & Weber, 2012 for a coherentist analysis of plate tectonics, the "picture-perfect example of a COI", Janssen, 2002, p.469, fn.21).

A second rationale for the pursuit-worthiness of COIs (and COI*s) is a bit more general. In particular, it doesn't hinge on a coherentist epistemology. Instead, it turns on a Peirce-inspired "economy of research" which strives to optimise (cognitive) resource allocation for investing in—that is, pursuing—ideas (see the elaborations by Nyrup, 2015 within a more formal and Bayesian framework, and Duerr & Fischer, forth. for a broader outlook, cf. Wolf & Duerr, 2024ab; Fischer, 2024a for case studies). Janssen's (2002) indeed evinces sympathies to such economic considerations: "(g)iven that both available time and available resources are finite, these are crucially important decisions [viz. which research project to work on]. Scientists need to feel that the project they decide to pursue promises greater *rewards* than alternative projects" (p.466).

The guiding thought behind this "economic approach" is a cognitive-epistemic cost/benefit analysis. A profitable balance—as far as one can judge, secundum prudentiam—undergirds the rationality of pursuit. Here, benefits and costs are metaphors, standing for cognitive-epistemic achievements, and the cognitive efforts researchers spend in pursuit of an idea, respectively. Nyrup (2015) and Duerr & Fischer (forth.) propose to identify these with the prospect of (or already actual) instantiation of certain theory virtues. Specifically, scientific theories, models or hypotheses are supposed to represent cognitive benefits if they exhibit explanatory and unificatory power, coherence (absence of ad-hocness), accuracy, or confer understanding (construed as grasping how things hang together, see e.g. Baumberger et al., 2017; Elgin, 2017;

Dellsén, 2020; De Regt, 2022). When aspiring to such gains, one inevitably incurs costs. They are lowered, the advocates of the economic approach to pursuit-worthiness suggest, by a *different* set of theory virtues:

- (mathematical or ontological) simplicity and parsimony
- ease of testability
- conservatism and familiarity,
- a powerful heuristic that sketches fruitful paths of further inquiry, and equips the researcher with versatile toolbox for tackling problems, sophisticate and extending an idea to other areas (cf. Lakaktos, 1989, pp.48).

Analogously to the weighing of anticipated costs/benefits in economic investments, the economic justification for pursuing an idea consists in a favourable balance of those cognitive costs and benefits (in economic terms: a favourable utility estimate): the anticipated cognitive benefits are judged to outweigh the cognitive costs. Pursuit-worthy ideas strike a propitious balance of plausibly anticipated prospects for the instantiation of theory virtues (cf. Kuhn, 1977, and Duerr &Fischer, forth., sect.V).

Both Janssen's original COIs and our generalised COI*s plausibly satisfy such a favourable utility estimate. The explanatory merits (or prospects thereof) carry over from our earlier discussion of the coherentist approach. The economic approach has a decisive advantage over the latter in that it factors in central aspects of COIs whose classification as "explanatory"—and a fortiori: evidential—seems iffy: simplicity, and heuristic power/fruitfulness, in particular,[22] don't sit comfortably amongst the coherentist-evidential virtues. Vis-à-vis the trail-blazing, programmatic nature of COIs, which beckons researchers to further explore a tantalising idea, Janssen (p. 466; 513; pp.478; also 2008, passim) rightly highlights both as distinctive trait of COIs. They find a natural home within the economic account of pursuit-worthiness: they enhance or *promote* research and the reaping of cognitive benefits (keeping scientific pursuit at moderate cognitive costs)—without necessarily *constituting* cognitive-epistemic achievements in their own right. In short, also the economic rationale pronounces COIs and COI*s pursuit-worthy because they typically strike a favourable estimated cognitive cost/benefit balance in terms of salient theory virtues, actually or plausibly instantiated.

[22] It's common—and much less controversial than for other theory virtues—to regard them as *pragmatic* factors, not squarely related to truth or epistemic justification (e.g. Douglas, 2013; Worrall, 2000).

IV. COI*s as inferences to pursuit-worthy ideas

By way of summary, let's distil from the foregoing the upshot of COI* reasoning, our proposed modification of Janssen's original COIs (as per the tenets, compiled in §II). Re-examining his seminal reflections yielded a handful of insights and, concomitantly, suggested ameliorations, as well as a normative-methodological supplement.

The first amelioration generalises the class of facts that can trigger COI reasoning.

> **(CONSTRAINT OMNIVORY)** Whereas COIs start from *empirical phenomena* as explananda in a COI story, we propose to include also "more theoretical" facts: COI*s start from *scientific constraints* more generally. Whereas the common origins of empirical phenomena are the target explananda of Janssen's COIs, our COI*s are "constraint-omnivorous": they account for a plethora of constraints at once.

Our second suggestion is to decouple COI*s (or COIs) from an ontic-causal account of explanation. It jettisons (CAUSAL EXPLANATION) in the original COI account, in favour of a more comprehensive class of explanatory (and kindred) relations (some of which, in contradistinction to explanations, admit of degrees/gradations—an additional advantage of our account, cf. Schindler, 2018, pp.136).

> **(EXPLANATORY PLURALISM)** Whereas Janssen originally portrays COIs as purveying *causal* information—their common origins as common causes or mechanisms—we drop this restriction to causal explanations: *also non-causal* types of explanations are permissible. In fact, we allow for a broader array of ways in which an omnivorous COI*'s constraints can be *accounted for* (such as accommodation, or coherent integration).

Thirdly, we found (SUPREME EXPLANATORY POWER) overblown.[23] COI*s aren't the epistemic-evidential faits accomplis as which Janssen portrays them.

> **(*TANTALISING* EXPLANATORY POWER)** The explanatory power of COI*s is impressive, in an epistemically non-trivial/non-arbitrary sense. Yet, it would be hasty to declare it, at least on its own, *compelling* evidence.
>
> The extent to which explanation and confirmation/evidence are intertwined is a separate matter; they plausibly are, though this is inessential here.

[23] Right at the end of his paper, Janssen (2002, p.514) seems to backtrack on his earlier (SUPREME EXPLANATORY POWER): COIs are "(risky propositions) at the frontiers of science. […] All one can ask for is the presence of safeguards against garden paths." We whole-heartedly concur with *this* characterisation, a paraphrase of (HOPEFUL REALISM).

Fourthly, mindful of their evidential-epistemic blemishes, we tone down (WEAK REALISM) to plausible hopes.

> **(*HOPEFUL* REALISM)** COI*s fall short of the evidential-epistemic standards that realists typically presuppose: their explanantia are epistemically-evidentially (still) too insecure. Nonetheless, COI* aren't entirely devoid of epistemic merits: meeting a credibility threshold, they qualify as reasonable working hypotheses that we may countenance.

Fifthly, (HOPEFUL REALISM) implies a *pruning* of (COMMITMENT) in Janssen's account. We limit kosher use of COIs (and COI*s) to the context of pursuit.

> **(PURSUIT)** Whereas Janssen deems COI reasoning legitimate *also in the context of acceptance* (i.e. as conferring evidential-epistemic reasons for belief), we restrict the reach of COIs and COI*s to the *context of pursuit*: they only warrant inference to promising *working hypotheses* which deserve imposition as constraints and/or further scrutiny, elaboration, and testing—an incentive to further work with them.
>
> *Contra Janssen*, this doesn't commit us to divesting COI/COI* stories of evidential-epistemic significance (nor explanatory power more generally). In particular, the theories constructed through COI*-reasoning may account for the empirical phenomena or constraints in an evidentially-epistemically relevant manner.

Finally, we showed how to *justify* inference to pursuit, based on COI/COI*-reasoning, as normatively-methodologically reasonable.

> **(PURSUIT-WORTHINESS)** Assuming no overriding shortcomings in other regards, COIs and COIs* underwrite warranted inferences to pursuit-worthy ideas on two major models of pursuit-worthiness.
>
> The coherentist account conceives of pursuit-worthiness as the plausible prospect of evidence/epistemic justification in the sense of a coherentist epistemology. Then, the characteristic coherence with constraints that COI*s promise grounds their inference to pursuit-worthy ideas. The virtue-economic account conceives of pursuit-worthiness as a favourable balance between cognitive costs and benefits, a favourable score in terms of theory virtues. Again, the characteristic coherence with constraints tends to secure this for COI*s.

(PURSUIT-WORTHINESS) fills a gap that Janssen (somewhat deliberately, 2002, p.463, p.513) left. We thus vindicated that "COIs capture an *element of rationality* in theory choice" (ibid.).

V. Cosmic Common Origins and COI*s

In the spirit of Janssen's pioneering paper, this section will further strengthen the case for COI*s (as summarised in §IV) by three case studies. Each covers a spectacular episode in modern cosmology: the Dark Matter hypothesis (§V.1), the Dark Energy hypothesis (§V.2), and "(t)he hottest COI story of contemporary science" (Janssen, 2002, p. 469, fn.21)— cosmic inflation

(**§V.3**). The case studies illustrate how COI*-reasoning plays out in, and permeates, science. They attest to the fertility of Janssen's proposal for historiography of science.

V.1. Dark Matter

We'll first (**§V.1.A**) outline, in broad brushstrokes, the pivotal points in the history of the Dark Matter hypothesis, the postulate that a significant amount of matter in the universe is non-luminous, and known so far only through its gravitational effects. **§V.1.B** will spell out how the establishment of the Dark Matter hypothesis fits the mould of COI*-reasoning.

V.1.A The Dark Matter problems

In the 1930s, "several authors noticed an inconsistency between the observed velocity dispersion of galaxies in galaxy clusters and that same dispersion as it followed from calculations on the basis of visible, luminous matter" (de Swart et al., 2017, p.1). Pioneering observations in this regard were Oort's determinations of velocity dispersion of stars perpendicular to the plane of our galaxy. They showed that, given standard (Newtonian or general-relativistic) gravity, more mass must exist in the Milky Way than all visible matter of known type. Zwicky subsequently demonstrated "the enormous mass-to-light ratios of clusters of galaxies, as determined by application of the virial theorem to the velocity dispersion of galaxies in the Coma cluster, brought vividly to light just how much dark matter there had to be in these systems" (Longair & Smeenk, 2019, p.425). These findings buttressed an increasingly strong case for an empirical phenomenon, **(MASS-DISC$_{\text{cluster}}$)**: a pronounced discrepancy in galaxy clusters between luminous mass and the mass one infers from the observed gravitational effects. A curious astrophysical anomaly in an apparently peripheral domain, (MASS-DISC$_{\text{cluster}}$) sparked little broader interest. Hence it comes as little surprise that Zwicky's first unambiguous articulation of the **Dark Matter hypothesis**—the postulate that large amounts of yet undetected, non-luminous/dark matter exist, and that its presence accounts for (MASS-DISC$_{\text{cluster}}$)–largely fell on deaf ears.

In the late 1950s attention shifted to that discrepancy. In part, the observational basis had become too strong to just shrug it off. Arguably more importantly, though, General Relativity started to prosper again during the 1950s—after decades of neglect and stagnation—as a flourishing and expanding area of research, with multiple theoretical and empirical discoveries and progress (e.g. Eisenstaedt, 1989; Longair, 2019ab). Scientists were eagerly starting to explore new applications of General Relativity, especially in astrophysics.

Various hypotheses were mooted to account for ($\text{MASS-DISC}_{\text{cluster}}$). Their respective cases remained inconclusive—well into the late 60s and early 70s: "(t)oo few observational and theoretical constraints were available to force a consensus on how to interpret the discrepancy. The existence of additional unobserved mass was just one possibility among a considerable number of alternatives" (de Swart, 2017, p.3).

With the rise of radio astronomy in the 1950s and 1960s, alongside novel detection techniques, evidence for another astrophysical phenomenon was garnered: galaxies tended to display *flat* rotation curves, **(ROT-CURVE)**. That is, the orbital velocity of gas and stars in galaxies continued to stay constant, well beyond where one would expect it to drop, if all gravitational effects could be attributed to visible matter. Once again, this signalled "missing mass". However, the rotation curves and ($\text{MASS-DISC}_{\text{cluster}}$) weren't seen as linked yet: "(e)ven though there were plenty of observations of flat rotation curves in the early 1970s […] interpretations of their consequences for the existence of unseen mass were scarce and lacked urgency" (ibid.). De Swart et al. (op.cit., p.5) sum up the situation: "the possible existence of unseen mass was a potential solution to two independent problems that arose in the 1960s and early 1970s. The suggestion was highly uncertain and itself problematic, if considered at all. Indeed, there was no consensus, in either case, on what a proper interpretation of observed results should be, and there was no definite sense as to how much weight might be attributed to any interpretation. […] (B)esides a few exceptions […], the two problems were studied *separately*."

Two papers from 1974 brought the two by-then well-known phenomena together as manifestations of the *same* missing mass. This established the adoption of the Dark Matter hypothesis in the scientific community: as a response to the phenomena, it was judged to deserve serious consideration.

What occasioned this elevation? de Swart et al. (2017; see also de Swart, 2019) identify the maturation of cosmology as the clincher, with a solid theoretical framework (firmly built on General Relativity), and underpinned by a plethora of observations. The core question within mainstream cosmology concerned the mass density of the universe. This parameter determined the choice of cosmological model. "(F)or essentially nonexperimental reasons" (Ostriker et al., 1974, p.L1), a strong preference prevailed for a mass density that would correspond to a geometrically "just closed" (ibid.) cosmological model. Another missing mass problem, **(MASS_{cos})**, now gaped—at cosmological scales: the mass estimates on the basis of visible matter in galaxies yielded a mass density too low for a closed model. “At this point, only after astronomy had acquired a new cosmological focus that produced a search for extra matter, did the flat rotation curves and the cluster mass discrepancy come together” (de Swart et al., 2017, p.9). The Dark Matter hypothesis, in short, was “born” (de Swart, 2019, p.19)—was adopted as a serious working hypothesis—when the above-cited landmark papers put together the three constraints: the two astrophysical ones, (MASS-DISC$_{cluster}$) and (ROT-CURVE), and the cosmological one (MASS$_{cos}$). Each pointed (more or less consistently) to the same amount of missing non-luminous mass.

The Dark Matter hypothesis voraciously accreted more constraints. This rapidly propelled the vigour of research on it. One important set of novel constraints, subsumed under **(MASS$_{BBN}$)**, came from nuclear physics in the late 1960s (see Turner, 2022 for details). The relative abundances of the light elements (deuterium, helium, and lithium), baked in the hot early stages of the Universe, imposed increasingly tight constraints on the total mass density of ordinary (“baryonic”) matter, composed of quarks. Not only was the total mass density nowhere near what was theoretically expected; it also fell significantly below what the astrophysical evidence from (MASS-DISC$_{cluster}$) and (ROT-CURVE) was suggesting. Vis-à-vis such data from primordial nucleosynthesis, ordinary matter couldn’t make up all—nor, in fact, most—matter in the universe. With (MASS$_{BBN}$), the Dark Matter hypothesis had forged a distinctive link to particle physics.

Other links soon emerged. The early 1980s, for instance, brought the first large 3D survey of galaxies, the CfA redshift survey (1982). It chartered the “cosmic web”, the distribution of matter at the large-scale level (galaxy clusters and above). This enabled comparisons of the output of simulations of the universe, and the mass estimates, based on visible matter. Significant improvements in the 2000s pulled even more forcefully in the same direction: the found large-scale structure formation patterns can’t be held together by luminous matter alone; the latter—provided one assumes the validity of General Relativity—must be supplemented by Dark Matter. These constraints from large-scale cosmic structure, **(COSM-STRUC)**, further solidified the case for Dark Matter—yet another cosmological line of evidence. They also allowed to compare predictions of simulations with *hot* Dark Matter models (i.e. more specific variants of the Dark Matter hypothesis, according to which Dark Matter consists of matter at ultra-relativistic speeds, for instance neutrinos) to those with cold Dark Matter. The discrepancies between the former and the observations quickly ruled out the hot Dark Matter. Moreover, "(a) decade later, the predictions of cosmological simulations had shifted in focus from the distribution of cold dark matter halos to the *shapes* of those halos" (Bertone & Hooper, 2018, p.23). This opened a fruitful new area of further research.

Other data in favour of Dark Matter on the basis of large-scale cosmological observations quickly followed. Observations of weak lensing events around individual massive galaxies in the 1990s (i.e. distortions of galaxy images, due to the gravity-induced bending of light by distant galaxies) spurred on the hunt for weak lensing observations due to large-scale structure. In 2000, success was announced, **(COSM-STRUC$_{\text{LENS}}$)**: four research teams had, simultaneously, detected the first weak lensing on cosmic scales—again in line with the Dark Matter hypothesis of substantial "missing mass" whose gravitational effects remained unaccounted for, if one confined oneself to luminous matter. Lensing data of the early 2000s played a major role in convincingly ruling out the most ordinary candidates for Dark Matter, "Massive Astrophysical Compact Objects", so-called MACHOS, such as planets, brown or red dwarfs, neutron stars or black holes (e.g. Longair & Smeenk, 2019, sect. 11.2.2).

The final, but arguably most powerful, set of constraints we'll consider, collectively labelled as **(DM-CMB)**, stems from the cosmic microwave background (CMB). The latter denotes the nearly uniform electromagnetic radiation engulfing us. It purveys a fossil-like snapshot of the Universe at ca. 380,000 years old, at the point when matter and light de-coupled from each other. Its detection in 1964 established the cosmological model of the Hot Big Bang, and conferred upon cosmology scientific maturity, the status of a fully empirical science (see e.g. Partridge, 2019). Germane to the Dark Matter postulate are the *deviations* of this radiation background from near-uniformity; these tiny temperature fluctuations mirror the seeds out of which the galaxies and cosmic structures we observe today grew. Already in the early 1980s, Peebles "pointed out that the absence of fluctuations in the cosmic microwave background at a level of about 10^{-4} is incompatible with a universe that was composed of only baryonic matter and argued that this problem would be relieved if the Universe were instead dominated by massive weakly interacting particles [...]" (Bertone & Hooper, 2018, p.22).

The WMAP mission (2001–2010) refined these measurements. They revealed so-called acoustic peaks in the power spectrum—oscillations caused by interactions between photons and baryons before they decoupled from each other. The relative heights and positions of these peaks strongly indicated the presence of non-baryonic dark matter; purely baryonic models couldn't account for the observed structure. The PLANCK mission (2013-2018) yielded the most precise measurements to date. They showed that Dark Matter makes up about 26.8% of the universe's energy density. The structural-statistical patterns of those CMB fluctuations directly shed light on Dark Matter: cold, non-baryonic Dark Matter (CDM) best fits the data, as hot dark matter would have erased small-scale fluctuations. Today, CDM forms an integral part of the cosmological standard model.

V.1.B Dark Matter and COI*s

The historically growing significance of Dark Matter exemplifies COI*-reasoning (cf. de Swart et al., 2017, p.11). The latter can be seen as explicating a sense of rationality underlying this development.[24]

It's natural to regard (MASS_{cos}) as the crucial "missing link" in virtue of which the Dark Matter hypothesis surpassed a widely perceived benchmark. With this third constraint in place, it qualified as properly "constraint-omnivorous" in the sense of **§II** and **§III.1**: *three* "striking coincidences" pointing in the same direction arguably seemed too much to shrug off. The Dark Matter hypothesis accounted for them by imputing them to the effects of the same origin, Dark Matter. Accordingly, given (PURSUIT-WORTHINESS), the scientific community was justified in taking Dark Matter seriously.

From the perspective of the coherentist account (§**III.2**), we can spell out the pursuit-worthiness in terms of a plausibility threshold: interpreted as potential lines of evidence or as potential explanatory coherence amongst three phenomena, (MASS_{cos}), (ROT-CURVE) and ($\text{MASS-DISC}_{\text{cluster}}$) *together* pushed the Dark Matter hypothesis beyond that threshold. Alternately, from virtue-economic perspective, the prospect of the cognitive benefit—a unified explanation or accommodation of these diverse phenomena—plausibly outweighed the costs (in the form of additional parameters).

Ever more theoretical and empirical results accumulated that seem (and seemed) to bear on the hypothesis; the corpus and strength of constraints on it steadily increased ((MASS_{BBN}), (DM-CMB), (COS-STRUC) and ($\text{COS-STRUC}_{\text{lens}}$)). The constraint-omnivorous Dark Matter postulate

[24] Note how the epistemic caution and reticence about realist commitments, recommended by (HOPEFUL REALISM) (**§IV**)), seems entirely appropriate to the case of Dark Matter (see Martens, 2022; Allzén, 2022 for illuminating analyses).

thereby grew in pursuit-worthiness (on both models): it promised even greater coherence, explanatory power and wider scope.

Let's delve a little deeper. The Dark Matter hypothesis (in its default variant) rests on General Relativity, the nigh-universally accepted standard theory of gravity, well-confirmed in many different domains (e.g. Will, 2018; Ishak, 2019). The attendant "gravitational conservatism" entails three merits. The first flows from General Relativity's entrenchment as a pivotal background assumption in several areas of astrophysics and cosmology (e.g. in the context of gravitational waves, black holes, neutron stars, galaxy dynamics, or primordial cosmology). Coherentists (§III.2) will hence cherish the conservatism as heralding a modicum of (potential) epistemic justification. Advocates of the virtue-economic account will no less enthusiastically hail it. They may, however, stress ramifications for the pragmatics of research: modifications of, or deviations from, General Relativity likely bleed into a multitude of domains. Not always are such effects known—or in fact easy to fathom. Major complications typically ensue; for the most part, things remain simpler overall, if one retains General Relativity. Quine's (1970) pragmatic "maxim of minimal mutilation" militates against opening a potential Pandora's box—the prospect of *significant* extra cognitive costs.

Quine's maxim buttresses another advantage that both models of pursuit-worthiness recognise: epistemic caution. Cleaving to General Relativity, we needn't worry about potentially dramatic and far-reaching repercussions of new assumptions. Thanks to the conservatism, we don't incur additional inductive risk (other than what our background assumptions already commit us to).

A third advantage of gravitational conservatism concerns heuristic aspects (most naturally accounted for in the virtue-economic model as keeping cognitive costs manageable). Conservatism allows researchers to tap existing resources for tackling the various phenomena: the powerful—empirically fruitful—set of problem-solving and modelling techniques, inference and justificatory practices that the general-relativistic research programme (and its Newtonian predecessor, see Smith, 2014) affords. This makes it possible to transfer valuable cognitive tools and ideas. A case in point is the weak lensing, (COSM-STRUC-LENS). The effect had essentially already been predicted by Einstein in 1936 and Zwicky in 1937 (being, at bottom, a generalisation of the light deflection around the Sun during an eclipse, famously confirmed by Eddington in 1919).

Advantages similar to these three emanate also from prospects of links between Dark Matter and non-cosmological/astrophysical areas of physics. Besides representing a case for "external coherence", they also allow, or facilitate, the testing of theories with *other* principal domains. It's this prospect that especially thrilled (and continues to thrill) particle physicists. The most popular candidates for Dark Matter tend to be more speculative proposals. Their empirical scrutiny has proven knotty on their "home turf". Axions, or super-symmetric particles (e.g., the WIMP) are cases in point. A major incentive for pursuing such ideas *as Dark Matter proposals* is that convincing links to Dark Matter phenomenology would provide much-desired evidence, which else appears elusive vis-à-vis the requisite energies in laboratory/accelerator contexts.

One mustn't underestimate the ability to draw on existing knowledge and ideas readily available "off the shelf" (rather than having to laboriously create new theoretical and empirical knowledge) as a practical bonus. To be sure, progress is continuously made; some options are ruled out. Before the 1970s, it seemed plausible to assume that Dark Matter could be provided by ordinary (baryonic) matter that happened to be non-luminous (Bertone & Hooper, 2018, sect. V), the MACHOS mentioned above. Their theoretical and empirical profiles—constraints from other domains of inquiry—could be harnessed for specific tests. Similar remarks apply to neutrinos (before in the 80s they were mostly discarded as unviable Dark Matter candidates). In the case of the now favoured models based on extensions of the standard model (supersymmetric particles and axions in particular) the heuristic power of their essentially theoretical profile redounds to keeping their cognitive costs moderate. Physicists can, more or less straightforwardly, study their properties.

V.2. Dark Energy

For our purposes, the "Dark Energy hypothesis" shall denote the conundrum of a non-vanishing cosmological constant Λ: should we assume such a $\Lambda \neq 0$? Or rather (since $\Lambda = 0$ reproduces standard General Relativity), ought we to assign it a non-zero value?

For a better grasp of the hypothesis and its rich history as a problem, we'll again first revisit some milestones (§V.2.A). We'll then demonstrate more explicitly that the Dark Energy hypothesis, too, exemplifies COI*-reasoning (§V.2.B).

V.2.A The checkered history of the cosmological constant

In 1917, in a paper inaugurating modern cosmology, Einstein proposed a modification of his original 1915 field equations. The physical principles which had served as the foundations for General Relativity, Einstein realised, turned out not to uniquely entail his 1915 version (cf. Weinberg, 1972, Ch.7.1 for a contemporary presentation). Rather, they allowed for a generalisation: the addition of an additional term, with a new constant of nature, the cosmological constant, Λ. Einstein's motivation for this innovation was to describe a static (unchanging) world—in line with both the prevailing opinion of his time (bolstered somewhat by tenuous empirical-astronomical data), as well implement his philosophical (viz. Machian) convictions, **(MACH-STAT)** (see Smeenk, 2013; O'Raifeartaigh et al., 2017).

Soon he had to abandon the latter for various (primarily mathematical) reasons. The subsequent decade and a half also cast increasingly graver doubt on the assumption of a static universe. Observational evidence was mounting—most notably in the form of Hubble's redshift-distance relation—in favour of an *expanding* universe, a notion that was gaining traction in the late 1920s. By the early 1930s, the dynamical evolution of the universe was widely embraced. Einstein himself jettisoned Λ (and its attendant extension of the original field equations). The fact remained, though, that the physical principles underlying General Relativity were consistent with a cosmological constant term. Also General Relativity's *mathematical* principles, as Weyl and Cartan showed (cf. Weinberg, 1972, Ch.72. for a contemporary presentation), fixed the theory only up to a cosmological constant term, with Λ as a free parameter—**(MATH-Λ).**

Accordingly, one could plausibly insist that Λ be retained for generality, and that arguments be expressly adduced for any specific value (including zero). Following Einstein, many ignored Λ—setting it to zero—primarily for reasons of expediency and simplicity. An influential cosmological model of the time, the Einstein-de Sitter model, incorporated this option (see O'Raifeartaigh et al., 2021 for details). Other cosmologists, such as Eddington, de Sitter or Tolman "felt it was an error to assign the value zero to a term that was in fact unknown." (O'Raifeartaigh et al., 2018, p.86); instead of brute fact stipulation, they called for an experimental determination.

Prima facie good arguments for reckoning with a non-zero Λ were occasionally voiced. For instance, still in 1952 Bondi wrote in his authoritative cosmology textbook: Lemaître's cosmological model (which postulated a non-vanishing cosmological constant) was "the best relativistic cosmology can offer". One reason was the so-called age problem, **(AGE)**. The latter referred to the glaring paradox, known at least since the early 1930s, that the cosmos as calculated from expanding models of the universe (without a Λ) appeared to be *younger* than the age of the oldest known stars (see e.g. Kragh, 1996, Ch. 2.4)! The above-mentioned cosmological models developed by Eddington (1930, 1931) and Lemaître (1927, 1931), by dint of a non-vanishing Λ, accommodated a(ny) suitable age. Those models interposed a cosmic "loitering/coasting phase" between an initial phase of gravity-induced deceleration and a Λ-driven final phase of acceleration. During this loitering phase (whose length depends on the adopted value of Λ), Λ's repulsive effect balanced out the gravitational pull. Temporary stagnancy resulted. By the end of the 1950s, thanks especially to the efforts of Sandage, Hubble's original estimates of the expansion rate underwent correction to significantly lower values. The problem frustratingly persisted: the standard cosmological models *sans* Λ still implied a timespan of the universe—absurdly—almost half the age of the oldest stars. Sandage himself, in 1961, suggested that the Age Problem might intimate a positive Λ.

Another reason for Bondi's above-cited verdict was that Lemaître's loitering model promised to "(offer) a possible mechanism for the formation of galactic structures, a phenomenon that presented a formidable puzzle in the context of the discovery of cosmic expansion […]" (O'Raifeartaigh et al., 2018, p.87; see also Kragh, 1996, Ch.6.2), **(GAL-FORM)**. The loitering phase acted as a relatively stable period during which perturbations in matter density (about whose origins Lemaître refrained from speculating) would condensate. Admittedly, Lemaître's ideas were somewhat qualitative. Nor were they free from difficulties. "But the general idea of tying structure formation to the coasting phase of the Lemaître model remained of interest through the 1950s […]" (Earman, 2001, p.203). The late 1960s dashed such hopes again: the model was revealed not to be viable (ibid., p.205).

Interest in Lemaître's cosmological model, with its non-vanishing Λ in particular, was briefly revivified in the mid-1960s from a different angle: the effects of a cosmological constant could explain an apparently observed excess of quasar-stellar objects from a particular cosmic era (at particular redshifts), **(QUASAR)**. Within a few years, though, the effect proved to be spurious. Interestingly, Petrosian, who had collected the pertinent data, underscored that *not* all Lemaître models had been ruled out, nor that a zero value for Λ was forced by the data (Earman, 2001, p.206). As far as observational astronomy was concerned, Λ faded once more—at least, pro tem—into the woodwork.

Notwithstanding this ill fate, the episode fired “the collective imagination” of particle theorists (ibid.): attention to a possibly non-zero cosmological constant had raised the issue of an enticing link between particle physics, especially high-energy domains with their latitude for speculation, and the cosmological constant—(Q-VAC). Re-analysing Lemaître’s earlier (classical) interpretation of the cosmological constant as an energy density of the vacuum, in 1968, Zel’dovich “convinced the physics community that there was a connection between Λ and the ‘energy density’ of empty space, which arises from the virtual particles that blink in and out of existence in a vacuum” (Calder & Lahav, 2010, p.32). That such *quantum* vacuum fluctuations actually exist gained, by and large (especially thanks to the Casimir effect), acceptance over the course of the 1950s.

Zel’dovich’s quantum field theoretical arguments entailed a contribution to the Einstein Equations that automatically had the *form* of a cosmological constant modification. Only its value was the fly in the ointment: it was 120 orders of magnitudes too big! Despite this spectacular failure, Zel’dovich’s idea sparked off a “minor industry” (Earman, 2001, p.207). The discrepancy between the calculations and the observational constraints on Λ “was not yet considered too pressing. Given the lack of empirical evidence for a cosmological constant, most physicists assumed that the quantum energy of the vacuum was reduced to zero by some as-yet unknown symmetry principle” (O’Raifeartaigh et al., 2018, p.94). In the same vein, the said mismatch presented a welcome opportunity for exploring and testing ramifications of speculative physics. Especially supersymmetry and the revisions it would afford was hoped to remedy Zel’dovich’s idea.

From the early 1980s onwards, also a different—at the time, empirically *not yet* compelling—new development in high-energy physics, making inroads into cosmology, boosted the case for $\Lambda > 0$: the theory of cosmic inflation (§V.3), (INF). It was largely taken to predict a flat spatial geometry. This rubbed against observational data: the amount of matter in the Universe—even factoring in Dark Matter (§V.1)—fell far below the requisite density. A non-vanishing cosmological constant was quickly gleaned as a rescue manoeuvre. “(F)rom the mid-1980s onwards, a number of analysts suggested that an inflationary universe of flat geometry, low matter density and a positive cosmological constant gave a better fit to astronomical data than the Einstein-de Sitter […]” (O’Raifeartaigh et al., 2018, p.97).

This aligned with another line of astronomical constraints which was gradually materialising. Detailed studies of galactic evolution from the mid-1970s onwards (studies which affected measurement techniques for the expansion rate) led to suggestions of a positive cosmological constant. “However, uncertainties in galaxy luminosity prevented a clear diagnosis.” (op.cit., p.96). 1990 brought the “quiet breakthrough” (Calder & Lahav, 2010, p.35): galaxy surveys provided evidence for galaxy clustering that was most easily accounted for by a cosmological constant, making up 80% of the energy content of the Universe, **(GAL-EVO)**. Further studies on galactic growth corroborated the findings.

The turn of the new millennium saw several advances, eliciting the “real excitement about Λ” (Earman, 2001, p.212)—and consolidating Λ as part of the cosmological standard model (commonly referred to as the “ΛCDM model”). On the one hand, measurements of the cosmic microwave radiation background in the early 2000s were capable of resolving miniscule temperature fluctuations, **(Λ-CMB)**. They allowed precise determination of key cosmological parameters. This strongly indicated a cosmological constant, as the major contribution to the spatially flat Universe’s total energy budget.

(Λ-CMB) is seconded by a new generation of observational missions, launched in the late 1980s. They were devoted to determining the extent to which the Universe’s acceleration was speeding up (see e.g. Calder & Lahav, 2010; Lahav, 2020). (Recall this is one of the natural manifestations of a non-vanishing cosmological constant.) Here, supernovae of a particularly homogeneous type—exploding stars with nearly the same absolute luminosity at their peak—were the decisive objects of study: thanks to their known intrinsic brightness, they serve as “standard candles”, underwriting reliable inferences from observed brightness to distance. Such supernovae measurements, especially those in the late 1990s, confirmed an upward—super-linear—trend in the redshift/distance relation: the expansion of the universe seems to accelerate, **(SUPERN)**—a discovery for which the 2011 Nobel Prize in Physics was awarded.[25]

[25] We’d be remiss to not mention one of the most exciting *very* recent developments: the latest data combining measurements from the CMB, supernovae, and baryonic acoustic oscillations displays a notable preference for *dynamical* explanation of Dark Energy phenomenology over the cosmological constant (DESI, 2025). While these results are still preliminary and must await further data and analysis before any strong conclusions can be drawn, much of the analysis in this paper regarding COI* reasoning in the context of the Dark Energy problem would still hold. For example, the most straightforward candidate for dynamical Dark Energy would be a so-called thawing scalar field known as quintessence. Such quintessence models (see e.g. Chiba 2009; Wolf & Ferreira 2023) approximate a cosmological constant at early times when the field is frozen, but then begin to evolve only at recent times. These models would preserve GR, while still offering a similar COI* story regarding constraints surrounding

V.2.B Dark Energy and COI*s

As another major unresolved problem in contemporary physics, the Dark Energy hypothesis incontestably *screams* for further pursuit. We'll here explicate how COI*-reasoning underwrites, and articulates the rationale behind, such a verdict.[26]

Beforehand, it's instructive to remark upon a startling feature of the Dark Energy hypothesis: the emergence and evanescence of key constraints that, *historically*, made it pursuit-worthy. For much of its history, the Dark Energy hypothesis (along with (MATH-Λ)) has absorbed a wide range of constraints. Some initially operative constraints—such as (MACH-STAT), (GAL-EVO), and (QUASAR)—later proved unreliable; as evidence evolved, they were discarded. Only recently has the cluster of agglomerated constraints stabilised, with (SUPERN), (Λ-CMB), and (AGE).[27] This volatility of some constraints reminds us of the fallibility of all our theoretical and empirical knowledge in science—and a fortiori of all methodological evaluations.

That the Dark Energy hypothesis exemplarily conforms to COI*-reasoning is readily gleaned from its historical trajectory. It traces back a number of phenomena to a common origin: a non-vanishing Λ naturally explains or accommodates (AGE), (GAL-FORM), (QUASAR), (GAL-EVO), (Λ-CMB), and (SUPERN) all at once (or, given the limitations and errors vexing some of these constraints: at least *appeared* to do so).

It deserves to be underscored how much the Dark Energy hypothesis coheres with background knowledge. Einstein's recognition that its physical principles imply the inclusion of a cosmological constant suggest that Λ is a theory-inherent, *free parameter* of General Relativity: including a cosmological constant isn't a modification of General Relativity, which goes *beyond*

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the growth of cosmic structure, the missing energy component in the CMB to obtain a flat universe, and luminosity distance measurements for supernovae, etc.

[26] As in the case of Dark Matter (§V.1.B), (HOPEFUL REALISM) with its reticence about any strong ontological commitments seems appropriate for Dark Energy. In this spirit, for instance, Turner (2018, p.1261) flags the provisional makeshift nature of the cosmological standard model, with its Dark Energy and Matter ingredients: "at best incomplete and at worst a phenomenological construct that accommodates the data" (see also e.g. Scott, 2018, for a sustained plea for "healthy scepticism", especially in light of various tensions and anomalies). (HOPEFUL REALISM) also seems a judicious stance vis-à-vis the underdetermination of theories accounting for Dark Energy phenomenology (Wolf & Duerr, 2024b; Ferreira et al., 2025; Wolf & Read, 2025)

[27] This stabilisation and mutual consistency are mirrored in another common name for the ΛCDM model: "the *concordance* model".

it; Λ is woven into the fabric of GR itself, rather than anything grafted onto it. This stance, championed by Tolman, de Sitter and Eddington, receives further plausibility from (MATH-Λ).[28]

The relationship between the Dark Energy hypothesis and cosmic inflation, as per (INF), is likewise best understood in terms of coherence; both hypotheses are interlocked, without one strictly speaking (or at least in any obvious sense) explaining the other. Peebles (1984), for instance, stressed that cosmic inflation (at the time almost exclusively pursued because of "the attractive inflationary explanation" (p.444) rather than hard evidence) and observational data would most naturally fit for a non-vanishing Λ.

In the same vein, the allure of a link between quantum field theory and Λ, (Q-VAC) is also best viewed as the prospect of coherence (a central indicator of pursuit-worthiness for both accounts in (PURSUIT-WORTHINESS)). *If* successful, a quantum field-theoretic determination of Λ would qualify as a glorious explanation of a cosmological parameter in terms of fundamental particle-physics—a prospect that would seem to also fit Janssen's original COI-reasoning. But because of both the egregious mismatch between those quantum-field theoretical calculations and the observational limits on Λ, as well as lingering scepticism over the validity of the arguments employed (see e.g. Rugh & Zinkernagel, 2002), the prospect of coherence would seem a more realistic promise, undergirding Λ 's pursuit-worthiness. The mismatch has intrigued physicists. They hoped (and hope) to use the possible link with fundamental physics as a way of "testing" speculative proposals (especially in quantum gravity and unified field theories). As in the case of Dark Matter (**§V.1.B**), the heuristic resources of those speculative proposals can be directly imported to those areas, where empirical investigations would otherwise be difficult.

V.3. Inflation

The third revolution in recent cosmology that we'll examine through the lens of COI*-reasoning is cosmic inflation, the postulate of a very early and short phase of the universe during which it

[28] Advocates of the virtue-economic account of pursuit-worthiness will add that as compared to *generalisations/extensions* of, or *distinct alternatives to*, General Relativity (see e.g. Will, 2018, esp. Ch.5) General Relativity with a non-vanishing Λ is the mathematically simplest option (see Wolf & Duerr, 2024b for details). The mathematical complexities of those alternatives tend to be formidable; their application even to standard tests of General Relativity in the solar system poses knotty challenges. The cognitive costs of pursuing General Relativity with a non-zero cosmological constant pale by comparison to those of pursuing most alternatives. That said, a non-zero cosmological constant creates more complexity (or, depending on one's viewpoint: structural richness) than standard General Relativity with $\Lambda = 0$ (see e.g. Belot, 2023).

underwent extreme growth (before segueing into the somewhat calmer expansion covered by the Hot Big Bang model). **§V.3.A** will outline the historical context of the physical problems that led to the development of cosmic inflation. **§V.3.B** will explicate how inflation fits in with COI*-reasoning.

V.3.A Enigmas of the Big Bang

Numerous observational findings during the 1960s (e.g., the predicted relative abundances of light elements, "cooked" in the heat of the early universe, the discovery of the Cosmic Microwave Background (CMB) as the remnant radiation of the universe's infancy, or the detection of novel bright radio sources, quasars in particular) firmly established the Hot Big Bang (HBB) model as the ruling paradigm (Kragh, 1996; 2007, Ch.4). Combining standard General Relativity and nuclear physics—both at this stage entrenched within mainstream physics—it postulated the universe's hot beginning in a "primeval fireball" (Peebles) and subsequent expansion (as predicted by Lemaître and corroborated by Hubble, see e.g. Nussbaumer & Bieri, 2009).

Specifically, the HBB model hinged on the assumption that on large scales (i.e., superclusters and beyond) the matter distribution in the universe is spatially homogeneous and isotropic. This is patently an idealisation: our universe has manifold structures that break that perfect symmetry—galaxies, galaxy clusters, etc. Understanding cosmological perturbation theory (which addresses the incorporation of matter inhomogeneities and anisotropies) and developing a quantitative model to calculate the growth of cosmic structure thus became key objectives in cosmology.

Cosmologists succeeded in constructing **phenomenological models** for the evolution of (linear) density fluctuations. Whilst reproducing observed structures, such models remained merely effective descriptions that "saved the phenomena". They shed no light on the origins of those inhomogeneities. Some of their key theoretical assumptions—that initial density fluctuations were adiabatic (i.e. compressed/diluted independently of its specific type of matter), normally distributed ("Gaussian"), and scale-invariant (i.e. exhibiting similar patterns at different scales)—rested on tenuous arguments. Peebles and Yu (1970, p.834) summarise the predicament: "the initial density fluctuations are invoked in an ad hoc manner because we do not have a believable theory of how they may have originated".

Observational progress also cast new conceptual issues into sharper relief (see Brawer, 1996 for details). Measurements of the CMB temperature in the late 60s revealed *just how* homogeneous and isotropic the universe was in the past. As Misner (1969) stressed, homogeneity and isotropy on such scales was puzzling: the astonishing uniformity in the CMB temperature holds over scales that couldn't have had any causal contact, according to the HBB model. That is, when we wind back the clock, the relic radiation is uniform across parts of the sky that were separated by distances larger than light signals could have traversed to causally connect these regions. This became known as the **horizon problem**. Whereas the HBB model had to *stipulate* suitable initial conditions as a priori input to account for this uniformity, Misner (1968, p. 432) enjoined cosmologists to attempt to "'predict' the presently observable Universe". Misner's clarion call was to strive for deeper explanations—a dynamical mechanism rather than a brute fact postulate of suitable initial conditions.

Dicke and Peebles (1979; see also Dicke, 1970) highlighted another puzzle that the observational situation had made poignant: the **flatness problem.** Empirical adequacy of the HBB model required *flat* spatial geometry. This in turn necessitated a delicate balance between the matter-energy density and the curvature term in the Friedman Equations, governing the HBB model's cosmological dynamics: even minute deviations from this equilibrium would rapidly amplify—driving the universe away from flatness; in the language of dynamical systems, spatial flatness represents an unstable fixed point. The requisite initial conditions look *suspiciously* fine-tuned.

Finally, the **monopole problem** was of historical[29] importance. It arose from so-called Grand Unified Theories (GUTs) in particle physics. These were field-theoretic attempts to unify electromagnetism, the weak and strong nuclear force—widely viewed as the *next* step following (and adopting the principles of) the successful unification of the electromagnetic and weak force, discovered by Salam and Weinberg in the 1960s (and experimentally verified by Glashow). Such GUTs generically predicted an abundance of certain hypothetical "relic particles" from extremely high-energy eras of the universe. (Most notably, these included so-called magnetic monopoles, i.e. particles similar to magnets with only one pole.) Albeit speculative, GUTs

[29] More recently, "(i)nflation became detached from the initial [GUTs] for several reasons. First, there are several independent ways to generate an effective period of inflation, that all utilize different speculative physics at high energies. Inflation was therefore thought to be a more general feature of high-energy physics, and not explicitly dependent on grand unification. Second, there is mounting evidence that grand unified theories are not true of our world, so a theory of inflation tied to grand unified theory is unlikely to be successful" (Koberinski, 2024).

induced the expectation that such particles should exist—in vast quantities. None (to date!) have been detected, though. How to account for this absence of evidence?

The foregoing quandaries of the HBB model weren't empirical defects sensu stricto: they weren't tantamount to *conflict* with observations. Nonetheless, the sense of explanatory inadequacy they generated is palpable: "(i)t is the existence of these problems which motivates the search for a modification of the [HBB] model" (Blau & Guth, 1987, p.530). With the advent of cosmic inflation, independently co-developed by Guth (1981) and Starobinsky (1980), a remedy for addressing this inadequacy seemed in the offing.

In a nutshell, cosmic inflation posits that in the very early universe the mass-energy density was dominated by a scalar field (the so-called "inflaton"), moving in a certain potential (whose form is motivated by particle-physical considerations, such as the Higgs mechanism) so as to effectively produce a *repulsive* gravitational effect. This led to a brief period of quasi-exponential expansion. During it, the size of the universe increased by a factor of (at least) ca. 10^{27}. After between 10^{-36} and 10^{-32} sec, the inflaton decays; its repulsive effect subsides. Standard matter and radiation now become dominant, and the standard-cosmological HBB model's evolution begins.

The horizon, flatness and monopole problem thereby find a dynamical resolution. Inflation allows the very early universe to be causally connected, before exponential expansion stretches distant regions beyond each other's causal horizons; *only today* do the regions we survey appear never to have been in prior causal contact. Inflation's exponential expansion also drives the universe *towards* spatial flatness, rather than away from it as in the HBB. *Given* inflation, we thus should not only expect that the universe is approximately flat; we should in fact expect the curvature of the universe to be *exceptionally close* to flatness. In the same vein, thanks to the enormous increase in the universe's size, relic particles (provided they indeed exist) are strongly diluted; very plausibly, their density is thinned out below the detection threshold.

Soon it was moreover realised that inflation could plausibly explain the origin of the initial density fluctuations; hitherto, as we saw above, only phenomenological models had been available. Within three years after Guth's publication, physicists rushed to apply cosmological perturbation theory and techniques from quantum field theory to inflation (see Smeenk, 2019 for details). Their results suggested that quantum-mechanical variations in the inflaton's value would cause inflation to occur at different rates across space. At the end of inflation, different regions of the universe might thus wind up with slightly higher or lower energy densities—imprints of the quantum fluctuations on the initial spectrum of density fluctuations. This tallied with the effective descriptions of earlier phenomenological models. Importantly, inflation also made a clear prediction: according to inflation, the power spectrum—the statistical distribution of initial fluctuations—should *slightly* (viz. at the percentage level) deviate from scale-invariance.

A number of satellite missions in the 2000s (especially the BOOMERanG experiment) confirmed the expectation of spatial flatness (to a high degree of accuracy) and the prediction of tiny deviations from scale-invariance. Later CMB experiments, such as WMAP and PLANCK, further refined these findings (Aghanim et al. 2020; Bennett et al. 2013; Guth et al. 2014; Linde 2015).

V.3.B Inflation and COI*s

Inflation offers "a particularly clear example of this style of reasoning [COI]" (Smeenk, 2005, p.257, fn.58). The monopole problem, the flatness problem, the horizon problem[30] and the seeds for structure formation described by phenomenological models are all traced to a common origin: the universe's inflationary epoch, with its stupendous growth spurt, elegantly accounts for these otherwise puzzling facts.

Strong ontological commitments towards inflations—the nature of the inflaton in particular—would indeed be rash[31]: we don't know *how* inflation is particle-physically realised; a glut of indistinguishable options exists (see, e.g., Martin et al. 2013; cf. Wolf & Read, 2025). On the other hand, some of inflation accomplishments arguably count as (empirically confirmed) novel predictions (Smeenk, 2017; Wolf & Duerr, forth., sect.7). These successes function as "reality

[30] Here, one should mention "the modern version of the horizon problem" (Baumann, 2022, p.139): not only near-uniformity, but also the more fine-grained structure "beneath": measured correlations between parts of the CMB that, according to the HBB-model, ought to be causally separated. Inflation accounts for these correlations, too (op.cit., Ch.4.2.4). How staggering inflation's ability to account for these correlations—arguably a *novel prediction*—is evocatively recounted by Guth (2004, p.9): "(w)hen my colleagues and I were trying to calculate the spectrum of density perturbations from inflation in 1982, I never believed for a moment that it would -+be measured in my lifetime. Perhaps the few lowest moments would be measured, but certainly not enough to determine a spectrum. But I was wrong. The fluctuations in the CMB have now been measured to exquisite detail, and even better measurements are in the offing."

[31] Here, we won't join the recent fray over the *evidential* status of inflation (i.e. how it fares within the context of acceptance), see, however, Guth et al. (2014); Ijjas et al., 2013, 2014 and Wolf 2024; Dawid & McCoy, 2023; Wolf & Duerr, 2024a., sect. 3 for philosophical commentaries.

checks" (ibid.), reassuring "safeguards against garden paths". (HOPEFUL REALISM)'s middle course therefore seems judicious (see also Scott, 2018, p.18 & fn.33). [32]

An inference to pursuit-worthiness, by contrast, is strong (see Wolf & Duerr, forth. for details). First and foremost in this regard is inflation's "compelling" (Turner, 2018, p.9) explanatory power—a major incentive for pursuit, on both models of pursuit-worthiness (§III.2). In fact, this motivation for pursuing inflation is widely endorsed in the physics community. In his authoritative textbook, Peebles (1993, p.394), for instance, concludes: "(a)t the time this is written the inflation scenario offers the only reasonably complete resolution of the puzzle of the large-scale homogeneity of the observable universe. […] The scenario thus certainly deserves close attention."[33]

Two remarks are worth emphasising. First, inflation not only promises to explain *several* puzzles.[34] By invoking an underlying mechanism, inflation provides the first *explanation proper* for each of them (cf., e.g., Guth & Steinhardt,1984): "although the standard [HBB] cosmology can *accommodate* these facts, it cannot *explain* them. The observed regularities would be due to primitive regularities in the initial data or to remarkable fortuitous results of random processes, and this does not constitute an explanation. We want a *dynamical* explanation, a theory that traces these regularities back to the operation of laws" (Maudlin, 2007, p.43, original emphases).

Secondly, and relatedly, the inflationary account is *explanatorily deep* (Wolf & Duerr, forth., sect.4; Wolf & Thébault, forth.): its explananda are modally robust under variations in physical possibilities. "(I)n the inflationary model *avoiding* the observed results requires the same fine-tuning of initial conditions, the same fortuitous results of random process, that the standard model needs to *accommodate* the phenomena. The inflationary laws render the results massively insensitive to changes in initial data […]" (Maudlin, 2007, p.45, original emphases).

[32] Despite regarding it as "the most important idea in cosmology since the big bang itself", Turner (2018, p. 9), for instance, qualifies its status: "inflation is not yet a well-formulated, complete theory, and there is no standard model of inflation. At best, we have a rudimentary description of inflation."
[33] Even staunch champions of inflation are most naturally understood as making a claim about pursuit, rather than acceptance. According to Guth et al. (2014, p.118), for instance, inflation "provides a self-consistent framework with which we may explain several empirical features of our observed universe to very good precision, while continuing to pursue long-standing questions about the dynamics and evolution of our universe at energy scales that have, to date, eluded direct observation".
[34] Besides those mentioned already, Guth (1997; 2004) adduces the size of the universe ("the number of particles, 10^{90} or more", op.cit., p.6), and "the possibility of explaining how the Hubble expansion began" (ibid.).

This goes a long way towards explaining why the universe, as we observe it, almost certainly had to be the way it is—arguably a beautiful fulfilment of Misner's dream. Lofty visions aside, inflation's explanatory depth squarely translates into reduced inductive-epistemic risk about the universe's particular initial conditions. Epistemic access to them—let alone their testability—is ipso facto extremely limited. Laws, by contradistinction, lend themselves more easily to it. Especially with respect to epistemic access, recall that the concrete models for inflation are motivated by high-energy physics.[35]

This last point is closely related to inflation's coherence with other areas of physics. Recall that this may be viewed either as (potential) coherentist justification, or, on the virtue-economic model of pursuit-worthiness, a cognitive benefit. Historically, the two original papers on inflation made some prima facie reasonable assumptions regarding beyond standard model particle physics (see Smeenk, 2005 for historical details): Guth (1981) considered phase transitions due to broken symmetries in GUTs, while Starobinsky (1980) considered quantum corrections to General Relativity. Over the years, the details of the relevant high-energy physics changed. Yet, inflation's links with particle-physics remain tight (Martin et al. 2013). For instance, both the so-called Higgs Inflation and Starobinski Inflation—two of the most promising inflation models—follow from minimal extensions of the standard model of particle physics, and General Relativity, respectively (op.cit., sect.4).

The coherence with other areas percolates to another boost for inflation's pursuit-worthiness: such inferential ties open up heuristic resources, and unleash synergies for further inquiry. Inflation brings together key concepts and techniques from quantum field theory and General Relativity—a form of (conceptual and methodological) unification (see Wolf & Duerr, forth., sect. 5 for details). In the parlance of the virtue-economic account of pursuit-worthiness, such readily available ideas lower cognitive costs: they encourage researchers to leverage them for new contexts—a pragmatically commonsensical way of husbanding available resources.

Prior to inflation, investigating the origins of density fluctuations as the seeds for structure formation was limited. Phenomenological models simply decreed initial conditions; they offered little further heuristic guidance. Inflation overcame that sterility: investigating the origins and

[35] This isn't to downplay that independent testability of inflation faces severe challenges (see, e.g., Smeenk, 2017; 2019; Koberinski & Smeenk, 2024; forth.). Our point concerns conceivable means of knowledge (pluralistically construed, as befits historical disciplines, see Currie, 2018, 2019) and fruitful further inquiry:

statistical properties of these fluctuations essentially reduces to the familiar quantum field-theoretic treatment of the harmonic oscillator (Baumann 2022, Ch.8), a modelling framework which physicists have under robust cognitive and heuristic control.

Cognitive pay-offs due to inferential ties also—and excitingly for our age of nascent gravitational wave astronomy—also pertain to primordial gravitational radiation. “Arguably the most robust and model-independent prediction of inflation is a stochastic background of gravitational waves” (Baumann, 2022, p.345, also for further details). Both the above-mentioned Higgs and Starobinski Inflation give specific predictions, plausibly within reach of the next generation of CMB experiments. Regardless of their outcome, they’ll teach cosmologists something about whatever physics might be lurking beyond the standard model—information not easily obtainable with current collider technology (cf. Fischer, 2024 on similar “no lose” situations and their manifest pursuit-worthiness). For example, detecting primordial gravitational waves in this region of parameter space would probe energy scales 10^{11} times higher than those probed at the LHC (Abazajian et al. 2016; Akrami et al. 2020).

In addition to exploring these ideas just beyond our current particle physics paradigm, inflation could grant insights into even more speculative ideas. Brandenberger & Martin (2013) and Martin & Brandenberger (2001), for instance, have suggested that inflation’s mechanism for gargantuan magnification might stretch even “trans-Planckian modes”, i.e. density fluctuations that originate in quantum gravity effects, to macroscopic scales. From such analyses, it might be possible to tease out powerful constraints on Planck-scale physics and quantum gravity (Burgess et al. 2021; Martin & Brandenberger 2003). This promises some of the few tangible empirical clues for quantum gravity. By the same token, the already mentioned primordial gravitational waves might represent “a rare example where the Planck scale—the quantum gravity energy scale—explicitly appears inside a quantity that is observable, not only in principle but also possibly in practice with current technology” (Cicoli et al. 2024, p.47).

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