The Discovery of the Expanding Universe: Philosophical and Historical Dimensions

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Abstract

What constitutes a scientific discovery? What role do discoveries play in science, its dynamics and social practices? Must every discovery be attributed to an individual discoverer (or a small number of discoverers)? The paper explores these questions by first critically examining extant philosophical explications of scientific discovery—the models of scientific discovery, propounded by Kuhn, McArthur, Hudson, and Schindler. As a simple, natural and powerful alternative, we proffer the "change-driver model": in a nutshell, it takes discoveries to be cognitive scientific results that have epistemically advanced science. The model overcomes the shortcomings of its precursors, whilst preserving their insights. We demonstrate its intensional and extensional superiority, especially with respect to the link between scientific discoveries and the dynamics of science, as well as the award system of science. Both as an illustration, and as an application to a recent scientific and political controversy, we apply the considered models of discovery to one of the most momentous discoveries of science: the expansion of the universe. We oppose the 2018 proposal of the International Astronomers' Union as too simplistic vis-à-vis the historical complexity of the episode. The change-driver model yields a more nuanced and circumspect verdict: (i) The redshift-distance relation shouldn't be named the "Hubble-Lemaître Law", but "Slipher-Lundmark-Hubble-Humason Law"; (ii) Its interpretation in terms of an expanding universe, however, Lemaître ought to be given credit for; but (iii) The establishment of the expansion of the universe, as an evidentially sufficiently warranted result, is a communal achievement, emerging in the 1950s or 1960s.

Keywords: Discovery, Kuhn, theory dynamics, progress, expansion of the universe

Contents

1 Introduction

Discoveries indubitably lie at the heart of science. Staple examples comprise a diverse array: the phases of the Venus, gravitational waves, Archaeopteryx, Pauli's exclusion principle, the Lascaux Cave Paintings, the double helix structure of the DNA, $E = mc^2$, complex numbers, isomerism, X-rays, pulsars, the theory of continental drift, HIV as the cause of AIDS, Lorentz attractors in chaos theory, the cosmic microwave background, Wiles' proof of Fermat's Last Theorem, or the Immortal Jellyfish's immortality.

Intertwined with the notion of a scientific discovery are major descriptive and historiographical, as well as normative and epistemological-methodological questions. A bit more in detail, it figures centrally in key facets of science: its internal dynamics, with discoveries viewed as the shining achievements of science (think of Human Genome Project, or the images of Black Holes) and hallmarks of progress (think of penicillin, or mind-boggling accuracy of discoveries in particle physics or high-precision cosmology), and several of its social practices, with honours (such as the Nobel Prize) heaped upon illustrious discoverers, and pedagogy, with scarcely any textbook or popular science story not revelling in the adventures and vicissitudes of science's heroes, discoverers.

But what exactly does "discovery" mean? What does it include, and what does the notion exclude? Should we regard the term as a passe-partout concept, applicable to anything that might preoccupy a person's intellectual/mental life, e.g. artistic inspiration, stumbling on a piquant photo of the Queen's manicurist, a solution to a crossword puzzle, an eccentric philosophical or theological brainchild, any scientific or technological break-through or what have you? Or ought we, at least in the context of science, to reserve the term for a more specific usage? If so, how to construe it? And how is it related to the various features of science?[1]

Given the ubiquity[2] of talk about discoveries in science, one is dumbfounded by the paucity of philosophical attention devoted to it (cf. Michel, 2022, especially sect.1&2): attempts at conceptually clarifying the notion are bafflingly rare. One might perhaps hope to find a more fleshed-out reconstruction of it and the role of discoveries in science in the loci classici—Popper's (1983; 2002a; 2002b) *Logic of Scientific Discovery*, Hanson's (1958) *Patterns of Discovery*, and Kuhn's (1996) *The Structure of*

[1] Nickles (2000, p.85) provides—without answering them—a list of salient questions: "(w)hat constitutes a scientific discovery? How do we tell when a discovery has been made by whom and whom to credit? Is making a discovery (always) the same as solving a problem? [...] May discovery require a long, intricate social process? Could it be an experimental demonstration? How do we tell exactly what has been discovered, given that old discoveries are often recharacterized in very different ways by succeeding generations? What kinds of items can be discovered, and how? Is the discovery of a theory accomplished in much the same way as the discovery of a new comet, or is discovery an inhomogeneous domain of items or activities calling for quite diverse accounts? Must a discovery be both new and true? How is discovery related to (other?) forms of innovation, such as invention and social construction?"

[2] Of late, the need for conceptual clarification has become especially acute. The notion of scientific discovery—or rather: the idea of an individual discoverer within the commonsense way of thinking about discoveries—has come under pressure in at least two regards.

First, we live in an era of Big Data science; research is often conducted within large-scale collaborations. Over one-third of the paper announcing the detection of the Higgs boson, for instance, is taken up by the author list (ATLAS, 2012). Papers that result from these collaborations are often referenced by the name of the collaboration, not the first author or PI. Can large collaborations as a whole be discoverers? Can present accounts of discovery make sense of the large scale and collaborative nature of much of modern science? Credit, such as in the form of awards, is often not given to these collaborations as a whole, but rather to a select few individuals. The Nobel Prize committee, in particular, limits its prizes to three people.

Secondly, the use of AI in science, leading to discoveries, raises the question of agency: whom to ascribe agency in the discovery to? Is the AI just a tool (with whom as its user?) or may it qualify as a discoverer sui generis? These intriguing questions lie outside of the present paper's ambit; we'll not be concerned with the question of attribution of a discovery—i.e. the identification of a discoverer (see however Clark & Khosrowi, 2022). It's clear, however, that these challenges to our received way of thinking about discoveries are grist for our mills: the notion of scientific discovery calls for re-examination!

Scientific Revolutions. Alas, one would look there somewhat in vain.[3] Relying on strong assumptions and being unduly restricted in their domain of applicability, the few recent examples (inspected in **§2**) turn out to be unpersuasive.

The present paper seeks to develop a more satisfactory, philosophical model of the notion in its multifarious dimensions, hinted at above: our goal is to provide an explication of discovery—in the sense of Carnap (1950, pp.5): a simple, intensionally and extensionally adequate, and fertile precisification.

The discovery of the expanding universe marks the birth of modern cosmology. The techniques developed to measure the expansion remain essential to cosmology. They drove the later, and only slightly less revolutionary discovery of our own times: that of "dark energy"—phenomena associated with the accelerated expansion of the universe (see e.g. Durrer, 2011). In spite of its profound importance—or quite possibly because of it—the attribution of credit for the revelation that the universe is undergoing expansion continues to be a topic of ongoing debate. Historical accounts of the discovery (e.g. Kragh & Smith, 2003 or Nussbaumer & Bieri, 2009) primarily impugn the received wisdom: both dispute the previously recognised discoverers, Edwin Hubble and Alexander Friedmann, and forefront the contributions of Georges Lemaître. Likewise, in 2018 the International Astronomical Union voted to rename the former "Hubble Law" (which characterises the present expansion rate of the Universe) to the "Hubble-Lemaître Law". This was done in order to honour Lemaître's role in the discovery. Yet, without a more rigorous philosophical conception of discovery, it's difficult to argue where the credit is due—or why getting the record straight even matters. The significance of this discovery, and the continuing debate over how to distribute credit, make the expanding universe a fruitful case for philosophical analysis.

We will proceed as follows. **§2** will critically examine the extant models of discoveries, found in the literature, beginning with Kuhn's account and continuing with philosophical elaborations to it. Having distilled from those analyses lessons about what a philosophical explication of the notion should achieve—and what to shun—we'll present and discuss, in **§3**, a natural and simple alternative, the "change-driver model". **§4** will apply it to the historical case of the discovery of the expanding universe—and juxtapose our model's verdict with those of the other accounts. The main findings of our paper are summarised in **§5**.

2 Extant models of discovery

Here, we'll take stock of the previous literature that provides an account of scientific discovery, critically examining in turn Kuhn's (**§2.1**), Hudson's (**§2.2**), McArthur's (**§2.3**), and Schindler's (**§2.4**). **§2.5** will cull general shortcomings of all existing accounts. This will cast into sharper relief the desiderata of a more satisfactory model, as sketched in **§3**.

2.1 Kuhn: discoveries and the dynamics of science

Kuhn's oeuvre (1962; 1977; 1996) is one of the earliest systematic explorations of discovery. Subsequent analyses, including those that we'll inspect in **§2.2-§2.4**, build upon his ideas. It's therefore apposite to commence our review of accounts of discovery with Kuhn.

Kuhn distinguishes between two types of scientific discoveries: "what-that" and "that-what" discoveries (in Schindler's (2015) evocative terminology). In **"what-that"-discoveries**, a theoretical framework (which, notoriously, Kuhn refers to as a paradigm) predicts—or induces the expectation of—a new fact or phenomenon: *what* is supposed to exist precedes the empirical confirmation *that* it exists. Classic cases of predictions fall into this class: "the neutrino, radio waves, and the elements which filled empty spaces in the periodic table" (Kuhn, 1962, p.761). By contrast, in **"that-what"-discoveries**, such as that of Uranus (observed by a number of astronomers, on multiple occasions, but eventually classified/identified as a planet by Lexell, see Kuhn, 1996, pp.115) or that of X-rays, the temporal order is reversed: a fact or phenomenon is observed in advance of its satisfactory conceptualisation within a theory or framework; thus the discovery isn't expected.

Each type impacts science in different ways. "What-that"-discoveries occur with a theoretical-conceptual framework in place (in Kuhn's historiographical model: within "normal science"[4]). They yield

[3] The otherwise excellent *Stanford Encyclopedia* entry (Schickore, 2022) is a case in point. As is Nickles (2000), despite raising the most pertinent questions (see fn.1 above)! An exception is Blackwell's (1969) largely neglected monograph, on which we'll draw for our account in **§3**.

[4] "(N)ormal science, an enterprise that, as we have already seen, aims to refine, extend, and articulate a paradigm that is already in existence" (Kuhn, 1996, p.122). In the parlance of Kuhn's postscript (op.cit., pp.174), successful "what-that"-discoveries figure centrally in the elaboration of the paradigm's disciplinary matrix.

"mere additions or increments to the growing stockpile of scientific knowledge" (Kuhn, 1962, p.763). By corroborating anticipations—expectations buttressed by other successes of the framework—they solidify and deepen knowledge. Usually, the assimilation of "what-that"-discoveries by the adopted theoretical framework requires the latter's further refinement, e.g. the development of new computational techniques, more detailed or de-idealised models, etc. Successful "what-that"-discoveries thus afford a "measure of progress" (ibid.), achieved within the theoretical framework.

Much more viscerally "that-what"-discoveries "act back upon what has previously been known [...]" (ibid.). They therefore have the potential for greater disruption or "subversion" (op.cit, p.5; 1962, p.362): "(a) discovery like that of oxygen or X-rays does not simply add one more item to the population of the scientist's world" (Kuhn, 1996, p.7). A "sense that something was amiss" (op.cit., p.56) dawns on the scientist—a mismatch between the received theoretical framework and the phenomenon. "That awareness of anomaly opens a period in which conceptual categories are adjusted until the initially anomalous has become the anticipated" (op.cit., p.64). The anomaly's "assimilation" or its "digestion" (Lakatos) by the framework—the phenomenon's conceptualisation—requires profound conceptual and theoretical re-adjustments in the existing theories. Such discoveries can thereby trigger scientific revolutions, a "qualitative transformation and quantitative enrichment of fundamental novelties" (cf. ibid.).

Kuhn's treatment of discoveries has a surprising, and indeed untoward, consequence: as far as "that-what"-discoveries (Schindler, 2015, pp.125) are concerned[5], "[...] any attempt to date the discovery or to attribute it to an individual must inevitably be arbitrary. Furthermore, it must be arbitrary just because discovering a new sort of phenomenon is necessarily a complex process which involves recognizing both that something is and what it is" (1962, p.762; verbatim: 1996, p.55). The recognition of what a phenomenon is isn't only a temporally extended process. According to Kuhn, it also involves a significant refashioning of the conceptual-theoretical background. Such a process tends to be convoluted, with contributions from sundry agents (cf. Arabatzis, 1996). In this regard, Kuhn makes three claims:

1. the absorption of discoveries is marked by a murky and prolonged conceptual-theoretical re-adjustment process;

2. the "distinction between discovery and invention, or between fact and theory" (1996, p.52) is "exceedingly artificial"—in other words: empirical discovery is theory-laden, with an inseparable mixing of theoretical and empirical elements;

2.* the (semantic) incommensurability of paradigms leads to the the impossibility of comparing theoretical (or, in conjunction with (2): scientific) claims couched in different frameworks, due to their radical shifts in meaning.

(1) usually complicates the unambiguous spatiotemporal localisation of a discovery act (and a discoverer). It's a practical hindrance for the working historian—not an in-principle impediment. Likewise (2) is merely a practical challenge: an enlightening comparison of theoretical descriptions that utilise different conceptual frameworks is typically a non-trivial task. It often requires judicious analysis;[6] occasionally even new conceptual-semantic or mathematical-formal tools need to be developed.[7] On both points, (1) and (2), we largely concur. With the more fine-grained taxonomy of discoveries, presented in **§3.2**, we'll show that these tenets of Kuhn's—which, given their fairly uncontroversial nature, we'll treat as philosophical insights—can be retained, without inescapably curtailing the ability to ascertain when and by whom a "that-what"-discovery was made.

For Kuhn, the clincher for this latter claim pivots on (2*). Recall that (for reasons we'll clarify shortly) conceptualisations for Kuhn are prerequisites for a discovery. As per (2), they are inseparably entrenched in a theoretical framework. Rivalling frameworks, however, differ so radically that those conceptualisations lack a common measure; semantically, they are mutually opaque. With their conceptualisations being incommensurable, talk of discoveries (both "that-what"-discoveries, as well as "what-that"-discoveries) that belong to, or inaugurate, different theoretical frameworks/paradigms is inane; a discovery is inherently framework-indexed, i.e. meaningfully defined only relative to a framework. In order to coherently talk about variegated discoveries in one breath, one must adopt the same

[5] "(O)nly when all the relevant conceptual categories are prepared in advance, in which case the phenomenon would not be of a new sort, can discovering that and discovering what occur effortlessly, together, and in an instant" (Kuhn, 1996, pp.55). In other words, "what-that"-discoveries often lack a significant time dimension, and do admit of unique identifications of a discoverer.

[6] For a successful example of such a comparison—in fact a rebuttal of one of Kuhn's explicit examples for (2*)—see Beisbart & Jung's (2004) analysis of pre-relativistic and relativistic notions of mass.

[7] For an example—again a rebuttal of one of Kuhn's examples for (2*)—see Fletcher (2019).

theoretical framework for their conceptualisation. As a historian, though, Kuhn is loath to single out any such framework; to privilege those of our present state-of-the art, in particular, would be tantamount to anachronism—a historiographical blunder. Consequently, a discovery remains eo ipso a perspectival construct. In short, Kuhn's incommensurability thesis strips the notion of a scientific discovery of an objective sense: for the historian—as a matter of principle—it's impossible to unequivocally study discoveries. This is surely a rebarbative conclusion (indeed one that Kuhn doesn't spell out explicitly). It hinges on (2*), Kuhn's incommensurability thesis. The authors discussed in **§2.2-§2.4** forgo it, and rescue an objective notion of discovery, by jettisoning the incommensurability thesis. Instead, they postulate a form of continuity (of varying strengths) between successful theories.

Blackwell (1969, p.86) rightly "(emphasises) that Kuhn does not intend to develop a theory of discovery as such. His is the wider question of the growth and development of science". Herein, we maintain, lies indeed the most important insight we glean from Kuhn's analysis: Kuhn highlights the connection between discoveries and the progressive dynamics of science; discoveries and the modus operandi of science, its historical evolution, are essentially interlaced. We regard this as a key lesson—one that other models of discovery don't sufficiently heed: the notion of a scientific discovery must, we urge (**§2.5**), be embedded in the dynamics of science.

This is precisely what Kuhn accomplishes with his distinction between the two types of discoveries (as Schindler (2015, p.124) rightly observes). Each plays a slightly different role within the dynamics of science (see especially Kuhn, 1962, pp.363). Decisively for this link, Kuhn ties the notion of a discovery to some kind of conceptualisation: "observation and conceptualization, fact and assimilation of fact, are inseparably linked, in the discovery of scientific novelty." (1962, p.762). "What-that"-discoveries presuppose a prerequisite framework; the discovery must, more or less, fit into its conceptual-theoretical resources. Any residual mismatch is the source of innovation: via modifications, additions, and/or refinements within the existing framework, one seeks to minimise that mismatch. Thereby, the successful resolution makes the framework more sophisticated, enhancing its accuracy, range of applications, versatility, etc. "That-what"-discoveries, by contrast, burst the confines of the old framework: relative to the latter, they are anomalous. The resolution must come from a new framework: if the tension between the existing framework and the discovery becomes too pronounced, it "(necessitates) paradigm change" (op.cit., p.58). The "that-what"-discovery has shaken, and led to a re-configuration of, the web of orthodox belief; it has thereby advanced science in a fundamentally more innovative way than its "what-that"-counterpart.

Later commentators on Kuhn (**§2.2-§2.4**) tie the notion of a discovery to a more demanding kind of prerequisite conceptualisation—problematically so (as we'll see). Our alternative model of discovery (**§3**) shows how Kuhn's insight into the intimate connection between discoveries and the dynamics of science can be salvaged—without subscribing to such a gratuitously strong link, nor without rendering the dating of a certain kind of discovery, and the identity of its discoverer, *necessarily* futile.

2.2 Hudson: Materially demonstrated base descriptions

On Hudson's (2001) model of scientific discovery, four criteria must be satisfied for the discovery of an object X (by an individual, the discoverer D):

1. **Base description**: D must provide what Hudson calls a "base description" for X. This denotes "a description of the object that suffices to identify it: something that satisfies this description is the object being considered" (p.77).

2. **Material demonstration**: D "succeeds in demonstrating materially that this base description is satisfied" (p.79).

3. **Novelty condition**: D "finds something which is novel relative to a particular social community [...]" (ibid.).

4. **Truth condition**: "The object described and materially demonstrated is, *in fact*, X [...]" (ibid., our emphasis)

Before delving into a critical evaluation, a few comments are in order. First, the level of detail and accuracy required of a base description "is not hard and fast" (p.78); it varies from (historical) context to context. Hudson deliberately refrains from further specification. What matters, he argues, is that given the knowledge available to D, a base description can fulfil two functions. First, it serves as a means of (re-)identification, as an indicator for X: "(t)o have discovered an object one need *only possess* *enough conceptual resources to recognize its presence in a fairly reliable manner*, and such resources are what base descriptions provide for us" (p.78, our emphases). The same dependence on D's context, the background knowledge of their time in particular, applies to the level of accuracy required of the material demonstration. A second function of the base description (touched on only in passing) is to instil in D at least an idea of her or his discovery's significance: "(Hudson) (takes) the act of discovery to be a reflective event, one that impresses itself upon the discoverer as significant and informative" (ibid.).

Secondly, Hudson (p.81) stresses "[...] the contextualized nature of the novelty condition: novelty is socially dependent [...]. Thus, priority will sometimes depend on who gets included in the relevant ambient social group." Note furthermore that for Hudson, relative to the relevant social group, the novelty condition is a temporal absolute, detached from social realities: it doesn't require that D's work and priority actually be recognised.

Thirdly, how do commonplace discoveries (of, say, the watch of one's grandfather in the attic) differ from scientific discoveries? According to Hudson (op.cit., pp.81, our emphases), "(s)uch *commonplace items are familiar* to us and well-understood, whereas *scientific kinds on their initial discovery are quite unfamiliar* and sometimes even exotic in their properties [...]. What will count as an adequate base description or material demonstration will be less certain and so more contestable. [...] We are admitting that scientific discoveries are *not of a different kind* than commonplace discoveries, but *differ only in the order of complexity* regarding the sorts of base descriptions that are used and the sorts of material demonstrations that need to be performed" (pp.81).

What to make of Hudson's model? Although the model has some useful features, our overall verdict is to dismiss it as untenable.

Commendably, the model aspires to a natural desideratum: unlike Kuhn's model, it seeks to clearly label individuals as discoverers. This practice, together with the usually attendant bestowal of honours and rewards, belongs to the social aspects of the notion (as we'll elaborate in **§3**). It's desirable that a philosophical explication reflect them.

One may however question that the desideratum is successfully realised: Hudson's model asks for both too little and too much. First, Hudson's flexibility towards what a base description must deliver exacts the price of vagueness. This isn't merely troublesome in its own right. It also prompts a quandary: is there a limit not only to how accurate in detail a discoverer's base description must be, but also to how much inaccuracy (or how much falsehood in the description) is still tolerable (cf. Schindler, 2015, p.128)? Analogous worries can be voiced about the material demonstration. Hudson's gestured-at minimal answer—that the base description and material demonstration should suffice to rule out a sufficient number of relevant alternatives (p.78)—is glaringly hand-waving. Moreover, it flies in the face of both contemporary and historical queries (e.g. Laudan, 1981; Psillos, 1999, Ch.5 & 6; cf. McArthur, 2011, sect. 3). Amongst the latter, the luminiferous ether stands out as theoretically indispensable prior to 1905, and likewise amply "materially demonstrated" (see e.g. Torretti, 2009). Hudson doesn't take up this historical challenge (the challenge that, as expounded in **§2.1**, led Kuhn to his incommensurability thesis).

Contemporary challenges stem from the usage of idealisations, and stark simplifications, omnipresent in science (e.g. the thermodynamic limit, in which both the number of, and volume occupied by, the constituents in an ensemble of gas atoms are taken to infinity, or the Hardy-Weinberg model in population genetics, which assumes absence of both mutations and selection). Such toy models are known to be drastic distortions—sometimes even physical impossibilities—of their target systems (see e.g. Norton, 2012; Luczak, 2017). Wouldn't reliance on them preclude, on Hudson's model, the making of a genuine discovery—contrary to widespread practice and intuition?

At the same time, Hudson's model asks for too much: why insist that a discoverer D possess *both* a sufficiently accurate base description and material demonstration? Sometimes, as in the case of gravitational waves or Bose-Einstein condensation, discoveries can be—prima facie—purely theoretical, with their material demonstrations (i.e. empirical confirmations) significantly delayed. Vice versa, as in the case of ultra-high energy rays in the cosmic radiation or dark energy, sometimes empirical discoveries precede an adequate "base description".[8] It strikes us as plausible to treat both kinds as *distinct* discoveries in their own right (as **§3.2** will elaborate).

A second defect of Hudson's model concerns its scope: given both Hudson's elaborations, as well as his examples, it's manifestly limited to the discovery of objects. This contravenes bona fide examples of discoveries concerning other types of entities: superconductivity (a *property* of certain materials), isospin (a quantum number related to a *symmetry* of nucleons), the photoelectric effect or seafloor spreading

[8] Bartels (2021, Ch.3.7) discusses the illuminating example of a phenomenological discovery, still awaiting its accurate "base description": Alzheimer's disease.

(a quantum mechanical and geological *process*, respectively). One may also be wary of the restriction to *material* objects (cf. also McArthur, 2011, pp.368): that implausibly precludes otherwise bona fide discoveries from the mathematical and theoretical sciences, e.g. the discovery of the Casimir effect of quantum field theory (op.cit., sect.4), the Bell inequalities in the foundations of quantum mechanics, or Turing's proof of the *Entscheidungsproblem*.

Thirdly, Hudson expressly allows for the continuity between commonplace discoveries and scientific ones. A modicum of continuity seems indeed an attractive feature of the model. Yet, his demarcation criterion strikes us as wrong-footed: familiarity—apart from being yet another problematically woolly notion—is too contingent a *psychological-subjective* condition to base the distinction between scientific and non-scientific discoveries on.[9]

Finally, Hudson rightly underlines the relevance of a discovery's social context. Via the novelty condition, Hudson's model indeed achieves an important desideratum: it rules out that, say, undergraduates in chemistry lab classes discover oxygen. Regrettably, his incorporation of a discovery's social dimension remains unsatisfactory. Hudson doesn't address the dilemma it entails: doesn't the community-relativity imply a form of relativism? What makes one community more suitable, or relevant, than another: one's academic school/tradition? one's tribe? one's country? one's language community? What about "independent researchers"? Hudson intimates awareness of the problem. As a remedy, he proposes that "(i)n scientific cases, one might argue that the ambient social group is the entirety of the human race, or more generally, all Earth-bound rational beings over the course of all time. But the group is not very likely any larger than this" (p.81, our emphasis). Such an expansion of the allegedly relevant community seems overblown. It surely detracts neither from the scientific value or significance nor the impressiveness of a discoverer's achievements, if in another part of the world her discovery had been made by someone else—so long as their two communities and cultures are sufficiently isolated. (They might not even know of each other's existence!) Accordingly, Hudson's novelty condition appears to be an unduly strong fiat, devoid of a convincing argument. In the same vein, Hudson's model precludes, by definition, simultaneous discoveries—a pervasive phenomenon in science (see Lamb, 1984).[10] Again, this consequence seems unduly prohibitive. It rubs against common practice (where simultaneous, independent discoverers are typically given *shared* credit).

2.3 McArthur: Discoveries for the structural realist

McArthur (2011) "(works) out some revisions to [Hudson's] account by drawing from a structural realist view of theory change" (p.361). Structural realists (of the epistemic stripe, see e.g. Votsis, 2020) contend that scientific knowledge primarily resides in the patterns and relationships amongst observable entities, information encoded in a theory's structural-mathematical content, its equations. Judgements about the nature of the entities that a theory appears to postulate, by contrast, ought to be suspended. Viewing its structural content as what is "essential" (McArthur, 2011, p.371) to a base description, McArthur relaxes Hudson's demands on the discoverer's epistemic grasp of it: a discoverer needn't possess an approximately accurate base description, as a scientific realist would interpret it (i.e. as a base description that, when read "at face value", is approximately true); instead, for McArthur, a discoverer must only identify the discovery's approximately true structural content. Rather than a "*literally*" (van Fraassen, 1980, esp. Ch. 1&2) approximately true base description, she or he must possess a base description that is structurally approximately true.

More in detail, for an individual D to qualify as the discoverer of an entity[11] X (an object, process, event, etc.), McArthur stipulates three conditions:

1. **Base description**: D must possess a base description of X (in the form of a minimal theoretical description/conceptualisation).

2. **Novelty condition**: D must be the first to provide such a base description.

3. **Structural adequacy condition**: This base description for X must be structurally adequate; its relevant structural content has already been corroborated.

[9]While perhaps the demarcation between science and non-science can't be drawn *rigidly* (cf. Laudan, 1996, Ch. 11), we don't believe that it lacks any basis other than a psychological-subjective hunch, such as "familiarity".

[10]It even has a wikipedia-entry of its own: `https://en.wikipedia.org/wiki/List_of_multiple_discoveries`!

[11]While McArthur isn't explicit about this extension of what kinds of entities can be discovered, our more permissive reading is buttressed by both his prime example, the Casimir effect, which isn't naturally interpreted as an object, as well as the structural-realist agnosticism about the nature of entities per se, and its epistemic restriction to relations and patterns amongst entities.

Many features—and shortcomings—carry over from Hudson's model. For our purposes, it will be instructive to zoom in on two.

First, McArthur rightly remarks that Hudson's model is predicated on "an inadequate account of theory change" (p.366): it presupposes an interpretation of the base description in scientific-realist terms, assuming that the base description deserves to be understood as (approximately) "*literally* true". Such a strong premise is, as we saw in **§2.2**, indeed pricklish. As a structural realist, McArthur bypasses that problem: structural realism posits a weaker sense of conceptual and referential continuity between past and present science. The structural content of empirically and explanatorily successful theories, structural realists avow, survive theory change.

Because of his commitment to structuralism, McArthur's proposal remains unconvincing, though. It sensitively relies on a specific (and as such controversial) position within the scientific realism debate. A more robust model of scientific discovery seems preferable. Here, we won't rehearse the general complaints about epistemic structural realism (see e.g. Psillos, 1999, Ch.7, 2001; Chang, 2003; Ainsworth, 2014). In the present context, one problem in particular stands out: structural realism is limited to theories admitting of a *sufficiently mathematised* form. But mathematised theories outside of the mathematical-physical sciences are rare! Should we therefore, as McArthur's model would seem to imply, deny the possibility of discoveries in, say, biology, psychology or medicine (e.g. the discovery of horizontal gene transfer, a vaccine for polio, false memories, or the discovery of mirror neurons)? This strikes us as eminently implausible.[12]

A second shortcoming of McArthur's model is inherited from Hudson's: it precludes (more) "purely" theoretical[13] or (more) "purely" empirical discoveries; on McArthur's account, both components must be present for a genuine discovery. This seems gratuitously austere: why veto—contrary to widespread practice—more purely theoretical (e.g. mathematical) discoveries, or more purely empirical-experimental discoveries as distinct, but nonetheless genuine, types of discovery?

Without such a distinction, one runs into an unpalatable consequence—even within the empirical sciences. Often, novel predictions draw on structural properties of a theory that haven't received independent empirical confirmation yet, even if other aspects of the theory have. The historical examples of Dirac's prediction of antimatter, or Einstein's predictions of gravitational waves, or the prediction of the Higgs boson, respectively spring to mind. All three antedated evidence by about half a century! It's natural to view these incontrovertible break-throughs as discoveries: they are novel features entailed by a theory which otherwise had already proven its mettle. However, the usual candidate (co-)discoverers would be barred on McArthur's model: the latter requires of a discoverer to be "the first to use some of [the theory's] *already-confirmed* relations to provide the base description of the effect that was later exploited in experiments" (2011, p.374, our emphasis).

2.4 Schindler: Discovery as essence identification

Schindler's (2015) model strengthens Hudson's account of discovery by demanding that the base description capture, at least in part, the discovered phenomenon's essence:

> A discovery of X requires **observing X or its direct effects** [...] and the **correct conceptualisation of those of X's *essential properties* that suffice (epistemically) to individuate X** at time t [...] (op.cit., p.132, emphases in the original).

For a discovery, the phenomenon's (materially demonstrated) base description—in Hudson's terminology—must contain at least some true beliefs about X's characteristic features, defining what X is. Moreover, a discoverer needs to possess enough knowledge of them such that, against her background knowledge, she can discriminate X from other conceivable entities with similar effects. Thanks to such robust partial knowledge about X, according to Schindler, the phenomenon has been sufficiently comprehended—both empirically (qua material demonstration) as well as theoretically (qua the identification of its (partial) essence)—to licence the title of a discovery.

Through the insistence on essential properties in the base description, Schindler tries to overcome what he perceives as the main shortcoming of Hudson's model (which Hudson (2001, p.78) in turn animadverts upon in Kuhn's)—the double "what-indeterminacy"-problem (p.128): how *accurate* does a

[12]For the mathematised sciences, an additional problem arises: several contemporary and historical theories contain inconsistencies (Vickers, 2013). Therefore, the deduction of a structurally adequate base description required of a discoverer can be knotty: *ex falso quodlibet*! For example, are ad-hoc methods for circumventing those inconsistencies acceptable in derivations?

[13]Curiously, despite initially proclaimed intentions to allow for "purely mathematical discoveries" (p.368), it's clear that McArthur's model remains limited to discoveries as results within the empirical sciences.

base description have to be for a discovery? And how much *in*accuracy or *in*completeness is permissible? The identification of some of X's essential properties is supposed to address the first; the epistemic background—enabling the distinction between X and alternative entities—is supposed to take care of the second question.

In our assessment of Schindler's proposal, we'll concentrate on his employment of essential properties.[14]

Schindler strongly invites a reading of his proposal at face value (see, inter alia, the literature he cites, see p.132, fn14): he seems committed to essences, as discussed in the metaphysics literature—that is, properties that define an object's nature or identity, and that the object can't lack without ceasing to exist. Regrettably, Schindler elucidates neither his understanding of essences nor the kind of essentialism he has in mind. In light of the sizable literature (see e.g. Roca-Royes, 2011; Bird, 2009; 2018; Ishii & Atkins, 2020 for reviews) on, and proliferating varieties of, essentialism, this omission makes Schindler's proposal tricky to evaluate.

It doesn't bode well, though. Already at a general level, three arguments militate against an appeal to essences in this strong sense.

First, one may simply shy away from such a controversial metaphysical notion. In fact, "it is not obvious how best to characterize the notion of essential property, nor is it easy to give conclusive arguments for the essentiality of a given property" (Roca-Royes, 2011, p.65). A fortiori, one may be leery of tying one's understanding of scientific discoveries—a high-level notion permeating scientific practice in all disciplines—to such a specific (and notoriously vexed) idea in metaphysics.

Secondly, accounts of essentialism typically stipulate a tight definitional link between essentiality and metaphysical necessity (see e.g. Ishii & Atkins, 2020, esp. sect.1-3). Necessity here is construed in terms of truth in all possible worlds.[15] Whether science has much to say about such modal speculations is questionable (cf. Norton, 2021). Accordingly, one may doubt the relevance of essentialism for understanding science in general, and scientific discoveries in particular. But even if one grants this relevance, an epistemological worry looms: how to ensure that science attains knowledge of essences? Schindler (implicitly) presumes that discoveries have *actually* been made in science. Given his account's commitment to essences, Schindler thus professes an epistemic optimism about science—the sanguine hope that it has uncovered at least some salient essences. Such triumphalism many will repudiate as panglossian.

Thirdly, essentialism sits uneasily with the special sciences, such as geology, biology, or social psychology. Due to their inherent complexity, fuzzy conceptual boundaries, variability and dependence on specific circumstances and interests of the inquirer, talk of essential properties for the objects of those disciplines faces well-rehearsed challenges. That, on Schindler's account, would compromise the possibility of discoveries in those disciplines. This strikes us as inordinately restrictive—as Schindler's (2015, sect. 4.1) own discussion of a bona fide discovery in geology illustrates, or more dramatically: the discovery of anthropogenic climate change (see Friedrich, 2022).[16]

Although clearly envisaging the foregoing strong metaphysical reading in terms of essentialism, Schindler also contemplates a weaker reading (p.132., fn14): "(a)nti-essentialists may plug in their preferred notion of natural kinds here." On this weaker reading, discoveries require the observation of X (or its direct effects), in tandem with a sufficiently correct classification of X's natural kind.

The world, its objects and processes, adherents of natural kinds opine, admits of natural groupings—a division into said natural kinds (see Dupré, 2000; Bird & Tobin, 2023; Brzović, 2018 for further details); the latter are supposed to carve nature at its joints. The most prominent proposal for such classification or categorisation in terms of natural kinds is indeed essentialism: natural kinds are anchored in (sufficiently similar) essences. Other approaches are metaphysically less fastidious and more pluralistic: they conceive of natural kinds as groupings on the basis of clusters of properties, i.e. family resemblance.

Again, Schindler's omission of further details hampers a deeper analysis of how these alternate ac-

[14] Since his account is intended as a debugged version of Hudson's (and Kuhn's) with respect to the "what-determinacy"-problem, it inherits several of their shortcomings. In particular, Schindler merely gestures at the link between scientific discoveries and the progress or growth of science (p.127)—to our minds, a crucial desideratum of any adequate model of discoveries. Note also how Schindler expressly brackets questions of priority, or any of the communal aspects of scientific discovery; his is "an account intended to capture the nature of discovery. It therefore does without any reference to people making the discovery" (p.132, emphasis in the original). We likewise regard this neglect of communal aspects as a defect of the model's adequacy (**§2.5**).

[15] Also Schindler explicitly endorses a (modal) definition of essences: "(p)resumably, there is a list of properties that are (metaphysically speaking) necessary and sufficient for the individuation of electrons"(p.132).

[16] Other examples are easily multiplied: think of the discovery of extremophiles, or Harvey's discovery of the circulatory system with the heart's function as a pump. Both significantly expanded our understanding of life, and the human body, respectively. Yet, one would be hard-pressed to argue why the properties of those discovered objects should qualify as essential in any of the standard metaphysical senses.

counts apply to his model. At first blush, though, it's far from clear that they are compatible with his further commitment to a "realist stance" (p.123). First, Schindler's invocation of essences, or natural kinds, is designed to underwrite an *objective* distinction between essential and accidental properties—properties that are necessary for a discovery's prerequisite conceptualisation, and those that aren't. One may wonder, however, whether groupings based on family-resemblance (rather than putative essences) can deliver that: instead, the taxonomic lines of demarcation tend to depend on the context (see e.g. Ruphy (2010) for stellar classifications, relevant also for our case study in **§4**) —and the *subjective* interests of the inquirers (as is frequently explicitly acknowledged in the non-essentialist approaches to natural kinds). Secondly, and relatedly, Schindler seems implicitly committed to semantic realism: his insistence on "*correct* conceptualisation" of a phenomenon (reiterated passim)—up to "at least some false and incomplete beliefs about X" (p.128)—is naturally read in realist terms (cf. Psillos, 1999, esp. Ch.1)—as an assertion about the (partial) literal truth of a theory's unobservable theoretical content.[17] Prima facie, however, such semantic-realist ambitions—whilst underwritten by essentialism—are at odds with a context-dependent classification and categorisation of the world into natural kinds.

2.5 General shortcomings

We'll now step back to compile a handful of general inadequacies of the foregoing models of discovery. By way of negative examples, they'll sharpen our sense of what a satisfactory model *should* achieve—paving the way for our task in **§3**.

First, the scope of the extant models is expressly **restricted to material objects**. As pointed out, this a priori limitation is unconvincing. Rather, it's desirable that an account of discovery not impose such constraints: not only material objects—but also properties, processes, events, etc. can plausibly be discovered. Moreover, in view of routine practice of nomenclature and the ground-breaking significance of some of them for science, likewise a limitation to material entities seems unduly narrow: also, say, physical laws, or mathematical theorems, can plausibly be discovered.

Secondly, all three post-Kuhnian accounts (i.e. McArthur's, Hudson's, and Schindler's) overtly evince **bias towards realism**: each presupposes substantial realist premises.

An alternative that isn't hostage to such—invariably controversial—assumptions would be desirable (especially for historians of science, see Arabatzis, 2001): a satisfactory model of scientific discovery ought not to depend on specific positions in the realism/anti-realism debate.

One reason for the pro-realist bias lies in warding off a consequence of Kuhn's account (of which the other three models are intended as improvements): Kuhn's incommensurability thesis spoils the objectivity of the notion of a discovery across major theory-change. One is then stuck with the catch-22 of having to choose between either an anachronistic theoretical framework, or of swallowing relativism about discoveries. Neither option is attractive. The extant accounts seek to dodge the dilemma by blocking the incommensurability by pro-realist assumptions. As we'll see in the next section, it's possible—and plausible—to renounce the former, without subscribing to the latter.

The pro-realist bias has, however, a second source of motivation. Its prima facie plausibility musters intuitions independent of one's philosophical penchants for scientific realism. One may likely want to eschew talk of the discovery of entities (e.g. ether drag, or the creation of mercuric oxide via calx), today discarded as scientifically obsolete. By the same token, the pro-realist bias is, we conjecture, also driven by the hunch that what makes a scientific discovery valuable is its enduring value: we appear to prize discoveries—and confer honours upon their discoverers (see below)—because they constitute lasting achievements, hard-won but solid insights. Such reasoning lures one towards an approach to discoveries that ab initio privileges our current understanding of science. Past scientific achievements would consequently have to exhibit sufficient continuity with it in order to qualify as discoveries. From a scholarly perspective, however, such a bias towards **triumphalist presentism** ("Whiggism", Butterfield, 1931) is disconcerting; it's a dubious historiographic doctrine.[18] It's desirable that a model of discovery overcome such a bias. The challenge is to achieve this whilst doing justice to the intuitions that drive the two arguments. (Our more fine-grained taxonomy of discoveries (**§3.2**) delivers that.)

Thirdly, the three philosophical accounts pay **insufficient attention to two key facets of scientific discoveries**:

[17] It deserves to be stressed that Schindler erroneously imputes such semantic realism to Kuhn (p.128): Kuhn is emphatically *not* a realist. With his (semantic) incommensurability thesis, and his rejection of a correspondence theory of truth, in tandem with his rejection of a model of progress in terms of increasing truth-likeness, it's difficult to see what a "*correct* conceptualisation" within Kuhn's thinking might even mean.

[18] And, in fact, the sustained target of Kuhn's writings (see Hoyningen-Huene, 2012).

1. **Their connection with theory dynamics**: how are discoveries and the advancement of science related? How do discoveries and science interact?

2. **Their communal dimension**, and the various social-institutional practices surrounding it: what makes discoveries scientifically valuable?[19] Why do textbooks extol the achievements of discoverers? What rationale—if any—underlies the idea of awards and other signs of recognition for discoverers?

Kuhn's account puts discoveries at the heart of his conception of the evolution and progress of science. The advocates of the other models reject Kuhn's conception of the dynamics of science. Nonetheless, they try to improve on his account of discoveries. Vis-à-vis this tension, they remain largely silent on (1), the significance of discoveries for the dynamics of science.

Those authors follow Kuhn, however, in setting aside the questions under (2). They explicitly elide the reward system of science in particular—with its premium on priority. Presumably, we surmise, such issues are relegated to the sociology of science—and viewed as a haphazard, messy, extraneous element of science, of little concern to epistemology.

This neglect of central features of discoveries is problematic: a satisfactory explication of the notion of a discovery, we contend, ought to reconstruct both a natural link between discoveries and theory dynamics, as well as between discoveries and the sociological dimension of science—a link that, ideally, dovetails the historical and current social reality of science.

Finally, the tension between the goal of emending Kuhn's model, and the rejection of Kuhn's larger framework for the dynamics of science spawns another problem (closely related to the preceding point (1)): **why** do Hudson, McArthur or Schindler **weld the notion of a discovery to the need for the discovered phenomenon's conceptualisation?** What speaks against (more or less) *purely* observational discoveries (of, say, sun-spots or place cells), or (more or less) *purely* theoretical-conceptual-interpretative discoveries (e.g. of the existence of quasi-stable orbits in dynamical systems within KAM-theory, or Dirac's proof of the equivalence between Heisenberg's matrix mechanics and Schrödinger's wave mechanics)? Within Kuhn's two-phase framework of science, the link is clear: no scientific result without a paradigm, either the received one, or a new and revolutionary one. But since those authors (rightly!) distance themselves from Kuhn's framework, why insist on a sufficiently detailed conceptualisation—and a fortiori a sufficiently accurate one!—as a sine qua non for a discovery? As we'll see in the next section, we agree that qua scientific results, discoveries must be integrated into the wider body of scientific knowledge; this requires *some* kind of conceptualisation. But we disagree with the extant models that this conceptualisation must necessarily be sufficiently "accurate" or "reliable".

3 The change-driver model: scientific discoveries as drivers of scientific advances

This section will canvass an alternative account of scientific discoveries, the "change-driver model". **§3.1** will introduce the model. §3.2 will present a more fine-grained typology of scientific discoveries which naturally arises within it. We'll then (**§3.3**) discuss the merits of the model . Finally, in **§3.4**, we'll inspect, and rebut, some prima facie challenges.

3.1 The change-driver model of scientific discovery

Combining ideas of Popper (1983; 2002a; 2002b) and Kuhn (1962; 1996), we propose to conceptualise discoveries in a way that puts them (again) at the heart of science's dynamics. Owing to its emphasis on how discoveries propel science, we'll dub our account the "change-driver model". It elaborates Blackwell's (1969) remark that "(t)he word 'discovery' is a rather generic term that includes quite diverse instances of the advancement of human knowledge" (p. 96). For that, "to be counted as a discovery, [a new idea] must be integrated in the accepted body of scientific knowledge. This acceptance may be friendly or disruptive, depending on the impact of the new idea on former views" (p.53).

A scientific discovery, we submit, is

1. a cognitive scientific result (observational or experimental finding, theorem, prediction, etc.)

2. that has significantly advanced the scientific field in question.

[19]Note the entanglement of this question and our above comments on the Whiggish bias of the extant philosophical accounts.

The objects of a scientific discovery, as per (1), are what scientists traffic in: empirical results (e.g. the detection of the cosmic microwave background, or the first observation of a quasar), proofs (e.g. Poincaré's discovery of the chaotic dynamical behaviour in the three-body problem of celestial mechanics, or Boltzmann's H-theorem), an application of a theory that yields novel predictions (e.g. the application of basic quantum mechanics and general relativity to white dwarfs or neutron stars), a novel formulation of a theory (e.g. Feynman's path-integral formulation of quantum mechanics, or the Hamiltonian ("ADM") formulation of general relativity), the articulation of a new and powerful conceptual or taxonomic framework (e.g. Mendeleev's periodic table), a new interpretation of a known result (as in the case of Special Relativity, see e.g. Janssen, 2002, or the various interpretations of quantum mechanics), etc. No a priori restrictions are imposed on what may in principle be discovered: discoveries can pertain to material or immaterial entities (e.g. exoplanets or axiomatisation of set theory), objects (e.g. archaea or elementary particles), structures (e.g. violations of CPT invariance), properties (e.g. superfluidity) or states (e.g. a false vacuum or Bose-Einstein condensation), or processes (e.g. photosynthesis or CNO/Bethe-Weizsäcker cycle).

We add the rider "cognitive" as a rough-and-ready further requirement that the scientific result pertain to science's *cognitive* aims: it ought to be related to, and further, understanding and knowledge. By contrast, the devising of, say, the telescope or the bubble chamber concerns more technological and practical aspects. It yields knowledge that informs practical applications. The guiding intuition here is that such applications don't per se expand our knowledge or understanding: pending further research, they can (but needn't) be used as tools for that. Sensu stricto, these *inventions* wouldn't count as cognitive scientific results.[20]

The second clause, (2) prescribes that a scientific discovery play a distinguished role for knowledge within a scientific field. In an unusual way, it contributed to scientific progress. Thanks to the discovery, our knowledge grew—either in a temporally more discrete form, a "leap" forward, or over a more extended period with continuous progress, culminating in a break-through result.

The significance condition demarcates not only discoveries from *non-discoveries* (such as successfully answering an exam question). It also demarcates scientific discoveries from *non-scientific* ones (such as discovering one's love for Rumi's poetry, or a wart on one's toe). We'll return to both points below.

In preparation of our discussion in **§3.3** and **§3.4**, it will prove profitable to adumbrate three themes already in the offing at this juncture.

First, science requires a scholarly community within which it operates: science has *essentially* a communal dimension—a profound Kuhnian lesson (cf. also Longino, 1990 and Solomon, 2001). Scientific discoveries, qua aspects of science, can't happen in isolation; they must conform to the organisational and normative structures of science as a community.

Secondly, the model doesn't impose any restrictions, neither directly nor indirectly, on the discipline in question. As far as we can see, the change-driver model can be applied to all fields in the physical, social and mathematical sciences.[21]

Thirdly, our second requirement, (2), refers to the advancement or progress of science. How to construe this? What is meant by the growth of knowledge, to which a discovery is supposed to redound? Lest our model be taken hostage to partisan views,[22] we'll adopt a pluralist and permissive attitude: science makes significant advances whenever a result has exceptionally expanded, deepened or rectified our theoretical or empirical knowledge. Knowledge can, of course, also grow through negative insights: we gain knowledge by figuring out which ideas don't work or which facts aren't the case. Hence negative discoveries are possible, as we'll elaborate below.

What counts as a *significant* advance lies on a continuum (more on this in **§3.3**). Moreover, we regard it as a question that historians of science must adjudicate: their scholarly expertise—rather than philosophical apriori reasoning—should apprise us of which developments in science were particularly influential and important. [23] To be sure, consensus on discoveries can't lay claim to being an infallible

[20] Nonetheless, the distinction between cognitive and technological results lies on a continuum (see e.g. Shaw, 2022). This parallels the blurry line that marks practical from theoretical knowledge.

[21] In principle, to our minds, nothing precludes advances in, say, history. Plausibly, historical knowledge can grow. Discoveries in history that seem to fit the change-driver model's characterisation concern the Rosetta Stone or the Cairo genizah.

[22] We use "progress" in a loose and permissive sense, with no specific commitment to any position in the debate (see e.g. Niiniluoto, 2019; Dellsén, 2019).

[23] NB: Any plausible account must necessarily rely on historical expertise, if one wishes to apply it to actual—and ipso facto *historical*—discoveries. Our model merely makes this reliance explicit. We thereby don't deny the objective existence of a historical record: historical events and data concern matters of *fact*. Their interpretation and grasp within a wider horizon of significance, however, will be mediated by historians' assessments.

authority: misjudgements (as evaluated vis-à-vis historical expertise) are possible. In line with Chang (2009, 2021), the change-driver model is compatible with a pluralist approach to the history of science.

Before unpacking the implications of the model, let's ponder: what does it mean to make *a particular* discovery?

3.2 Three types of discovery: the discovery of X

So far, we have only characterised what counts as a discovery in general: the model of **§3.1** delimits criteria for declaring a finding a scientific discovery. Often, however, we want to know something more specific about a particular discovery—say, of the neutrino or fractal geometry.

For particular discoveries, we must disambiguate what *exactly* the question is when asking about the discovery of X. Scientific results may be grouped into three broad types.[24] Accordingly, should such a result X significantly advance science, we can, on the change-driver model, understand "the discovery of X" in (at least) three different ways: [25]

1. ***Empirical-material* discovery of X.**
   The object of an empirical-material discovery concerns the physical-material reality of a phenomenon. To discover X in this sense means to discover the existence of a phenomenon which is referred to, or empirically adequately described, by dint of X. Here, X may be a (possibly even heavily) theory-laden term.

The discovery of various phenomenological laws (e.g. the Snell-Descartes-ibn-Sahl law of refraction, or Zipf's law of word distributions), the Doppler effect, tool-use in chimpanzees, or the Miller-Urey experiment are cases in point, as *relatively* theory-free observational regularities. An example implicating more theory-ladenness in an empirical-material discovery concerns that of gravitational waves: if one refers to them as "ripples of spacetime curvature", one relies on General Relativity.[26] The phenomena into whose discovery—in its physical-material sense—we inquire when asking about the discovery of "ripples of spacetime curvature" are gravitational effects that propagate in a wave-like manner.

2. ***Theoretical* discovery of X.**
   The object of theoretically discovering X is to discover X qua *theoretical* scientific result (i.e. as treated within a given theoretical context). X may be, for instance, a theorem proven to be entailed within a theory, the proposal of an (at least pro tem speculative) idea or theory as a solution to various problems, etc.

Theoretical discoveries—remarkable, cognitively impactful ideas "stumbled on" or laboriously arrived at within a theoretical framework, in a manner similar to empirical discoveries—include, for instance, the Nash equilibrium in game theory, the spin-statistics theorem in relativistic quantum field theory, super-symmetry (as a speculative proposal to solve, inter alia, the hierarchy problem, implement an aesthetically pleasing symmetry, and as a prerequisite for the consistency of string theory), singularity theorems as mathematical facts within relativity theory, or Weyl's discovery of the gauge principle (which laid the foundation for the standard model of particle physics, but was developed for a theory quickly discarded as empirically unviable, see e.g. O'Raifeartaigh, 2000).

3. ***Synthetic* discoveries about X.**
   Synthetic discoveries express a relation between X as a theoretical concept/conceptualisation and

[24]The taxonomy equally applies to negative discoveries. Null-results of violations of Lorentz invariance for high-energy particles, or the non-observation of magnetic monopoles (as predicted by typical grand-unified theories) are examples of negative physical-material discoveries. The non-renormalisability of General Relativity is an example of a negative theoretical discovery. And the coincidence of the speed of gravitational and electromagnetic waves is an example for a negative synthetic discovery about TeVeS (an alternative to General Relativity that predicted a small discrepancy, see e.g. Abelson, 2022).

[25]The taxonomy is in no problematic way tied to realism (except in its metaphysical—viz. anti-idealist—dimension, i.e. in regards to assuming a mind-independent reality—a benign assumption that few anti-realists will contest). Physical-material discoveries only rely on a sense of empirical adequacy, and its anti-idealist interpretation as reflecting *something* in a mind-independent, material-physical reality. Theoretical discoveries are inherently theory-relative/internal: they are made within a given theoretical framework, refraining from inferences about any putative external material-physical world. Synthetic discoveries needn't be interpreted in problematically substantive realist terms either: all they require are standards of evidential-epistemic support. Anti-realists (except for radical sceptics and relativists) typically don't deny the existence of such standards of theory appraisal vis-à-vis observational data. (They merely contest the realist interpretation of the theory in toto, i.e. also with respect to all of its theoretical claims.)

[26]Albeit the standard theory for describing gravitational physics, empirically indistinguishable alternatives exist that don't conceive of gravitational radiation in terms of spacetime curvature (e.g. Hohmann et al, 2020).

the world. They combine elements of the two other forms of discovery. To synthetically discover X means to discover that X, as a theoretical idea, latches onto something in the physical-material world: X can be successfully used to explain, model or accommodate certain phenomena. The synthetic discovery about X consists in demonstrating some epistemic-evidential warrant for taking X, and its theoretical framework, seriously.

Synthetic discoveries often take the form of corroborations (e.g. Brownian motion as a confirmation of the kinetic theory of heat, or Frisch's deciphering of the bees' waggle-dance) or falsification (e.g. the detection of neutrino oscillations as refuting the original assumption of the neutrino's masslessness, or the various forms of counterevidence to kinship selection). To deflect the suspicion that one here succumbs to realist temptations, we hasten to add that a synthetic discovery can be agnostic with respect to realism: the metaphysical quarrel between realists and anti-realists seldom stymies agreement on which theory to regard as the most explanatory or empirically best-supported (e.g. the standard model of particle physics, or the modern evolutionary synthesis). Synthetic discoveries concern such theory appraisals.[27]

The three types of discoveries are clearly distinct.[28] Theoretical results can be seminal, even ground-breaking—and thus, according to the change-driver model, discoveries proper (theoretical discoveries in the sense of (2), that is). Yet, their relevance for physical reality (and hence status as empirical-material discoveries in the sense of (1)) may remain (at best) controversial. Cases in point are Hawking radiation (i.e. the creation of particle/antiparticle pairs near a black hole that annihilate each other, resulting in the black hole's gradual evaporation) or the holographic principle (a conjectured map/correspondence between a higher-dimensional gravitational theory, and a lower-dimensional quantum field theory, exemplified e.g. by the AdS/CFT correspondence). Conversely, it's possible to have genuine physical-material discoveries whose adequate theoretical conceptualisation—their theoretical or synthetic discovery—is still pending.[29] A spectacular example of our times concerns Dark Matter (see Martens, 2021). A multitude of robust empirical phenomena is known—empirically-materially discovered by, inter alios, Kapteyn, Oort and Zwicky (see Bertone & Hooper, 2018). A plethora of possible models—each a theoretical discovery in its own right—exist to account for them. Notoriously, however, no conclusive synthetic discovery is forthcoming: evidentially-epistemically, contemporary cosmology and astrophysics are stuck in an impasse.[30]

This taxonomy cuts natural distinctions, tracking intuitively different kinds of discovery. The history of the electron with its manifold theories exemplifies this (cf. Norton, 2000; Bains & Norton, 2001): the "discovery of the electron" is a polysemous umbrella term that encompasses empirical phenomena (e.g. the results of Millikan's oil drop-experiment establishing the existence of an elementary unit of charge, or the photoelectric findings of Lenard's cathode ray experiments), theoretical triumphs (e.g. the natural incorporation of spin via Dirac's theory of the electron, or the development of Feynman diagrammes) and extraordinary evidential-epistemic accomplishments (e.g. the stunningly accurate prediction of the electron's gyromagnetic ratio in quantum electrodynamics). Each of these results—to name only a few!—was a ground-breaking discovery in its own right, tokens of the types of discovery (1)-(3).

A subtlety concerning theoretical and synthetic discoveries merits attention. Eis ipsis, they may involve potentially strong theoretical—that is, strongly theory-laden—assumptions about their objects; to describe the latter, they presuppose theoretical apparatus that can't be easily severed. Einstein's discovery of a static universe solution to his gravitational field equations (**§4.1**), or Gödel's theoretical discovery of general-relativistic time-travel (i.e. spacetimes with closed time-like curves), for instance, are

[27]Proofs-of-concept—preliminary demonstrations or experimental evidence that support the feasibility and in-principle viability of a scientific theory, hypothesis, or modelling approach, and that serve as initial validation that the proposed concept or idea has the potential to work—can likewise be plausibly interpreted as synthetic discoveries. They are particularly interesting in the present context because, owing to their preliminary nature, they don't lend themselves to a realist interpretation. Schelling's dynamical segregation model, which shows how collective discriminatory social facts can inadvertently emerge from small individual preferences and, in the main, individual tolerance, is a case in point (see Bartels, 2021, Ch.3.8). Likewise germane are psychological—and, in particular, psychiatric—disease patterns (op.cit., Ch. 3.7).

[28]Needless to say: insofar as, following e.g. Quine (1950), one disputes the analytic/synthetic distinction, a clear-cut distinction between empirical and theoretical distinction is likewise blurred at a fundamental level. Typically, though, in specific historical/scientific contexts at least pragmatically it can be fruitfully upheld.

[29]Caneva (2001, 2005) illustrates the relevance of the distinctions for several cases in the history of science: "[...] what a scientist is typically credited with having discovered often differs significantly from the way in which the scientist himself characterized his work" (Caneva, 2001, p.1).

[30]Cosmic inflation provides a nuanced example, worth pointing out because of its—with respect to our taxonomy—hybrid status. Historically, it started off as a theoretical discovery, as a common solution to a few well-known puzzling (so-called fine-tuning) facts of the inflation-less standard model of cosmology, before physical-material discoveries (most importantly related to the power spectrum of the cosmic microwave background) accumulated; inflation makes novel predictions that were subsequently borne out. This gradually shifted its status (see e.g. Smeenk, 2005, 2017). Yet, a *fully* satisfactory synthetic discovery—a sufficiently warranted, specific model of inflation—is still pending.

inextricably interwoven with the theoretical framework of General Relativity; likewise, a characterisation of the Higgs mechanism for explaining the origins of mass in particle physics can't be disentangled from the quantum field theoretical framework.[31]

An analysis of theoretical and synthetic discoveries harbours conceptual-semantic challenges. How similar to the theoretical framework in which X is described does X' have to be in order to qualify as an antedating theoretical discovery? How similar does the evidential-epistemic warrant for—how similar do the arguments in favour of—X and X' have to be for X' to qualify as an earlier synthetic discovery? Both problems are especially poignant when we phrase the question about the discovery of X in terms heavily leaning on *current* theoretical understanding, e.g. when talking about particles (see e.g. Falkenburg, 2010) or light (see e.g. Hentschel, 2023), concepts that have drastically evolved over the past century. As arguably typical of assessments of similarities, they depend on various contextual factors. This we also take to be the case here: the extent to which X and X' resemble each other conceptually-semantically and epistemically-evidentially can vary; it depends on the concepts in question (X and X', respectively), and the specific aspects of the comparison one inspects. Fortunately, in the case of the expanding universe these complications affect an analysis only marginally.

In conclusion, the question about the discovery of X requires specification of which sense of discovery (1)-(3) one is interested in; the choice typically determines the answer. Our analysis of the discovery of the expanding universe in **§4** will illustrate this.

Before weighing the benefits of the change-driver model, it is crucial to discuss the role of the discoverer. To which entity should we ascribe agency in the discovering process? Who or what makes a discovery— an individual or a group (e.g. the scientific community as a whole)? The model remains neutral on this: it is compatible with either option, as well as a case-dependent hybrid. While Clark & Khosrowi (2022) have convincingly argued against the received individual agent-view, we'll adopt a more permissive view that allows for case-dependent continuity: some discoveries are attributable to individuals, others to a collective, and yet others (in varying degrees) to both, as illustrated in **§4.3**. The model doesn't necessarily presuppose the existence of an individual discoverer. Rather, we'll treat it as an open question—one that historical analysis has to answer—whether one can be identified for the specific discovery under discussion.

3.3 Merits

Its simplicity and commonsensical ring notwithstanding, the model of discoveries as change-drivers is surprisingly powerful. It overcomes the flaws of the received models, synthesises several of their respective advantages, and effortlessly realises a number of further desiderata for a satisfactory explication of "discovery".

We begin with its most patent advantage: its inherent **connection with theory dynamics**. This is built into the model ab initio via the requirement (2): a scientific discovery must have contributed non-negligibly to the advancement of science; its significance should jut out against the epistemic "background noise". Discoveries are thus integral to the healthy—progressive—development of science. On the change-driver model, this link is essential. On the other models it comes out as either accidental, or dependent on potentially problematic further assumptions.

Merging the insights of Kuhn and Popper (especially in their contributions to Lakatos & Musgrave, 1970), scientific discoveries advance science in three major ways:

1. They *expand* our knowledge. Physical-material discoveries increase our factual-empirical knowledge about the world. Theoretical ones increase our theoretical-formal knowledge about, and understanding of, a given theory, its logical content, potential applications, relation to other espoused beliefs, etc.; more generally, they typically broaden our intellectual-conceptual horizon and enrich our conceptual-theoretical resources. Synthetic discoveries increase what we know (have epistemically warranted beliefs), and understand (gain cognitive control over, cf. McCoy, 2022) about the world.

2. No less importantly, discoveries in their various forms can also *confirm, corroborate or solidify* existing knowledge. Discoveries can deepen our understanding of a theory, both "internally", as well as with respect to *other* theories, and theory's empirical relation to the world. Thereby, they refine and improve the epistemic quality of our expectations and beliefs.

[31] In the case of logico-mathematical discoveries—a special case of theoretical ones—such as the discovery of fractal geometries, Lie groups, or complex numbers, the very notion of a discovery requires formulating a mathematical theory (Bartels, 2021, Ch.3.9).

3. Discoveries can also *challenge* existing theories and beliefs—by exposing problems and anomalies (as eloquently expressed in the quotes cited in Schindler, 2015, sect. 4.2; cf. Laudan, 1977, esp. Ch. 1&2 for a systematic elaboration). Theoretical discoveries can disclose problems, contradictions or even conceptual-logical inconsistencies. Physical-material discoveries can constitute empirical anomalies, or even refute theories. Synthetic discoveries of this ilk flag gaps in our knowledge, e.g. applications for which our established knowledge breaks down. The upheaval that scientific discoveries wreak—through revealing gaps, inadequacies, consistencies, conflicts with other theories etc.—calls for a revision, and/or extension of established beliefs. Challenging discoveries thus elicit innovation.

As a second major advantage, the change-driver model divorces the notion of a scientific discovery from the **realism/anti-realism debate**: the change-maker model remains neutral.[32] It doesn't rely on substantive premises germane to that debate. Compatible with a pluralist approach to progress, it's not tied either to any specific position regarding global notions of progress.[33] In particular, the model isn't tied to construing progress in terms of verisimilitude (i.e. increasing truth-likeness—the most common realist model for progress).

Due to its link with theory dynamics, the change-driver model provides a **unified account of "that-what" and "what-that"-discoveries**. Physical-material discoveries that precede their satisfactory conceptualisation and theoretical grasp, as well as "material demonstrations" of such conceptualisations—synthetic discoveries in our taxonomy—*both* qualify as discoveries sui generis. By contrast, owing to their insistence on a suitable "conceptualisation" (Schindler) or "base descriptions" (Hudson), Schindler's and Hudson's models only admit "mixed" scientific discoveries; *purely* theoretical or (primarily) empirical ones flout their requirements.

What these authors got right is a key point stressed (in a complementary way, with each lop-sidedly fixating on one aspect) by Kuhn and Popper: in order for an observational or theoretical result to acquire and fully unfold its scientific significance, it must cohere with our wider web of beliefs (see Blackwell, 1969, esp. Pp. 140 for details; also Bonjour, 1985 for an in-depth study of the broader epistemological point). We try to make sense of the discovery against the backdrop of our prior knowledge.

Note that the latter comprises *both* empirical-factual knowledge, as well as theoretical knowledge. Hence, "what-that"-discoveries—phenomena hitherto materially not yet demonstrated, but entailed by our background beliefs—generate no less a tension in the total web of beliefs than "that-what"-discoveries. Scientific efforts to confirm the former serve to alleviate that tension—just like efforts to fathom enigmatic "what-that"-discoveries, anomalies resisting comprehension within our old background knowledge. In rough approximation of Kuhn's account (**§2.1**), this process of integration happens in two principal manners: through more conservative inclusion in, or absorption by the existing body of background knowledge (with suitable, "small-scale", more cumulative modifications), or through larger-scale revisions in our theoretical background. The tension that triggers the subsequent integrative process is what Kuhn (1959) aptly calls "the essential tension" for science, one of the main motors of scientific progress (cf. Turner, 2018 for the specific case of cosmology). The urge to lessen the tension, and the attendant need for integration, whereby coherence is restored in our knowledge-*cum*-discovery, constitutes the unifying thread of all types of discoveries; it's the common function in virtue of which motley scientific results can be meaningfully subsumed under the umbrella term "discovery".

In the same vein, the change-driver model seamlessly subsumes ***negative*** **discoveries**—in contrast to the rivalling accounts: the non-observation of a predicted phenomenon (null-results e.g. for magnetic monopoles, or certain particle Dark Matter candidates), impossibility proofs (e.g. Gödel's incompleteness theorems), a counterexample, implausible consequences (e.g. "ghosts" in particle-physics, or dynamical instabilities), etc. Due to their negative content, such discoveries sit uncomfortably with the other philosophical models. The change-driver model naturally accommodates them. Negative findings of that sort not rarely are even more important than positive ones—as Popper never tired of haranguing about.

Within the change-driver model, the problem besetting the standard accounts that we monikered the **"Problem of Theoretical Continuity"** doesn't arise. We neither have to make strong and overly optimistic assumptions about what science can and does discover (or what current science *has* discovered) nor does the model privilege our present scientific knowledge as the arbiter of what to classify as a real discovery. The need lapses to postulate a strong link between the scientific discovery in its historical understanding, and the discovery as *we* interpret it.

What advances science is—with details and specific judgements up to the historians' discretion—a

[32]As a litmus test: an advocate of Turner's (2007) "Natural Historical Attitude" can embrace it.

[33]It's natural, but by no means inevitable, to adopt Laudan's (1977) view of progress as problem-solving.

historiographical question: whether, say, the observations indicating the existence of galaxies outside of the Milky Way impacted astronomy and cosmology must be investigated through historical studies. Our present knowledge and conceptualisation/understanding of discoveries stand in a loose and motley relationship to how the discoveries were understood at the time (see also Chang, 2003).[34] The change-driver model purges the notion of discovery from residual "Whiggism" in the other models: in judging what counts as a discovery, circumspect *history* of science—not necessarily, let alone exclusively, contemporary scientific understanding—matters.

The change-driver model is historically sensitive: to classify a scientific result as a discovery doesn't commit us to anachronisms; a result can be granted that status on terms natural to its historical context. It allows us to honour past theories' achievements (without indulging in Whiggism). The ether theory, for instance, entailed several genuine physical-material discoveries (see e.g. Janssen & Stachel, 2004), as did Bohr's model of the atom. In the same vein, we can recognise the various explanatory and evidential successes of now-defunct, but formerly rightly cherished theories, such as the caloric theory (see e.g. Chang, 2003) or the Steady-State Theory (see Kragh, 1996), as synthetic discoveries. [35]

An appealing by-product of this historical sensitivity is its modesty with respect to current, more speculative research: without (prematurely) taking sides, it's consistent and natural to speak of theoretical discoveries in string theory (e.g. in the context of what Dawid (2013) calls the unexpected explanatory coherence argument for string theory), of material-physical discoveries in MONDian phenomenology (see e.g. Merritt, 2021), or of—even if perhaps evidentially not (yet) compelling—synthetic discoveries in inflationary cosmology (see e.g. Guth, 2004).

The change-driver model of discoveries not only remedies shortcomings of the extant accounts. It also has several *additional* benefits.

What, according to the model, constitutes a scientific discovery lies on a **continuum**. Assessing the significance of a scientific advance (or a particular contribution to it) is ineluctably a somewhat vague affair. Typically, it hinges on a historian's particular perspective. Siding with a pluralist approach to history (and philosophy) of science, we don't consider this a particular problem. It simply mirrors the complexity of history of science, as well as that of science itself.

At the same time, continuity of discoveries doesn't divest the change-driver model of the ability to draw salient distinctions. The invocation of both advances and the significance of certain results does rule out intuitive cases. On the one hand, consider the case of students in curricular lab classes, or when hitting on solutions of their assignments: they don't make scientific discoveries. These cases don't advance our scientific knowledge (although ideally they ought to advance the individual students' knowledge); they serve training purposes. A similarly unambiguous classification is obtained for homely discoveries, such discovering that a marten has been nesting in the attic over the summer. These are *mundane, not scientific* discoveries; for the most part, they are irrelevant for science.

When it comes to *research*, the picture becomes less clear-cut. Thereby, to our minds, it gains in being realistic. Contributions can span a wide-range as far as regards advancing science; the significance of contributions lies on a spectrum. (Think of, say, nuclear fission vs. the recent discovery that viagra cures jetlag in hamsters.) The extent to which a particular result contributes to the advance of science—the extent to which the change-driver model's second clause is satisfied—is a problem that only historians of science (and[36] scientists themselves) are competent to answer. It's incumbent on them to reconstruct the development of science in a coherent manner; that reconstruction, then, substantiates the discovery's significance.

This continuum aspect of scientific discoveries, together with the change-driver's science-inherent link to the social recognition and reward system for discoveries (more on this below), also *explains* another phenomenon that, on the other models, looks puzzling—an apparently contingent, sociological fact: why *aren't* all scientific discoveries celebrated equally? In particular, why does the glory associated with a scientific discovery seem to be correlated with the scientific impact it makes? The first discovered exoplanet, for instance, made a big splash (with credit being assigned, discoverers' names being remembered, plenty of discussion and media attention, etc., see e.g. Winn, 2023). Now discoveries of exoplanets are a dime-a-dozen; the reaction to discovering a new one has calmed down. The change-maker model easily

[34]We don't deny that some continuity between past and present science can typically be identified (see e.g. Bain & Norton, 2002). Our point is merely, in line with its non-partisan position in the realism/anti-realism debate, that the change-driver model of discovery is independent of any *specific* continuity thesis.

[35]The latter case is illuminating because it illustrates how a series of synthetic discoveries—a complex and prolonged back-and-forth between positive and negative synthetic discoveries about the Steady-State Theory—ultimately shifted evidential-epistemic superiority from the Steady-State Theory to the Big Bang model.

[36]In the spirit of Chang's (2004, 2008, 2016) "complementary science", the distinction between scientists and historians of science is, at best, blurry. Historical analyses of scientific episodes can be, and often are, contributions to science.

explains this in a science-internal manner, vindicating the rationality underlying the social practice: a discovery is, in the main, celebrated according to its scientific value, its significance. The first exoplanet discovered validated a range of observational techniques, instrumentation, theory; determining its properties like mass, radius, composition, etc presented new problems to solve. By the 5,000th exoplanet, the flurry of activity following such a discovery has subsided; in terms of significance, scientific results of the same kind have diminishing returns. On the change-driver model, their status as discoveries pales—and concomitantly the occasion for accolades. For the other accounts, by contrast, the discovery of the first and million-th exoplanet are both discoveries. Nothing more, it seems, can be said—nothing in particular about the rewards meted out to the discoverer.

With its emphasis on the progress of *science*, the change-driver model has ab origine a **communal dimension**: science is essentially a communal endeavour (cf. Duerr, 2023 for a Popperian perspective). Accordingly, scientific discoveries are advances within, and for, a scientific community. This complements our above discussion of mundane and scientific discoveries: the marten in the attic—no matter how personally momentous—constitutes a discovery disconnected from the (any) scientific community.

An interesting problem case (other variants of which we'll elaborate in more detail in **§3.4**) concerns privately made discoveries, *potential* scientific discoveries, even of ground-breaking relevance—*had they had* broader impact (but didn't, say, because the discoverer prescinded from publishing her results). On the change-driver model, they fail to meet the criteria for scientific discoveries: as such they require the connection with the actual development of science; else they remain discoveries simpliciter. To our minds, this consequence is plausible: it follows from the communal and organisational structure of science, as an institutionalised practice, rather than an intellectual activity of isolated individuals (see e.g. Ziman, 2002).

The model's in-built communal dimension has two noteworthy corollaries; both are germane to blemishes in Hudson's account (**§2.2**). The first concerns the priority condition. Discoverers are usually assumed to fulfil it. But why demand that a discoverer be the first to make the discovery? Rather than stipulating by fiat, the change-driver model can, to some extent, *derive* it.

According to the model, temporal priority is, strictly speaking, inessential for a scientific discovery: what matters instead is that the result in question increases knowledge of the scientific community—and usually sparks off further developments. More often than not, this is going to be correlated with temporal priority. Nonetheless, the correlation doesn't always hold: unpublished work or publication in obscure journals may later be found to have presaged a claimed scientific discovery. We bite the bullet that adverse circumstances, or fatal decisions on the side of the researcher, can prevent such discoveries simpliciter from becoming scientific ones (or, if one prefers the modal paraphrase: they can prevent *potential* scientific discoveries from becoming *actual* ones).

Secondly, recall one of our quibbles about Hudson's account: which social group is supposed to be relevant for judgments of temporal priority? In light of the stunning scientific achievements of, say, Chinese science and the Islamic world the question becomes especially pressing (with worries about unacceptable eurocentrism looming). Hudson's model ended up with a slightly outré answer; the other models skirted the question altogether. The change-maker model, with its explicit community-relativity, embraces a pluralism of communities: the notion of a scientific discovery is always relative to a scientific community—i.e. relative to a local network of inquirers with strong information links and a somewhat institutionalised form of critical exchange, rather than to any *arbitrarily selected* social group, or (pace Hudson) to "all Earth-bound rational beings over the course of all time".[37]

Thanks to its in-built communal dimension, the change-driver model allows a rational reconstruction of the **normative aspects** surrounding scientific discoveries. We take this to amount to another score on the model's intensional adequacy.

First, let's wonder: what underlies the general appreciation of scientific discoveries? What makes them, in line with the prevalent attitude amongst scientists, valuable? Bracketing all normative issues, the extant accounts remain silent on that. The change-driver model, by contrast, gives a simple answer: it's arguably an intrinsic aim of science as an activity to advance—which, as per the second clause, (2), of the model (**§3.1**), is what scientific discoveries purvey!

Secondly, the change-driver model sheds light on the credit/recognition practices regarding scientific discoveries: discoverers deserve credit for having advanced science. The other models, by contrast, consign those practices to the contingent side of science, white noise of human hustle and bustle: they

[37]To be sure, insofar as science has always been global (see e.g. Poskett, 2022), any talk of local scientific communities remains, to some extent, an idealisation that belies the more complex historical reality; ideas and knowledge have (fortunately) always travelled across borders. Yet, this idealisation underlying orthodox historiography of science has, we think, value in at least a rough-and-ready sense. It's beyond the present paper's ambit to question it.

are traditionally banished to its "external history", not susceptible to further rational analysis—and maybe "of great interest to empirical psychology; but [...] irrelevant to the logical analysis of scientific knowledge" (Popper, 2002b, pp.7). Construing scientific discoveries as drivers of change yields a different verdict: these practices implement the "normative structure of science" (Merton, 1942; see also Ziman, 2002), the complex set of institutionalised action-guiding rules, aimed at the realisation of the aims of science.

Most directly connected with the importance of scientific discoveries, visible at the social level—in priorities disputes, and the general reward system (prestige, prizes, and eponymy in particular)—are two norms of that "ethos of science", the institutionalised "complex of values and norms which is held to be binding on the man of science" (Merton, 1973, pp.268). One norm is originality, or the novelty of insights. A premium on a scientific result's originality incentivises researchers to strive for innovation. In order to advance science, however, novel ideas in science must necessarily be accompanied by "communism" or "communalism": they must be made publicly available. Results can only unfold their potential for scientific progress if disseminated in the scientific community (last not least, for further critical scrutiny). Both norms, originality and communism, thus explain the credit/recognition system in a science-*internal* manner: those practices reflect the norms inherent to science; through their institutionalised implementation, they promote scientific progress. The change-driver model hence is not only tied to the social practices surrounding scientific discoveries. It also anchors them and their rationality in science itself (see Merton, 1957 for a detailed analysis along those lines): *given* the institutional values and norms of science, scientific discoveries are indeed worthy of applause.

Our account also facilitates the integration of discoveries into **science education**. Kuhn (1962, ch. 11) points out that science textbooks often present a simplistic and linear-cumulative history of scientific progress. In that Whiggish narrative, individuals make discoveries at specific times contributing to the steady, inexorable march of science towards its present-day climax. Such textbook mythology fails to capture the complexity of the history of science. Yet, despite the misleading nature of "great men of science"-histories, even Kuhn notes that identifying "heroes" is often expedient in science education (ibid. 139).

The change-driver model steers a middle course between the two extremes. On the one hand, although an element of presentism lingers in identifying who discovered the various features of *present* science, this needn't be the tidied-up "textbook science" Kuhn warns against. First, while we are happy to label individuals as discoverers, our account is also inherently communal. To be a change-maker is to change something about one's community; it's not an epiphany of, nor for, lonely geniuses (see also Michel, 2022, sect.2.5.2). Further, since it's the historian of science who has to identify the change-makers of scientific progress, the model has the flexibility to be as complex, rooted in historical context, and messy as required to convey the true significance of each discovery.

On the other hand, the model recognises that discoverers, who made exceptional contributions to the field, can serve as role models and exemplars (a point stressed especially by Duhem, see e.g. Stump, 2007). In line with Chang's (2021) plea for pluralism in the history of science, we are also sympathetic to using (without, obviously, distorting the facts!) historical figures and episodes to illustrate exemplary use of methods, ideas and practices that current science still employs. A presentist approach to history is a possible perspective amongst many. And even if their methods and practices have become outdated, those discoverers' attitudes, behaviours, and sagacity and other intellectual (as well as moral) virtues are of pedagogical/didactic interest. Teaching such exemplars is arguably indeed "crucial to maintaining the values of the institution of science—the specificity and unique character of knowledge it produces, for example" (Pestre in: Caneva, 2001, p.20, fn.59).

3.4 Problem cases and responses

We'll close our discussion of the change-driver model by inspecting potential problem cases for—prima facie implausible or repugnant consequences of—the model, and proffer rebuttals.

The fine-graining problem. Doesn't our account demote discoveries to fleeting surface phenomena that disappear once one looks into greater details? The more closely one zooms in on an episode in the history of science, the worry goes, the more continuity with earlier (and later) research is revealed—the more elusive a discovery's significance becomes. Consequently, the notion of a scientific discovery, as in the change-maker model, would seem to fade away into an artefact of a surface-level description, denuded of deeper substance.

Our response is that this fine-graining problem is a variant of the problem of special sciences (Fodor, 1974, 1991): the fine-graining problem presumes that the macro-historical details can be reduced to micro-historical details—that macro-historical descriptions lack autonomy as explanatory frameworks, and that they are replaceable by micro-historical ones. We reject this premise as reductionist (cf., for instance, Bunge, 2004). Macro-historical accounts in the history of science (or other aspects of history) are illuminating in a way that more fine-grained ones (insofar as they are even available) aren't; certain patterns are only visible at that higher, more coarse-grained level of description. This, to our minds, suffices to take these descriptions seriously. Scientific discoveries play a role in those descriptions, typically contributing to explanations of a discipline's historical trajectory. Accordingly, discoveries are legitimate "higher-level" concepts, with explanatory clout (cf. Dennett, 1991).

The omnipotent plagiarist. Take your favourite (bona fide) discovery. Imagine that whom you consider its discoverer actually plagiarised her or his result from an unknown original discoverer OD. The plagiariser effaced all traces of his crime (obliterating his victim, OD, from the face of history). The discovery subsequently made an impact on the scientific community—falsely giving credit to the cunning plagiarist P. Did P, on the change-driver model, make the discovery then–rather than OD? The answer is—no! For the chain of developments leading to the scientific advances originates *in OD's* discovery: P *learnt about* the result in question from OD, and then resolved to assassinate OD and erase all traces. OD made the scientific discovery—even though no one (apart from the plagiariser) would ever know. That history of science spuriously identifies the plagiariser P as the discoverer discloses no defect of the change-maker model. It merely reflects the fallibility of historical knowledge—an insuperable limitation of our general epistemic predicament.

Scientific discoveries amidst Orwellian totalitarianism. Consider state-enforced *négationnisme* about some particular scientific discovery, say in an Orwellian state where that discovery which gainsays the ruling ideology is effectively suppressed. Doesn't the change-driver model grant the powers-that-be the ability to determine what counts as a discovery? Isn't, in short, the change-driver model too sensitive to vagaries of political power?

We regard this merely a feature, not a bug of the model: what the scenario illustrates is that science can only flourish in societies with a modicum of freedom. Political power structures can prevent a scientific result from advancing science. Thereby the discovery is prevented from becoming a scientific one. What makes such an act of successful *négationnisme* so pernicious is less that it deprives a discovery from its elevation to a fully scientific one (or that it robs the scientists of their well-earned recognition). Rather, it's that the act damages science itself—by stifling its progress, viz. by obviating the possibility of discoveries. We descry no implausibility in this.

Swampman scenarios (cf. Davidson, 1987)**.** Someone, P, after laborious years of continued efforts, is just on the verge of making a great discovery—when a lightning hits them, annihilating all traces. A few seconds later another lightning strikes someone else P'. Instead of annihilating her or him, this time the lightning infuses in them—a flash of inspiration—all the requisite details for duplicating the discovery that the just-annihilated person was about to make. The discovery is disseminated, and makes a significant contribution to the advancement of science. According to the change-maker model, P' made a discovery—rather than P.

Should one demur at this conclusion? We don't think so. True—P' was lucky, whereas P was damn unlucky. Lamentably, that—the ineliminability of a quantum of luck for the heroes of science and, often, bad luck for those more tragic figures whose toils don't bear fruit—is a generic feature of science and scientific discovery (see also Copeland, 2018). Any model of discoveries faces this element of fate; the change-maker model is no worse off than other accounts.

The discovery of America. Imagine an ibn-Batūtta-like explorer E, embarking on a daring mission to map unexplored continents and conduct ethno-anthropological research.[38] As she lands on a populated continent never before visited by anyone from her culture back home, E diligently records the location, and shares her studies with her community. Now, does E genuinely discover the continent and its indigenous population? One might hesitate to answer affirmatively: the indigenous inhabitants are obviously well-acquainted with their own land and culture!

But this is the verdict of the change-driver model: the continent and its population *have* been discovered—a scientific result for geography and anthropology of E's scientific community. Recalling the communal nature of science, we find nothing objectionable in this consequence. While E's work might have not changed anything in the science of the native people (assuming that they practise science), it *would* impact E's scientific community.

[38] We can make the example even more vivid by imagining E to discover a scientifically highly developed exra-terrestrial civilisation on an exoplanet.

4 Illustration: the discovery of the expanding universe

The change-driver model purports to take history seriously. What better episode to apply it to in order to showcase the model's virtues than "one of the major milestones in the development of the science of astronomy during the last 100 years, [...] one of the founding pillars of modern cosmology" (IAU, 2018): that the universe expands. However, who—if anyone—made this discovery remains contentious. After reviewing the main historical developments[39] (**§4.1**) we'll here compare the verdicts of the extant accounts of discovery for the case of the expanding universe (**§4.2**). **§4.3** will apply the change-driver model's perspective.

4.1 Historical overview

The gates to modern theoretical cosmology were flung open in 1917, when—less than two years after completing his new theory of gravity, general relativity (GR)—Einstein applied it to the universe as a whole (see e.g. O'Raifeartaigh et al., 2017 for details). This was the first cosmological model, an exact solution of GR's field equations for the cosmos at large. In it, Einstein assumed, matter was distributed uniformly (i.e. homogeneously and isotropically), and the universe as a whole finite (i.e. its spatial geometry closed). Einstein's model quickly stimulated further investigations—the pioneering work of modern cosmology's founding fathers (amongst them, Friedmann, de Sitter, Eddington, and Lemaître, about whom we'll hear more momentarily).

Two peculiarities of Einstein's result are worth adverting to. First, Einstein's primary motivation wasn't to apply GR to a new domain—one where one might perhaps expect incisive discrepancies with Newtonian theory. He didn't intend his model as a realistic description of the universe, apt for empirical tests (nor was Einstein even particularly informed about the state of the art in astronomy). "Instead, Einstein's foray into cosmology was a final attempt to guarantee that a version of 'Mach's Principle' holds" (Smeenk, 2013, p.228), i.e. the idea that inertia is fully determined by matter. In that regard, de Sitter's work soon dashed Einstein's hopes.

Secondly, and of key relevance for our purposes, Einstein's model was, by fiat, static: the universe it represented stays the same across time. In order to balance out the gravitational attraction of matter, and thereby to achieve the demanded staticity, Einstein had to modify his original field equations of 1915—by including the cosmological constant term Λ (introducing a free parameter). "Although he originally treated this as only a simplifying assumption, Einstein later brandished the requirement that any reasonable solution must be static to rule out an anti-Machian cosmological model discovered by de Sitter. Thus [...] Einstein was blind to the more dramatic result that his new gravitational theory naturally leads to dynamical models. Even when expanding universe models had been described by Alexander Friedmann and Georges Lemaître, Einstein rejected them as physically unreasonable" (op.cit., p.229). In clinging to the static nature of the universe Einstein conformed to the prevailing consensus, rooted in the time-honoured doctrine of the immutability of the heavens[40]—and a tenuous observational basis in "the small velocities of stars" (Einstein, 1917, p.139).

Throughout 1916 and 1917, de Sitter propounded his own model of a homogeneous and isotropic universe (de Sitter 1916a; 1916b; 1917). In contrast to Einstein's model, it was devoid of matter; the model contained only a cosmological constant Λ. Likewise in contrast to Einstein, de Sitter "took his model seriously enough to study its observational consequences, as we will see" (Smeenk, 2013, p.244).

Over the next decade, Einstein's and de Sitter's models remained the only well-known relativistic models of the universe; most cosmological investigations focused on those two rivalling models. "The major conceptual innovation introduced by Einstein was the very possibility of a mathematical description of the universe as a whole, but it was not immediately clear what observational and physical content these abstract models possessed" (op.cit., pp.242).

De Sitter makes two points important for our story. First, unlike Einstein, he countenanced a dynamical universe as a physical possibility: "(i)n [de Sitter's model—in contrast to Einstein's] [...], if there is more than one material particle these *cannot be at rest*, and if the whole world were filled homogeneously with matter, this could not be at rest without internal pressure or stress" (1917, p.18, our emphasis). This wasn't quite tantamount to entertaining an expanding universe; the de Sitter model was generally considered a static solution. De Sitter's spacetime metric didn't contain a time-dependent scale factor

[39]We follow the presentations in North (1965, Ch.5-7); Smith (1983, Ch.5); Ellis (1989); Kragh (2007, Ch.3), and Nussbaumer & Bieri (2009), to which we refer the interested reader for details.

[40]It's imperative to appreciate how different the received wisdom about the universe was in 1917: recall that Einstein was writing two years before "The Great Debate" over the size of the Milky Way and the existence of "nebulae", i.e. extragalactic systems (see e.g. Smith, 1983, Ch.1-3)!

(describing the change of physical spatial distances across time). In 1922, however, Lanczos showed how the de Sitter model could, through a change of coordinates, be interpreted as an expanding universe with a (time-dependent) hyperbolic, spatial geometry.[41] That it actually wasn't a static model took a surprisingly long time to realise. The waters were muddied by confusion over artefacts due to coordinates (a common ailment of early work in relativistic physics, see e.g. Kennefick, 2007, Ch. 4&5).

Secondly, whereas Einstein adamantly spurned non-static models as unphysical, de Sitter (seconded, in 1925, by Lemaître) hailed the dynamical character of his model as an avenue for further research (e.g. 1917, p.28): might it, he mulled, explain the receding motion of spiral nebulae? Indeed, de Sitter (1917, sect. 7) predicted a distance-dependent redshift in the spectral lines of far-away objects. Using available data, he even calculated the effect. Moreover, also methodically, he "took an important step in suggesting that the movements of the spiral nebulae rather than the stars (which Einstein had focused on) should be used as gauges of cosmic structure on the largest scales. The nature of the redshift effect and the precise functional dependence of redshift on distance for the De Sitter model were both matters of substantial controversy for the following decade and a half" (Smeenk, 2013, p.249). De Sitter initially derived—erroneously—a *quadratic* dependence of redshift on distance. Later, Weyl (1923) claimed that if test particles were introduced on geodesics emanating from a common point in the past, objects would appear to recede approximately according to a *linear* redshift-distance relation. Introducing matter in a different manner would lead to different predictions. Improving on Weyl's result, Robertson (1928) confirmed the linear redshift-distance relation.

The prospect of predicting redshifts was enticing. De Sitter's model "soon became a foundation for further theoretical work, among both astronomers and mathematicians" (Kragh, 2007, p.136)—including Eddington and Weyl. In 1909, Slipher had begun measuring redshifts at the Lowell Observatory, taking the spectra of spiral nebulae. In 1917, he published his results: all 25 spiral nebulae exhibited quite large blueshifts or redshifts. The high radial velocities invited an interpretation as recession velocities, resulting from the (standard) Doppler effect. The redshifts, Slipher averred, may have been the result of the Earth's motion through space: the nebulae we moved towards would be blueshifted; the ones we moved away from would be redshifted. But by the early 1920s, the redshift observations of spiral nebulae "left little doubt that there was a systematic recession. [....] (F)rom about 1920 there developed a minor industry based on the [Einstein's and de Sitter's respective] models. It was predominantly a mathematical industry" (Kragh, 2007, p.136). The next milestone would be to build a bridge to physics, and astronomical observations in particular. "The question of the relationship between cosmology and the observed redshifts remained unresolved for a decade or so, for other reasons, because it was difficult to distinguish a cosmological redshift (the de Sitter effect) from gravitational redshifts and the Doppler shifts caused by relative motion" (ibid.).

For some time, the Einstein and de Sitter cosmologies were the only well-developed cosmological solutions to GR. Two papers, one in 1922 and the other in 1924 (as well as in a semi-popular book of 1923, in Russian), ventured further. In them, Friedmann disclosed a much larger class of solutions: its members corresponded to matter-filled universes that *expanded.* Friedmann procured nothing short of "a complete and systematic analysis of the solutions of Einstein's cosmological equations that went beyond earlier analysis" (Kragh, 2007, p.141). The Einstein and de Sitter model turned out to be special cases of a richer space of solutions; these solutions demonstrate "the possibility of a world in which the curvature of space is independent of the three spatial coordinates but does depend on time" (Friedmann, 1922, p.377). Specifically, Friedmann had solved Einstein's field equations of GR for open and closed homogeneous and isotropic universes with time-*dependent* radii of curvature. For different values of Λ, Friedmann explored the consequences of cyclical models: the universe, in those solutions, periodically expand and contract, undergo accelerated expansion, or contract back into a point. With Friedmann, dynamical cosmological models of GR were squarely on the table—as mathematical possibilities. Friedmann *explicitly* didn't connect his models with astronomical observations: "(o)ur information is completely insufficient to carry out numerical calculations and to distinguish which world our universe is" (ibid., pp.385-386). No references to astronomical data are included in Friedmann's papers. For Friedmann, his analysis seems to have been first and foremost a *mathematical* exercise (see also Kragh & Smith, 2003, pp.146). Did Einstein and others overlook Friedmann's work? Not at all: he, like most others, simply didn't attribute to Friedmann's non-static cosmological models any physical significance. Friedmann had ushered in an "unnoticed revolution" (Kragh, 2007, p.140). Still in 1927, firmly in the grip of the ruling paradigm of a static universe, Einstein unabashedly dismissed non-static cosmological models as, from a physical perspective, "tout *à* fait abominable".

[41] In his first article on cosmology (1925), Lemaître also introduced new coordinates that rendered the de Sitter metric dynamical (with vanishing spatial curvature, and an exponentially increasing scale factor).

The occasion for Einstein's aspersion was a meeting with Lemaître. It concerned cosmological models for an expanding universe. They had been the subject of an article by the latter a few months earlier (1927) that "a friend had made (Einstein) read" (Lemaître as quoted in Luminet, 2013, p.1625). Its agenda Lemaître had already foreshadowed two years earlier: "Eddington [1923] writes [...]: 'It is sometimes urged against de Sitter's world that it becomes non-statical as soon as any matter is inserted in it. But this property is perhaps rather in favor of de Sitter's theory than against it.' Our treatment evidences this non-static character of de Sitter's world which gives one *possible* interpretation of the mean receding motion of spiral nebulae" (Lemaître, 1925, p.41). Unravelling this thought, Lemaître's 1927 *opus eximium* "combines the advantages of the Einstein world and the de Sitter world" (Kragh, 2018, p.1334) in order to find a solution to the Einstein field equations that satisfactorily accounts for both the redshifts and the existence of matter. This led Lemaître to a cosmological model with a *time-dependent* radius of curvature—corresponding to an expanding, homogeneous and isotropic closed universe. In the main[42] Lemaître's formal results duplicated Friedmann's; to his work Lemaître was only alerted in the above-mentioned meeting with Einstein. The crucial innovation, as far as the physical thrust of Lemaître's paper is concerned, was his renunciation of the ruling paradigm of staticity: Lemaître—"in stark contrast to Friedmann" (op.cit., p.1334)—audaciously suggested that *our* universe *in fact* grew in size. "What distinguished Lemaître's paper over Friedmann's was in particular his analysis of the galactic redshifts and his clear recognition that they were caused solely by the expansion of space" (op.cit., p.1338)—that is, the realisation "that the redshifts were caused not by galaxies moving *through* space, but by galaxies being carried *with* the expanding space" (Kragh, 2007, p.144).

Theoretically, Lemaître derived an approximately linear relationship between the distances and recession velocities of extragalactic nebulae (i.e. galaxies)—in the form still used today (i.e. with the proportionality constant given by the "Hubble-parameter", the ratio between the rate of change of the scale factor, and the scale factor). He then connected his solution to astronomical observations. Relying on averaged data for the ratio between radial velocities (through data due to Strömberg and Slipher) and distances (through data due to Hubble), Lemaître calculated a rough estimate of the "Hubble-parameter". What, in other words, Lemaître thereby did accomplish was to empirically determine the proportionality coefficient of his theoretically derived linear distance-velocity relationship; he thus made a genuine prediction—that, once sufficiently accurate data were available, it would corroborate that linear relationship (roughly with the proportionality coefficient Lemaître had determined). By contradistinction, what he *didn't* do was to provide original empirical evidence for that relationship; he didn't, and couldn't yet, verify his prediction.

"Lemaitre's prediction of an expanding universe made no more impact than did Friedmann's work. On the contrary, his paper seems to have been almost completely unknown and to have received no citations from other scientists until 1930" (ibid.). The reasons plausibly lie in the relative obscurity of the journal, the *Annales de la Société Scientifique de Bruxelles*, in which he had decided to publish it (in French).[43] Through the hands of Eddington, three years after the paper's appearance, the tide would turn for Lemaître's fame. But first someone else would step into the limelight: the very person usually, but contentiously, credited with having discovered the expanding universe.

During the mid-1920s, Hubble had ascended to international renown as an astronomer at the world's most powerful telescope, the Mt. Wilson Observatory. His meticulous observations conclusively refuted the hitherto prevailing view that nebulae were part of the Milky Way; being too far away, the nebulae formed galaxies in their own right.

Hubble was thus well-prepared for the next problem he tackled—the redshifts of galaxies. As we saw above, both theoretically and observationally, a correlation had been postulated between redshifts and distance of extragalactic nebulae: they appeared to flee from earth the faster, the farther away they were. But no consensus existed as to the functional relationship—or even the reliability of such a systematic correlation between observed redshifts and distances (see also O'Raifeartaigh, 2020).[44] At a meeting of the Astronomical Union 1928, Hubble probably learnt from conversations with de Sitter and others how

[42] A qualification is due: Lemaître included thermodynamic aspects. "Whereas Friedmann had ignored the radiation pressure, it was an important quantity in Lemaître's more physical and ambitious project" (op.cit., p. 1338).

[43] Luminet (2013, p.1625) locates the "main obstacle to a larger diffusion of Lemaître's article" in the fact "that most of the physicists at the time, such as Einstein and Hubble, could not accept the idea of a non-static universe." According to O'Raifeartaigh (2020, p.9), "it is very plausible that the observational data cited by Lemaître in support of his model were not sufficiently robust to cause Eddington and others to consider expanding cosmologies."

[44] Especially noteworthy amongst these early empirical endeavours prior to Hubble was the work of Wirtz (1922, 1924). Despite too insignificant a sample size for further conclusions, he was able to demonstrate (upon suitable grouping of the data) a systematic positive correlation between redshift and distance (and interpreted this as a confirmation of de Sitter's model), see Duerbeck & Seitter, 1990 for details. A similar result was achieved by Lundmark (1924, 1925), who drew on Slipher's velocity measurements and his own distance measurements.

his previous work and expertise might be relevant for on-going work in theoretical cosmology. The study Hubble embarked on would, Hubble hoped, also enable a critical test between Einstein's and de Sitter's model of the universe.

A significant leap forward came with the discovery that Cepheid variable stars can function as 'standard candle' distance indicators: "a formidable obstacle had to be overcome before securing a relation between redshift and distance that would be convincing to the majority of astronomers: the accurate determination of the distances to the [spiral galaxies]. But in 1923 and 1924 this barrier was in part removed when Hubble discovered Cepheid variables in nearby spiral nebulae. Before Hubble's observations of Cepheids, astronomers had been restricted almost entirely to the crude distance indicators provided by the novae, the apparent luminosities and the apparent diameters of extragalactic nebulae" (Smith, 1983, p. 175).

Cepheids denote a group of stars with a variable brightness, pulsating with a well-defined, stable period. Leavitt (1908) (and Leavitt & Pickering, 1912) had shown that the length of a Cepheid's period is correlated with its intrinsic luminosity: the longer the period, the brighter the Cepheid. Together with Hertzsprung's (1913) calculation of the zero-point luminosity and calibration of the period-luminosity relation (by measuring the distance to Cepheids in the Milky Way through parallax), Cepheids could thus be utilised as so-called standard candles, i.e. as means for determining cosmic distances. Hubble hadn't observed Cepheids in every galaxy he had redshift data for. (Individual Cepheids are difficult to resolve at high distances, and don't occur in all galaxies.) So he calibrated other assumed standard candles, such as the brightest star in a galaxy or the total brightness of a galaxy, to determine the distance to galaxies without observed Cepheids.

Plotting the distances of the galaxies (measured by himself) against their velocities (essentially drawing on measurements by Slipher and Humason), Hubble (1929) "(established) a roughly linear relation between velocities and distances among nebulae for which velocities have been previously published" (p.173). "(M)any commentators have noted that the quality and quantity of the data shown on Hubble's graph only marginally supported his conclusion of linear relation between redshift and distance for the nebulae [...]. However, the graph marked an important turning point [...]" (O'Raifeartaigh, 2019, p.9). The data on which Lemaître had relied in 1927, in particular, was contaminated by significant uncertainties due to scatter. The linear redshift-distance relation dimly discernible in Hubble's data could at best count as preliminary: "the nebular distances were established using a method that was prone to large errors" (op.cit., pp.8). Hubble's subsequent paper, co-authored with Humason (Hubble & Humason, 1931) dispelled lingering scepticism. "The linear correlation between redshift and distance seemed to be even clearer than before and effectively ended the debate on the existence of a linear relation" (Kragh & Smith, 2003, p.150).

How did Hubble interpret the empirical finding? He never—throughout his life—unambiguously championed its interpretation as evidence of an expanding universe (see op.cit., pp.151 for details). In 1929, Hubble had little to say on the interpretation of his data except that it could be a manifestation of the "de Sitter effect" (p.173): "(i)n the de Sitter cosmology, displacements of the spectra arise from two sources, an apparent slowing down of atomic vibrations and a general tendency of material particles to scatter. [...] The relative importance of these two effects should determine the form of the relation between distances and observed velocities" (ibid.). No mention is made of an expanding universe. Also later on, Hubble remained, at best, agnostic about that now-standard interpretation; deferring its interpretation to others (as he expressly wrote in a letter to de Sitter, quoted in e.g. Luminet, 2015, p.5), he was wont to underscore the empirical/phenomenological nature of the linear redshift-distance relation.[45] In line with this, Hubble "was not aware that the proportionality factor between redshift and distance, wrongly named the 'Hubble constant' was not a constant since it varies with time" (ibid.)—a variation that follows directly from interpreting the redshift-distance relation in terms of an expanding universe.

Hubble's paper fell on fertile soil. By and large, it had dawned on the astronomy community that Einstein's and de Sitter's cosmologies might be inadequate. The question of making sense of the redshift-distance relation became increasingly pressing. At the Royal Astronomical Society in 1930, Eddington went for the jugular: "why (should there be) only two solutions [i.e. Einstein's and de Sitter's models, both of which Eddington regarded as static](?) I suppose the trouble is that people look for static solutions" (p.39). Seconded by de Sitter (likewise present at the conference), he enjoined that dynamical solutions be looked into in earnest, as a solution of the redshift conundrum.

Reading the reports of this London meeting, Lemaître instantly apprehended that he had *solved* the

[45] Indeed, that interpretation ran into a chronic embarrassment: the age problem. As implied by that interpretation, given Hubble's measurements, the cosmos would be younger than the Earth (as inferred from radioactive decay) or the oldest known stars—a glaring absurdity (see also Grøn, 2018, p.13).

problem three years earlier in his paper. He immediately wrote to Eddington. Not only was Eddington the leading authority on relativity and astronomy, but he also happened to have been Lemaître's supervisor during his postdoctoral studies in Cambridge. In fact, a few years before, Lemaître had shared with Eddington a copy of his 1927 paper—which Eddington simply seemed to have forgotten about.

In his letter, Lemaître reminded Eddington of his paper. He also attached copies, requesting that they be circulated. Eddington—embarrassed by the oversight—obliged, enthusiastically endorsing Lemaître's results: not before long would he and de Sitter fully acknowledge that Lemaître had hit on "a brilliant solution" (Eddington, as cited in Kragh, 2007, p.147) of the redshift enigma—as a manifestation of an *expanding* universe. Eddington made sure that Lemaître's work would be brought to the attention of the world at the next meeting of the Royal Society, and moreover, he sponsored an English translation.

Having studied—this time carefully—Lemaître's (1927) paper, Eddington re-analysed Einstein's 1917 model. He showed that Einstein's corresponding solution of his field equations was in fact unstable, "like a pen balanced on its point" (Luminet, 2011, p.2914): a puny deviation from the delicately balanced matter distribution that Einstein had postulated would undo the model's staticity; any arbitrarily small perturbation would cause the resulting universe to either contract or expand. This instability argument was the coffin nail for Einstein's model: it exposed his static universe to require *unphysical* fine-tuning. By the beginning of the next year, 1931, even Einstein converted to the dynamical universe (see Nussbaumer, 2014; McCoy, 2020, esp. sect. 2 for details). "By 1933, the theory of the expanding universe was accepted by a majority of astronomers and subjected to detailed reviews [. . .]. It was also disseminated to the public through a number of popular works [. . .]" (Kragh, 2007, p. 148). While the expanding universe had become a reality, it provoked new questions: what drives the expansion? How to improve our cosmological—from now on: typically dynamical—models? And how to test them?[46] These would define the agenda of cosmology for the rest of the century until now (see e.g. Kragh, 2007, Ch. 4&5; MacCallum, 2015; or the contributions in Kragh & Longair, 2019).

Who, then, discovered the expanding universe? In 2018, the International Astronomical Union voted to rename the redshift-distance relation, commonly known as "Hubble's Law". The resolution recommends "that from now on the expansion of the universe be referred to as the Hubble-Lemaitre law". While the resolution passed with 78% of voters in support, there was some pushback (Kragh, 2018b; Grøn, 2018; Elizalde, 2019, 2020, Ch.5; O'Raifeartaigh & O'Keeffe, 2020). In part, it excoriated historical omissions and distortions (grievances that the above presentation, we hope, evades); in part, the criticism was based on different construals of the very notion of a discovery—without explicit attention to those construals themselves. The following subsections will fill this lacuna. To that end, we'll first (**§4.2**) apply the philosophical accounts discussed in **§2** to the expanding universe case, and then turn to the change-driver model (**§4.3**).

4.2 The view from extant accounts

With the historical material in hand, let's now apply the models of discovery (**§2.1**-**§2.4**) to the case of the expanding universe. Whom amongst our dramatis personæ of **§4.1** should we proclaim its discoverer? We'll argue: none! Those models rule out the most plausible candidates, or are simply inapplicable—for reasons *inherent* to those models.

A **Kuhnian** analysis is quickly dealt with.[47] The single most important candidate "what-that"-discovery of the expanding universe—a discovery where a phenomenon's anticipated existence and conceptualisation predate its empirical confirmation—was Lemaître's prediction of a linear redshift-distance relation. But since for Kuhn, discovery requires both a theoretical and an empirical component, the applicability of Kuhn's model falters: while Lemaître lacked the empirical data confirming his prediction, the person who provided the confirmation, Hubble (together with Humason), rejected the interpretation as a confirmation of the expanding universe (an epistemologically not unjustified reaction, as we'll elaborate below). Arguably the most plausible reaction, within Kuhn's model, is to regard the episode as a discovery with joint (or collective) discoverers, none of whom can individually be credited with the dis-

[46]This question is of special importance since alternatives to models of an expanding universe were studied (e.g. Zwicky's (1929) tired light hypothesis (expressly intended as an alternative explanation of the redshift-distance relation), Milne's (1935) special-relativistic cosmological model, or the Steady State Cosmology of Bondi and Gold (1948))—even if only as *minority* research programmes. O'Raifeartagh & O'Keeffe (2020, p.222) rightly point out that "the redshifts of the nebulae are merely one manifestation of cosmic expansion; other manifestations exist, notably the frequency range of the cosmic microwave background [...].

[47]We'll set aside a fundamental line of attack, criticising the applicability of Kuhn's model of theory dynamics to cosmology (see Marx & Bornmann, 2010; and O'Raifeartagh, 2013 for negative assessments, and Kragh & Smith, 2003, pp.143; Kragh, 2007, pp.243 for more positive ones).

covery.[48] The same conclusion is reached for the "that-what"-discovery of the expanding universe: while the researchers involved in the "anomalous redshift data"—chiefly: Slipher, Lundmark, and Hubble and Humason—lacked the theoretical framework for their findings; the person who provided that, Lemaître, lacked the empirical findings.

The Kuhnian perspective is overtly disappointing in its reduction of the rich nexus of discoveries, reviewed in **§4.2**, to the above black and white conclusion; it belies the complexity of historical science.

Let's move on to Schindler's model. For **Schindler**, to discover means both to observe an object X or its direct effects, and to conceptualise the essential properties needed to identify it.

A first stumbling stone to applying Schindler's model to the expanding universe concerns quibbles regarding the requisite ontological category of the discovered entity. Cosmic expansion itself isn't an object: it's a *global property* of the universe (as the largest possible physical system in its large-scale structure).

On the one hand, although Witz, Lundmark, Hubble and Humason have plausible claims to first observing the redshift-distance relation, this falls short of having discovered the expanding universe. None held the right conceptualisation—the interpretation of the phenomenological redshift-distance correlations in terms of an expanding universe. On the other hand, Schindler's model also disqualifies Friedmann and Lemaître: even though they possessed the right conceptualisation, neither had at his disposal a convincing material demonstration of the universe's actual expansion.

Perhaps one might reconsider Lemaître: after all, he understood the significance of Hubble's results as evidence for his previously identified conceptualisation. But also this option runs afoul of another problem: Schindler's insistence on essence-identification. We'd be hard-pressed to say how this condition would be satisfied here. Is cosmic expansion, or does it constitute, a natural kind? Does it have an essence? In which sense is expansion an essential property of the universe? As far as standard cosmology is concerned, any such invocation of essentialism seems a non-starter: universes *needn't* expand; in no sense is expansion necessary! Even as associated with a natural kind, expansion doesn't fit the bill. Relativistic cosmological models are arguably more naturally classified according to the spacetime (or spatial) curvature, i.e. geometrically. But there is *no* one-to-one link between a cosmological model's curvature and whether it represents an expanding universe.

On **Hudson's** account, discovering an object X requires being the first person to have an appropriate base description, and the latter's material demonstration; here, the base description is supposed to ensure—as far as we know—that whenever it's satisfied, X is present. Setting aside the limitation that Hudson's account shares with Schindler's (due to the account's focus on "objects"), let's parse out Hudson's criteria for the expanding universe. A natural base description of the expansion of the universe would seem:

> The distance between any two objects that are not gravitationally bound (and interact only negligibly in all other ways) increases over time.

Now what would have to be "materially demonstrated"?[49] Recall that for him (paraphrasing Hudson, 2001, p.78), "to have discovered the expanding universe one need only possess enough conceptual resources to recognize its presence in *a fairly reliable manner*." The material demonstration should confer sufficient empirical-evidential credentials upon the base description: when it's satisfied one should have sufficient reason to epistemically prefer X's presence *over alternative hypotheses*.

For two historical reasons this condition rules out the standard candidate discoverers: none of them had sufficient epistemic warrant for the base description's adequacy over alternatives.

First, the methodological status of general relativity, and a fortiori cosmological models based on it, until the 1950s was flimsy (Eisenstaedt, 1989): the theory could hardly count as well-tested, even on terrestrial or astronomical scales (with the available tests probing only minor deviations from Newtonian theory). The empirical evidence in its favour was, at best, inconclusive. On cosmic scales, general relativity was simply untested: "[...] (i)ndeed, it could be said that cosmology did not truly constitute a test for the general theory of relativity in these years [1940-1955]" (O'Raifeartaigh, 2022, p.13, original emphasis). With the age problem one had a puissant reason to be leery of models of an expanding universe: until the 1950s, the main ones entailed an age of the universe below that of the Earth and the

[48] Verbatim the same conclusion is reached for other "what-that"-discoveries concerning the expanding universe, such the predictions of the Alpher-Beta-Gamow theory.

[49] One may also wonder: since the expansion of the universe is a global claim about the universe at large, does one also need to materially demonstrate it globally? If so, given our necessarily limited observational access to a fragment of the universe, material demonstrations don't seem possible in cosmology. For the sake of the argument, we set aside this difficulty.

oldest known stars! Distrust in an expanding universe, insofar as it rested on general relativity's validity at cosmic scales, was thus *not irrational*!

Secondly, and relatedly, alternative explanations for the redshift-distance relation—the main empirical clue for an expanding universe until the 1950s or even 1960s—existed (see Kragh, 2017). Promptly following the publication of Hubble's paper, Zwicky (1929) "advocated thinking of the redshift as the result of an interaction between photons and intervening matter rather than cosmic expansion [...]" (Kirshner, 2003, p.11). In a gravitational analogue of the Compton effect, light loses its energy ("gets fatigued") through scattering on its journey from the source to the receiver. Zwicky indeed calculated a linear redshift-distance relation—effectively reproducing Hubble's phenomenological law—but construed now as an energy dissipation. Several variants of the "tired light"-hypothesis have been suggested (see e.g. Kragh, 2019, sect. 4.7). Admittedly, after WWII its popularity dwindled. Nonetheless, Kirshner (2003, p.11) concedes: "(t)he reality of cosmic expansion and the end of the 'tired light' has only recently [in the 90s and early 2000s!] been verified in a convincing way."[50]

Lastly—**McArthur's** account, a liberalised version of Hudson's, amalgamated with structural realism: McArthur allowed, as will be recalled, for discoveries even prior to material demonstrations—provided the discoverer harnesses *already well-confirmed theoretical* relations in deriving a base description of something not yet observed.

Like Hudson's model, the application of McArthur's model scuppers on the historical standing of relativistic cosmology: GR wasn't yet sufficiently well-tested. Accordingly, Friedmann's or Lemaître's derivations of the base description for cosmic expansion (i.e. the redshift-distance relation as a manifestation of the universe's expansion) couldn't rely on relations—GR's structural content—that would qualify as well-confirmed. NB: not even the third of the so-called "classical tests" of GR that is phenomenologically and explanatorily closest to the redshift-distance relation, gravitational redshift (i.e. energy loss of electromagnetic radiation in the presence of a gravitational field, manifesting itself in redshift) had been satisfactorily performed;[51] a convincing test of gravitational redshift would have to wait until the famous Pound and Rebka experiment in 1960.[52]

In conclusion, the three main models of discovery don't apply to the case of the expanding universe. Per se, we don't deem such a negative result—difficulties in identifying an individual discoverer—problematic. What *does* seem problematic is rather the kind of reason for those model's inapplicability: general *philosophical* assumptions about what science can and does achieve, i.e. something inherent to the model. Unproblematically, by contrast, applying models of discovery may fail through *historiographical complexities*: say, because many researchers contributed important elements or because the relevant theoretical and empirical developments took a long time, it may not be possible to single out one or several individuals.

4.3 Applying the change-driver account

The change-driver model yields a nuanced verdict on the discovery of the expanding universe. It identifies different plausible individuals for *distinct* tokens and types of discovery under that umbrella term. The two key advantages of the model become manifest: it not only does justice to the complexity of the science and its historical evolution but also aligns with the judgements of the main historical analysts.

4.3.1 Empirical-material discoveries of the expanding universe

The empirical-material discovery of the expansion of the universe begins with **Slipher** in the 1910s. His measurements suggested a curious empirical trend amongst faint spiral nebulae. In a list of velocities measurements (inferred from spectral displacements) published in 1914, "all but a few of the velocities were velocities of recession", i.e. moving away from Earth/our galaxy. "In addition, the radial velocities of

[50] For an assessment of an earlier verification—but still significantly after an expanding universe had already been elevated to the standard model in cosmology—see Norton (2023).

[51] While Adams' measurements of the star Sirius B, a white dwarf, in 1925 were viewed as (tentative) confirmatory evidence, a critical re-evaluation has deflated such a view (Hetherington, 1980; Holberg, 2010; see also Earman & Glymour, 1980 for fallacies and subtleties in the derivation and interpretation of gravitational redshift).

[52] A similar conclusion is reached, with a curious twist, for a slightly different base description for an expanding universe (for which we take our cue from O'Raifeartaigh et al.'s (2014) characterisation of Einstein's cosmological bias in terms of his preference for an unchanging—rather than static—universe. This is compatible with stationarity. Equating "expansion" with "evolution" or "variation with time", one may adopt the alternative base description: "The universe changes over the course of time, with its evolution being time-dependent; cosmic structures aren't stationary." In this case, a discovery of the expanding universe, on McArthur's model, wouldn't have been possible before the Big Bang Theory's main rival, the Steady-State Theory, had become empirically discredited, again in the 1950s or 1960s (see Kragh, 1996, Ch. 6&7).

the spirals were generally much larger than the radial velocities of the stars or gaseous nebulae. The very size of the spectral shifts prompted some astronomers to query the Doppler, or velocity, interpretation of the shifts in the spectrum of the spirals" (Smith, 1979, p.136). Slipher, in other words, empirically-materially discovered the phenomenon that (i) the distances between us and other galaxies systematically tend to increase, and (ii) the velocities corresponding to this apparent "recession" are *unusually* high (vis-*à*-vis the proper motion of stars and nebulae).[53]

This anomaly was quickly recognised. According to Eddington (1923, p.161), it constituted "one of the most perplexing problems of cosmogony". Its absorption by extragalactic-astronomical background knowledge enabled new computational techniques (primarily for calculating solar motion), crucial for further developments. Soon attempts followed to correlate the radial velocities of those spiral galaxies with other of their observable parameters, in particular, their magnitudes and diameters (in turn correlated with distance).

The next milestone in the empirical-material discovery of the expanding universe is the fruit of such endeavours: a positive correlation between the radial velocities and apparent magnitudes of the nebulae. Here it's difficult to attribute this result definitively to an individual. "As early as 1918 Harlow Shapley [...] had suggested that 'the speed of spiral nebulae is dependent to some extent upon apparent brightness, indicating a relation of speed to distance, or possibly, to mass', but he did not follow up this speculation" (Smith, 1979, p.142). Wirtz's announcement that "he had established a well-defined observational relation between the radial velocities and apparent magnitudes of the spiral nebulae [...] were not taken seriously by the principal students of the spiral nebulae" (ibid.); hence, not impacting the knowledge of the scientific community, the change-driver model disqualifies him, too, as a discoverer. Only once less crude distance indicators were available—Hubble's observations of Cepheids variables in nearby spiral nebulae in 1924—could progress be made. **Lundmark** may be credited with priority in establishing—albeit only tentatively—a relation between redshift and distance in his 1924 study. We therefore identify him as its empirical-material discoverer. In his 1925 paper on "the motions and distances of the spiral nebulae" he even determined the relation to be dominantly linear.

The establishment of this linear relationship concerns the third major empirical-material discovery relevant for our purposes. As Smith (1979, pp.147) writes, during the 1920s several anticipations of such a linear redshift-distance relation were made. "Hubble did not claim that he had found a linear velocity-distance relation [...]. Also Hubble, as he himself acknowledged, was working within a well-defined problem area, and before 1929 other astronomers had considered the existence of a redshift-distance relation. Hubble's success was not to 'discover' a relation; rather, it was to *convince* his colleagues that the relation was linear" (op.cit., p.133, emphasis in the original). On the change-driver model, the superior quality of the data, and the impact Hubble's (and Hubble and Humason's) work had for the body of astronomical knowledge—in particular the quickly recognised relevance for the theoretical models of the universe—**Hubble** should be credited with this third discovery, the firm consolidation of a linear empirical distance-velocity relation. It "ushered in a new area in cosmology by presenting a novel set of problems", producing "a dialectic between theory and data in studies of the large-scale properties that was undreamt of only a few years before" (op.cit., p.157).

This leads us to the other motor of that dialectic, theory—and the associated **theoretical discoveries**, i.e. theory-internal, formal results related to the expanding universe.

4.3.2 Theoretical discoveries concerning expanding universes

The first milestone is best jointly attributed to **Einstein and de Sitter**, right at the cradle of relativistic cosmology in 1917. Einstein may be said to have formally discovered that his original (1915) field equations, sans cosmological constant, didn't allow for a static solution—even if he *rejected* that conclusion. To forestall it, he resorted to an ad-hoc modification of his original theory via the cosmological constant. Einstein's (indirectly expressed) insight is complemented by de Sitter's discovery of his "solution B", the first non-static solution (even though its non-staticity was fully grasped only later). The object of Einstein's and de Sitter's twin formal discoveries were non-static cosmologies as theoretical possibilities within GR.

[53]Recall that Slipher's pioneering velocity measurements were indispensable for Hubble's (and Lemaître's) own later work; Hubble's ground-breaking achievement resulted from the combination of his own distance measurements and Slipher's velocity measurements. Interestingly, "(i)t is one of the great ironies of science that Hubble's measurements of distance were later substantially revised due to significant systematic errors. Due to an error in the classification of Cepheid variables, Hubble's cosmological distance ladder was later substantially revised by Walter Baade (1956) and Allan Sandage (1958)" (O'Raifeartaigh, 2013, p.8).

That they are generic possibilities is the object of the second group of formal discoveries. Friedmann (1922) not only derived the equations governing cosmological dynamics from general relativity, but also explicitly showed that cosmological models typically describe expanding universes. But given the delayed attention to his work, which—when it finally came—was mentioned in one breath with Lemaître's independent re-discovery, this discovery is, on the change-driver model, most plausibly attributed to *both* **Lemaître and Friedmann**, as independent co-discoverers. A second, impactful discovery in this second group is Eddington's (1930) explicit demonstration of the instability of Einstein's cosmological model. This exposed Einstein's static universe to be fine-tuned (as a mathematical property)—or extraordinarily special (and thus the opposite of generic).

A third group of formal discoveries revolves around the linear redshift/distance relation as a *consequence* of cosmological models. Here, simultaneously and independently of each other, **Weyl** (1923) and **Eddington** (1923) were the first to obtain the result in a solid mathematical way, even if it was limited to the de Sitter solution. **Lemaître** (1927) derived a more general result, for a generically expanding universe. A further refinement came a year later by Robertson: he derived the linear redshift/distance relation from the most general solution to Einstein's field equations, where matter is subject to the observational constraints of homogeneity and isotropy.

4.3.3 Synthetic discoveries of the expanding universe

What synthetic discoveries does the change-driver model highlight—i.e. results that reveal an evidential relationship between empirical phenomena—already detected or still-to-be-performed—and the notion of an expanding universe?

We can discriminate between two distinct types, negative and positive synthetic discoveries. Members of the former pertain to evidential or explanatory deficiencies, members of the latter, to positive explanatory or evidential achievements.

Negative synthetic discoveries comprise two clusters. The first is composed of the waxing realisation that redshift data pose growing and intensifying anomalies for the existing cosmological models. It begins with Slipher's redshift data, and continues with their solidification and the final emergence of a (linear) redshift-distance relation. The redshift data posed a perplexing challenge for static models of the universe: how to explain the trend towards redshift amongst galaxies? What sense to make of the high velocities (if one decided to translate, via an interpretation in terms of a Doppler effect, the redshift into velocities)? The redshifts weren't predicted or easily explained within Einstein's static model. The explanatory resources of de Sitter's model seemed more promising (based on minor positive synthetic discoveries, such as the various vague theoretical anticipations of a redshift-distance relation)—but the latter was patently unrealistic. Significant research efforts were invested into connecting de Sitter's model with observed redshifts. Neither Einstein's nor de Sitter's model had the resources to explain the redshifts without additional suppositions. This, in the main, drove Lemaître to explore an expanding universe. It's inherently difficult to identify individual discoverers for the insight that anomalies aren't naturally accommodated or explained within the existing cosmological models: different researchers must typically try out different ansätze—each of which turns out to be unsatisfactory. Plausibly, such negative synthetic discoveries are a **collective** achievement: the explanatory inadequacy of the extant models gradually dawns on the scientific community as a whole.

A second family of negative discoveries, often overlooked, concern—even after Hubble's establishment of a linear redshift/distance relation, and after the widespread acceptance of the expanding universe—the evidential shortcomings of cosmological models representing an expanding universe: that they were evidentially underdetermined, with alternatives (*later* to be refuted), plausibly counts a as significant insight that incentivised improved testing and the development of (unsuccessful) rivals (cf. for instance, Bschir, 2015).[54] Again, several researchers can be given credit for such discoveries.

On the side of ***positive*** **synthetic discoveries**, one outshines all others: the insight that a linear redshift-distance relation receives a natural explanation—or is in fact predicted—by an expanding universe. This insight can be attributed to **Lemaître**; he was the first to determine this empirical prediction of an expanding universe model, and identified the body of evidence that would, eventually, be used to confirm that prediction. Of course, only Hubble's result would corroborate it, and thereby actualise Lemaître's heretofore merely *potential* synthetic discovery. As likewise pointed out, only thanks to Eddington's interposition did Lemaître's discovery become a scientific discovery proper and advanced scientific knowledge within the physics community. Nonetheless, it was Lemaître's interpretation of

[54] Kragh (2019, p.120, also for a discussion of such rival theories) rightly stresses: "(t)hey belong to the history of the field as much as do the more successful theories that in a more direct way paved the way to the modern view of the universe."

the redshift data that drove the acceptance—and further development of cosmology—of an expanding universe.

Hubble was well-aware of that interpretation, but didn't commit to it. On the change-driver model, however, this—Hubble's personal convictions—isn't decisive for a synthetic discovery. To be sure, Hubble's work, as an observational finding, had a huge influence on the acceptance of an expanding universe in virtue of Lemaître's interpretation. On its own, it was an impactful empirical-material discovery—not a synthetic one: as a phenomenological law it didn't per se link the notion of an expanding universe and observational phenomena. Hubble may, to some extent, be credited with a negative synthetic discovery, however: the impactful insistence on the underdetermination of the expanding universe interpretation of the linear redshift-distance relation.

The importance of this negative discovery—perhaps even a matter of intellectual honesty—for the status of cosmology as an empirical science can't be overstated. Still in 1963, "there (were) only 2.5 facts in cosmology" (Longair, 1993, p.160): the darkness of the night sky (peripheral to developments in cosmology at issue here), Hubble's empirical redshift-distance law, and the half-fact, in the twilight since the first reliable source counts in the mid-1950s, "a matter of considerable controversy" (ibid.), concerning the universe's evolution, i.e. the idea that "the contents of the universe have probably changed as the universe grows older" (ibid.). Within a few years, cosmology multiplied the number of facts it could confidently point to. Important tests were invented and performed that confirmed the model of an expanding universe (see e.g. Longair, 2019). These developments in turn amount to (positive) synthetic discoveries.

5 Summary and conclusion

The present paper undertook a comprehensive review of extant philosophical models/explications of scientific discovery: how to construe the notion, and its link to other facets of science, especially its dynamics?

While Kuhn's account is compromised by its entanglement with his wider, and problematic, views on science (especially incommensurability), we discern the principal weaknesses of the more philosophical accounts in their unduly strong metaphysical and epistemological commitments (especially to realism), and their disconnect from both scientific change and the growth of scientific knowledge, as well as from normative-social aspects of discoveries (especially the valuation of discoveries as scientific goods and the concomitant reward system).

As an alternative that avoids these defects, we proposed the change-driver model. It conceptualises scientific discoveries as cognitive scientific results that have significantly impacted science, and advanced communal knowledge. In tandem with a more fine-grained taxonomy of forms of discovery—dependent on the kind of interaction between the discovery and existing knowledge—the change-driver model was shown to exhibit superior intensional and extensional adequacy over other models.

The case of the expanding universe further illustrated this, underscoring the model's fertility and relevance for a recent science-political debate. Our model yields a nuanced verdict on whom to identify as a discoverer of what—a verdict that does greater justice to the complexity of science and its history than rivalling models. Chiming with other historical analyses, the change-driver model *opposes* the IAU's (2018) recommendation: while the empirical law in question—the correlation between redshift and distance of galaxies—ought to be attributed to Slipher, Lundmark, Hubble and Humason (rather than *just* Hubble, or Hubble *and Lemaître*), Lemaître (but not Hubble) deserves privileged credit for discovering its interpretation as evidence for the expanding universe. The discovery of the expanding universe as a broader claim—its wider theoretical understanding and epistemic backing—is best viewed as a more collective accomplishment, involving several researchers, an extended process that is on-going.

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