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The good old discovery-justification distinction: Remarks on Melogno's analysis of a Kuhnian account

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Received date: 2024-02-06 | Approved date: 2024-09-23

DOI: https://doi.org/10.17533/udea.ef.356239

Abstract: The discovery-justification distinction stands as a pivotal issue within 20th-century philosophy of science. It subtly underpins many foundational topics and concepts pertinent to our comprehension of knowledge. Thomas Kuhn's contributions are indispensable in this regard, with his critiques playing a pivotal role in shaping both his initial model of scientific progress and its subsequent revisions. Kuhn addressed this dichotomy head-on in the first of his Thalheimer Lectures, presented in 1984. In this paper, we revisit Pablo Melogno's (2019) examination of Kuhn's engagement with this theme. Concurring with Melogno, our analysis extends his interpretation by exploring Larry Laudan's objections to certain research programs focused on a logic of discovery. We further scrutinize specific assumptions about discovery heuristics that have been misinterpreted within Laudan's methodological framework, particularly the one stemming from Herbert Simon's pioneering work. By synthesizing these perspectives, we aim to set up a preliminary framework for a more refined understanding of how history and philosophy of science inform the epistemic practices of agents operating with bounded rationality

Keywords: Thomas Kuhn, scientific discovery, historicist scientific rationality, scientific practices, heuristic methodologies

Cómo citar este artículo:

FIlcic, A. A., & García, P. (2025). The good old discovery-justification distinction: Remarks on Melogno's analysis of a Kuhnian account. Estudios De Filosofía, 72, 104-128. https://doi.org/10.17533/udea.ef.356239

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La vieja y conocida distinción entre descubrimiento y justificación: Observaciones sobre el análisis de Melogno de un planteamiento kuhniano

Resumen: La distinción entre descubrimiento y justificación se erige como una cuestión cardinal dentro de la filosofía de la ciencia del siglo XX. Sutilmente subyace a muchos temas y conceptos fundamentales pertinentes para nuestra comprensión del conocimiento. Las contribuciones de Thomas Kuhn son indispensables en este sentido, con sus críticas desempeñando un papel fundamental en la configuración tanto de su modelo inicial de progreso científico como de sus revisiones posteriores. Kuhn abordó esta dicotomía directamente en la primera de sus conferencias Thailheimer, presentadas en 1984. En este artículo, revisamos el examen de Pablo Melogno (2019) acerca del uso de la noción por parte de Kuhn. Coincidiendo con Melogno, nuestro análisis pretende ampliar su interpretación explorando las objeciones de Larry Laudan a ciertos programas de investigación centrados en una lógica del descubrimiento y sus ramificaciones filosóficas. Además, examinamos suposiciones específicas sobre heurísticas de descubrimiento que, en nuestra opinión, se han interpretado erróneamente dentro del marco metodológico de Laudan, particularmente la que surge del trabajo pionero de Herbert Simon. Sintetizando estas perspectivas, nuestro objetivo es establecer un marco preliminar para una comprensión más refinada de cómo la historia y la filosofía de la ciencia informan las prácticas epistémicas de los agentes que operan con racionalidad acotada.

Palabras clave: Thomas Kuhn, descubrimiento científico, racionalidad científica histórica, prácticas científicas, metodología heurística

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To Pablo Melogno, in memoriam.

1. Introduction

In a 2019 article, Pablo Melogno examines the implications arising from Thomas Kuhn's explicit critique of the dichotomy between the contexts of discovery and justification (DJ) within the philosophy of science, with particular attention devoted to the 'static approach' as elaborated in his Thalheimer Lectures. There, Kuhn presented his most direct engagement with the distinction. As observed by Melogno, an analysis of Kuhn's rejection of the distinction between discovery and justification contexts sheds light on various aspects of his broader philosophical endeavor. Following Melogno, in this paper, we investigate how such an analysis might facilitate a reappraisal of the philosophy of science, one that treats the history of science as more than a mere repository of examples while at the same time remaining faithful to the intricacies of contemporary scientific practices and the relevant philosophical literature about them.

In the first of the Thalheimer Lectures, as well as his essay "Scientific Knowledge as a Historical Product,"[1] Kuhn contends that certain historiographical approaches not only undermine the classical approach to philosophy of science but also furnish an evidentiary basis that can be "used to launch what (it could be) a more adequate approach" (Kuhn, 1984/2017, p. 53; Kuhn, 2022, p. 1). As we aim to show, Melogno's analysis helpfully elucidates the implications of repudiating the DJ distinction and sheds light on a conception of the philosophy of science for which the history of science is integral. His interpretation clarifies that rejecting the DJ distinction entails a reconceptualization of philosophy of science and a reorientation toward historical methods and sources. Ultimately, grappling with Kuhn's critique of the DJ dichotomy provides insight into the possibility of a profoundly historicized philosophy of science.

To elucidate the magnitude of Kuhn's disavowal of the discovery-justification (DJ) distinction and its association with a particular comprehension of the philosophy of science, a comparative analysis of his perspective alongside alternative propositions is warranted. In this context, Laudan's (1980/1981) historic-philosophical scrutiny concerning the pertinence of the DJ distinction bears a resemblance to Kuhn's stance, albeit with notable divergences. Laudan contests the then contemporaneous endeavors to delineate a logic of discovery as a philosophically inconsequential endeavor whilst concurrently delineating the legitimate domain of action for philosophers of science. In this paper, we hold that the DJ distinction, rather than being perceived as a

1 It was not unusual for Kuhn to deliver different lectures using the exact same wording. As Mladenovic explains, "«Scientific Knowledge as Historical Product» [(Kuhn, 2022, ch. 1)] was drafted and revised multiple times between 1981 and 1988. Various versions of it were given as invited lectures" (Mladenovic, 2022).

Estud.filos n.º 72. Julio-diciembre de 2025 | pp. 104-128 | Universidad de Antioquia | ISSN 0121-3628 | ISSN-e 2256-358X
https://doi.org/10.17533/udea.ef.356239

monolithic concept or attitude, should be more accurately conceptualized in terms of a constellation of attributes that can be differently weighed.[2]

To undertake this analysis, our initial step involves revisiting Reichenbach's foundational proposition regarding the DJ distinction. Subsequently, we will interrogate which attributes, among the plethora discussed within the scholarly discourse on scientific discovery, are pivotal for delineating a Kuhnian philosophy of science. At the same time, we will evaluate which of these attributes are instrumental in differentiating philosophical stances on the DJ distinction. Through this analytical framework, we aim to isolate key elements that, in our interpretation, form the essence of both Kuhn's and Laudan's propositions: namely, the delineation of the legitimate purview of the philosophy of science, its interrelation with other academic disciplines, and the practical application or instantiation of these theoretical accounts in case studies. Contrasting with Laudan's account, particularly his critique of the numerous attempts to formulate a logic of discovery circa the 1980s, facilitates an examination of which facets of the DJ distinction remain contentious. Our assertion extends beyond merely understanding a generic version of the philosophy of science or its interdisciplinary connections; it encompasses an exploration of how these theoretical frameworks are applied to specific instances, both historical and contemporary. The philosophical significance of the process of construction or development (of theories, models, experiments, instruments, etc.) serves as an illustrative example of this argument.

2. The DJ distinction: Where it came from and the issues involved

To elucidate the conventional interpretation of the DJ distinction and Kuhn's critique thereof, it is instructive to revisit its origins. The DJ distinction is commonly attributed to Hans Reichenbach's contributions. However, this attribution is not without contention. For instance, Karl Popper (1934) had already delineated a clear demarcation between subjects amenable to logical analysis and those that fall within the purview of empirical psychology, a distinction Reichenbach acknowledges. To gain a comprehensive understanding, let us succinctly examine the principal aspects of Reichenbach's argument. This step is also important insofar as Kuhn's early understanding of traditional epistemology stemmed from his reading of Reichenbach.

In the inaugural chapter of his seminal work, *Experience and Prediction* (1938), Reichenbach delineates three primary functions of epistemology: descriptive, critical, and advisory, with our focus being on the first two. He posits epistemology as treating "knowledge as a given sociological fact" (Reichenbach, 1938, p. 3), thereby situating

2 Here we follow a suggestion by Thomas Nickles (1980) and Paul Hoyningen-Huene (1987).

a theory of knowledge at the intersection of psychology and sociology, fulfilling a descriptive role and sharing the "available basis." From this premise, Reichenbach argues that epistemology is defined by specific inquiries (e.g., how do we ascertain that a proposition is true) that bifurcate the field of study into internal and external relations concerning knowledge. According to Reichenbach, the development of epistemology hinges exclusively on interrogations of internal relations, which in turn, facilitate the execution of its descriptive task:

> We may then say the descriptive task of epistemology concerns the internal structure of knowledge and not the external features which appear to an observer who takes no notice of its content. (Reichenbach, 1938, p. 4)

In contrasting the descriptive endeavor of epistemology with that of psychology, a further distinction is required. Epistemology is concerned with the formulation of rational reconstructions rather than the documentation of "actual events". Reichenbach articulates this distinction by stating, "what epistemology intends is to construct thinking processes in a manner in which they ought to occur if they are to be ranged in a coherent system" (Reichenbach, 1938, p. 5). He further clarifies, "epistemology considers a logical substitute rather than real processes. For this logical substitute the term rational reconstruction has been introduced" (here Reichenbach acknowledges Carnap, 1928). This differentiation can be comprehended by distinguishing between the "thinker's method" of deriving a solution and the manner in which it is depicted or "the way it's represented" (Reichenbach, 1938, p. 6). It is within this context that Reichenbach delineates explicitly his DJ distinction:

> I shall introduce the terms *context of discovery and context of justification* to mark this distinction. Then we have to say that epistemology is only occupied in constructing the context of justification. (Reichenbach, 1938, pp. 6-7)

Reichenbach's articulation of the DJ distinction is situated within the ambit of epistemology's descriptive task. Consequently, both rational reconstruction and the DJ distinction serve as *methodological approximations* toward fulfilling the comprehensive objective of a theory of knowledge endowed with descriptive capacity. Incidentally, this stance represents a marked divergence from Popper's perspective, primarily due to their differing interpretations regarding the potential role of probability within any rational reconstruction of an idealized scientific methodology.

Following the descriptive task, Reichenbach identifies a second imperative for epistemology, termed the critical task. It is within this domain that the quintessential function of epistemology is fully actualized. At this juncture,

> The system of knowledge is criticized; it is judged in respect of its validity and its reliability. This task is already partially performed in the rational reconstruction,

> for the fictive set of operations occurring here is chosen from the point of view of justifiability; we replace actual thinking by such operations as are justifiable, that is, as can be demonstrated as valid (Reichenbach, 1938, p. 7).

The question of what is ultimately justifiable can only be pursued to its fullest extent when we free ourselves from the bonds of descriptive limitations of 'actual thinking', forcing us to take a more abstract and universal point of view:

> But the tendency to remain in correspondence with actual thinking must be separated from the tendency to obtain valid thinking; and so we have to distinguish between the descriptive and the critical task (Reichenbach, 1938, p. 7).

Reichenbach's formulation of the DJ distinction can be encapsulated by two principal assertions. Firstly, the foundational aspect of the DJ distinction is predicated on a conceptualization of justification as a form of rational reconstruction, coupled with the premise that distinct phases within scientific inquiry—discovery and justification—can be unequivocally delineated. Secondly, Reichenbach's DJ distinction offers a glimpse into the fundamental role of philosophy, which is to assess the validity and reliability of knowledge. These elements constitute the core of Reichenbach's DJ distinction. The fact that this distinction is posited within the descriptive phase of epistemology does not detract from its significance in highlighting the philosophical endeavor of justification through rational reconstruction.

Prior to delving into Melogno's reinterpretation of Kuhn's perspective, it is, however, imperative to delineate the broader implications of the DJ distinction beyond Reichenbach's initial proposition. As elucidated by Nickles and Hoyningen-Huene, the DJ distinction encompasses multiple dimensions. Within the philosophical discourse on DJ, various interpretations of the distinction have emerged. Hoyningen-Huene identifies the DJ distinction as a battleground for philosophers with either a positivist or historicist bent (Hoyningen-Huene, 1989, p. 501), many times viewing DJ as a temporal sequence where the act of discovery precedes the justification of a theory. This perspective often bifurcates the process of discovery from the methodology of subsequent verification. In alignment with Reichenbach's thesis, the DJ distinction is frequently interpreted as a demarcation between logical and empirical aspects (Hoyningen-Huene, 2007). Furthermore, DJ might serve to differentiate between disciplines, positioning philosophy as an evaluative field in contrast to the empirical domains of sociology, psychology, and even history (Hoyningen-Huene, 2006; Laudan, 1980/1981; see also Popper, 1934).

Which of these aspects is relevant to Kuhn's account? Kuhn's brief critique of the DJ distinction in *The Structure of Scientific Revolutions* characterizes it as a "hidden theory" that inadequately reflects the philosophy of science, particularly in its neglect of the historical dimension of scientific development. Thus, he ends the book's introduction with this passage, worth quoting in full:

> Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge, and I still suppose that, appropriately recast, they have something important to tell us. Yet my attempts to apply them, even *grosso modo*, to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic. Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields. If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate. How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply? (Kuhn, 1962, p. 9)

Hoyningen-Huene distills this "theory" into three main propositions: the bifurcation of innovation into discovery and justification phases, the assertion that discovery lacks a structure amenable to logical scrutiny, and the sufficiency of logical analysis for understanding justification (Hoyningen-Huene, 2006, p. 126; see also Melogno, 2019, p. 23). Kuhn challenges these propositions, advocating for a nuanced understanding that accommodates even weaker formulations of the hidden theory that do not so directly follow Reichenbach's very strong proposal. Such weaker formulations might recognize the structured or discernable nature of discovery and entertain alternative conceptions of justification beyond strict logical analysis. Similarly, the strict and linear differentiation of phases within scientific research, as posited by the strong interpretation of the hidden theory, is linked to a logical framework of justification. Adopting a different model of justification allows for the exploration of diverse evaluations and considerations. In subsequent sections, we will explore how these softer interpretations of the hidden theory could significantly inform our comprehension of the philosophical debates surrounding the DJ distinction.

3. An account of philosophy of science from the first Thalheimer conference: Melogno's analysis

To engage with the DJ distinction through the lens of Thomas Kuhn's philosophy, the *Thalheimer Lectures* present an unparalleled opportunity. As Melogno (2019) articulates, "In no other place of his published work does Kuhn discuss the Discovery Justification distinction with the thoroughness and detail" (p. 43). These lectures were

Estud.filos n.º 72. Julio-diciembre de 2025 | pp. 104-128 | Universidad de Antioquia | ISSN 0121-3628 | ISSN-e 2256-358X
https://doi.org/10.17533/udea.ef.356239

delivered in 1984 at Johns Hopkins University, Baltimore, Maryland, upon an invitation extended by Peter Achinstein.[3] Melogno's analysis is bifurcated into historiographical and philosophical dimensions, with a critical examination of the DJ distinction serving as a focal point. The essence of this critique is directed towards the underlying theory of justification that sustains the distinction. Melogno aims to "establish a connection between Kuhn's historiographical thought and his criticism of the traditional distinction between the context of discovery and the context of justification" (Melogno, 2019, p. 1). Central to Kuhn's critique is the interrogation of the foundational aspects of the theory of justification and its purported independence from the genesis of scientific theories. This critique allows Kuhn to evaluate the conventional philosophy of science, which he characterizes as adopting a *static approach*. This approach, according to Kuhn, encapsulates an idealized representation of science where the DJ distinction holds significance. By dissecting the DJ distinction through various attributes, this paper not only elucidates Kuhn's critique but also refines the primary arguments proposed herein.

The first feature of a *static approach* is a foundational attitude. Bacon and Descartes are the two philosophers who, for Kuhn, exemplify the foundationalist *ethos* in epistemology: methodically finding a secure starting point for all knowledge claims is central to their philosophical enterprises. When a foundation is understood in empirical terms, as in Bacon's case, then the "building" of knowledge depends on "the existence of an observational and purely descriptive basis" (Melogno, 2019, p. 8). These are the "necessary conditions to make observation a source of the epistemic authority" (Melogno, 2019, p. 8). Although Bacon and Descartes' accounts are based on very different assumptions, namely "inductive empiricism" and "mathematical deductivism," they share the search for an (unquestionable) foundation. Of course, Melogno is fully aware that Kuhn's account of Descartes and Bacon and the way that their reception of philosophy in the 20th century is described can be put in question. In this particular case, however, the issues raised by Kuhn's analysis are more important than their historical or exegetical accuracy. For Kuhn, foundationalism is the "epistemological core of the static approach" (Melogno 2019, p. 8).[4] The principal attribute of foundationalism lies then in its inherent nature of being beyond dispute, embodying the pursuit of absolute certainty.

The second essential assumption within the static approach is deductivism, specifically the utilization of a "deductive chain" as an adjunct to a foundationalist

3 The Thalheimer Lectures, together with the Shearman Memorial Lectures, the Notre Dame Lectures, and the draft of the book *Plurality of Worlds* constituted, until recently, the most important part of Kuhn's unpublished papers. In 2017, a translation of the *Thalheimer Lectures* into Spanish was published, edited by Pablo Melogno and Hernán Miguel, and translated by Leandro Giri. That edition includes an illuminating introductory study by Pablo Melogno and a short informative prologue by Paul Hoyningen-Huene (Kuhn, 1984/2017).

4 In Kuhn's later thought, a historically oriented philosophy of science and an analysis of a lexicon and its dynamics are the main topics (see, Melogno, 2023).

perspective. This approach encompasses not merely the quest for a foundational base but also endeavors to identify a "truth-certifying method of discovery" (Kuhn, 2022, p. 4). This method aims to ensure the transmission of foundational certainty throughout the entire epistemic structure. This facet is crucial within the narrative constructed by Kuhn, as it highlights the preservation of "the agenda and the spirit of the deductive model" in scenarios where inductive reasoning fails to facilitate the derivation of knowledge from experience. Furthermore, this perspective introduces a novel interpretation of science, characterized by the "generation of testable conclusions from generalizations", a stance that significantly contributes to the "increasingly categorical distinction between" the processes of discovery and justification (Kuhn, 1984/2017, p. 58). It is imperative to recognize that the commonality between these two features—the foundational and the deductive—is their shared attribute of absolute certainty, whether pertaining to a foundational element or a procedural methodology.

The way Kuhn delineates the first two characteristics of the *static approach* bears a notable resemblance to the analysis presented by Laudan (1980/1981) in his evaluation of proposals for a logic of discovery. This parallel sets the stage for a potentially enriching comparative analysis in the next section, where contrasting Kuhn's framework with Laudan's insights could be a fruitful route to tackle the issue at hand. With such a comparison, we attempt to enhance our understanding not only of the respective analytical strategies used by the two philosophers-historians but also of their divergent perspectives on the objectives and methodologies of a philosophy of science that is attuned to the historical development of scientific inquiry.

4. Larry Laudan's account of DJ: logic of discovery and philosophy of science

Laudan (1980/1981) investigates why there was so much optimism for a logic of discovery in the 17th century and why there is (and there should be) a certain philosophical pessimism about the possibility of restoring that project. An obvious reference here is Herbert Simon's logic of discovery research program, which was under discussion in philosophical circles at the time. According to Laudan, the desire to develop a logic of discovery was historically based on two quite different motives. On the one hand, there was the "heuristic and pragmatic problem of how to accelerate the pace of scientific progress, of how to increase the rate of new discoveries" (Laudan, 1980/1981, p. 183). On the other hand, there was the epistemological problem of how to provide a solid warrant for our knowledge claims. With a logic of discovery, both problems would have been solved. As conceived in the 17th and 18th centuries, a logic of discovery should function epistemically as a logic of justification. Following this line of analysis, Laudan

Estud.filos n.º 72. Julio-diciembre de 2025 | pp. 104-128 | Universidad de Antioquia | ISSN 0121-3628 | ISSN-e 2256-358X
https://doi.org/10.17533/udea.ef.356239

describes two groups that differ in how they view the justificatory claims of genuine science: consequentialists and generators. The consequentialists believed that one could select a set of results of a theory and compare them with observation, while the generators believed that theories could be established by showing that they followed logically from statements taken directly from observation. The method used by the former was that of hypothesis and "post hoc" confirmation, while the latter was seeking algorithms that *generate* knowledge.

The final ingredient in this story is also epistemological infallibilism, a stance that is fundamentally at odds with consequentialism due to the typically inconclusive nature of arguments derived from the latter. It is crucial here to acknowledge that the logic of discovery, as conceptualized in the 17th century, was oriented toward the formulation of infallible claims. This era witnessed the flourishing of discovery logics at a time when empirical generalizations were considered the quintessence of scientific endeavor, or when there was a prevailing skepticism towards the existence of unobservable entities. The pursuit of theories positing any form of "deep structure," or those whose central tenets lack direct empirical analogues, appears to be at odds with the quest for an infallible algorithm of discovery. It is in this context, namely that of clarifying what a rational reconstruction amounts to, that the DJ distinction is proposed. Thus, the evaluation of the theory becomes separated from the process or method by which it was formulated. In other words, the way a theory is generated is now inconsequential in epistemic terms, at least within the framework of a hypothetical-deductive method. Clearly, there is an obvious reminiscence of Reichenbach's account of DJ.

As Melogno notes, Kuhn's account follows a very similar historical path: "once hope for a constructive method had been given up, discovery was relegated by the tradition to psychologists and sociologists" (Kuhn, 2022, p. 5). Under such a view "only justification, the evaluation of proposed laws and theories, remains the proper concern of philosophy of science" (Kuhn, 2022, p. 5) in so far as theories can be generated in many different ways. Again, we note that certitude and infallibility were a key part of the logic of discovery in the 17th century.

So far, two aspects of Laudan's account can be identified. Firstly, Laudan identifies the infallibilist ethos as a foundational element in the original conceptualization of the logic of discovery. This infallibilism, as Laudan articulates—and Kuhn concurs—serves as a critical *epistemological value* underpinning the DJ distinction. Such quests for infallibility of knowledge significantly influence the philosophical motivations driving the establishment of the DJ distinction. Secondly, Laudan's analysis extends to the importance of historically situated methodologies. He posits that both the proponents of discovery (generators) and those advocating for consequentialism must be understood primarily through their methodological commitments before considering the epistemological values they espouse. This emphasis on the historical

context of methodological approaches underscores the scientific inquiry's dynamic and evolving nature of, suggesting that philosophical analyses of science must account for the temporal and contextual specificity of scientific practices. For Laudan, this is a particular way to do philosophy of science: the analysis of historically situated methodologies. But this is not the only example of the instantiation of Laudan's account.

For Laudan, exploring heuristic problems within the scientific domain, such as optimizing the generation rate of new and promising discoveries, raises fundamental questions about the very nature of scientific inquiry. The question that emerges in this context is the identification of what aspects of the study of theory genesis are inherently philosophical. Laudan's perspective on this matter has been the locus of some controversy, for he posits that "a theory is an artifact, fashioned perhaps by certain tools (e.g., implicit rules of 'search')" (Laudan, 1980/1981, pp. 190–191). Are the processes of manufacturing worthy of philosophical elucidation? Laudan argues against that being the case:

> The investigation of the mode of manufacture of artifacts (whether clay pots, surgical scalpels, or vitamin pills) is not normally viewed as a philosophical activity. And quite rightly, for the techniques appropriate to such investigations are those of the empirical sciences, such as psychology, anthropology, and physiology. (Laudan 1980/1981, p. 191)

Here, we have not only a characterization of how to understand philosophy of science but also a new *methodological* instantiation: the context of construction belongs to empirical sciences not to philosophy.

How can we evaluate Laudan's proposal? First, Laudan's assessment of the logic of discovery as philosophically irrelevant can be criticized in several ways. According to McLaughlin (1982), although Laudan and some defenders of a logic of discovery like Simon are at odds in their proposal:

> they share the assumption that the DJ distinction must be maintained, they share one thesis—namely, that the "logics" or inference procedures involved in discovery and in justification are different and independent. This thesis permits Laudan to argue that there is no rationale for the philosophical study of discovery— in contrast to justification, which is of central interest to epistemology. (McLaughlin, 1982, p. 199)

As a defense against his apparent dismissal of discovery, Laudan (1983) remarks that he is introducing the idea of the *context of pursuit* as a nuanced addition to the traditional dichotomy of the context of discovery and the context of justification. This concept highlights that evaluative processes, typically reserved for the domain of justification, intersect indeed with the context of discovery, albeit partially. Laudan's

assertion that some evaluative instances can be present during the discovery phase suggests a more integrated view of the scientific process, where the demarcation between discovery and justification is not as rigid as previously thought. On the other hand, it is obvious that Laudan's overall account is far from Reichenbach's conception of epistemology. Laudan's proposal must be understood in terms of a philosophy of science that could (if not should) be influenced by the history of science in non-trivial ways. Laudan's notion of change and progress in the methodology of science is an example of this perspective. However, how this way of doing philosophy is actually instantiated can be questioned. Laudan's example of theories as artifacts can be used to illustrate this case. Kevin Kelly (1987), for instance, has pointed out that the very same reference to theories as artifacts can be used to emphasize the philosophical relevance of the construction process. Even though the context of pursuit may be a good answer to McLaughlin, its particular instantiation indicates Laudan's understanding of the slightly narrow scope of philosophy. But here, we can make a stronger statement. The instantiation of a conception of philosophy of science is not a secondary aspect. It is a very different way of conceiving the philosophical task if the construction process gets excluded. More importantly, this interpretation is consistent with the way in which Laudan developed his general proposal for how a (historical) philosophical account must be developed after taking actual scientific practices into account. Examining the values, methods, and goals that shape a scientific community is a prerequisite for developing a philosophical account.

An interesting interpretation of Laudan's paper was advanced by Thomas Nickles (1985). According to Nickles, the philosophical landscape in the mid-1980s can be grouped around two theses: the *per se* thesis, which posits that the methods of generating (new) solutions carry "inherent probative weight", and the *anti-divorce* thesis, which argues for a necessary continuity between "the original conception of an idea" and its justification (Nickles, 1985, p. 177). Nickles believes that Laudan's paper is attacking the *per se* thesis, and McLaughlin is discussing an *anti-divorce* thesis. Thus, Laudan's pessimism about a philosophy of discovery depends on an interpretation of how the new program of the logic of discovery follows its goals (compared to the old one). As Laudan points out in his reply to McLaughlin, there can be evaluative instances in the so-called context of discovery. In this sense, the core account of DJ is challenged, but the task of philosophy seems to be preserved without endeavoring into discovery territory. As we have seen above, Reichenbach construed what we have called the core account of DJ in terms of two aspects: an understanding of justification (in terms of rational reconstruction) and the assumption that it can be properly distinguished between two steps or stages. Both assumptions are challenged by Laudan as a way to draw new boundaries for the evaluative stance within the context of pursuit, thus reshaping the original DJ distinction under a new methodological perspective. Nickles defends that only the divorce thesis (not the *per*

se thesis) "violates" the contemporary logic of discovery programs. However, looking at Laudan's discussion through the lens of Kuhn's critique might point towards other relevant aspects.

Two critical points warrant consideration. First, there is the matter of delineating the philosophical enterprise through its practical applications or instantiations. As previously established, this is not a peripheral concern but rather a fundamental issue. Second, an often-understated aspect merits attention: the coercive nature inherent in rule systems. As previously observed, both Laudan and Kuhn identify certainty (or infallibility) as a crucial element underpinning the original distinction between the contexts of discovery and justification (DJ distinction). It is pertinent to examine this issue through one of Laudan's analytical targets, specifically, Herbert Simon's logic of discovery. This reference provides a foundation from which to challenge the robust interpretation of Kuhn's account (the "hidden theory"), a perspective that Laudan partially endorses.

Herbert Simon's project regarding a logic of discovery, particularly through the utilization of computer programs (e.g., Simon, 1973), presents a nuanced perspective that diverges significantly from the static approach critiqued by Kuhn and the generator model discussed by Laudan. Simon's project is often characterized by its algorithmic approach to structuring discovery in the form of a set of rules (implemented as computer algorithms). Indeed, the project led to the production of several computer programs with algorithmic rules that were held up as paradigmatic examples of the context of discovery (and thus some aspects of human creativity). This would suggest that human problem-solving has a discernible structure. Such general characterization and some computer simulations might then superficially appear to align with the quest for rigid patterns of certainty and infallibility that Kuhn and Laudan associate with traditional epistemological frameworks. However, a deeper examination of Simon's project reveals critical distinctions that challenge this initial perception.

His concept of bounded rationality is central to understanding Simon's departure from the static approach. This notion acknowledges the inherent limitations of human cognitive capacities, including constraints on time and resources. Bounded rationality, therefore, necessitates a form of rationality that is, at least in Simon's interpretation, inherently fallible and adaptable, contrary to the pursuit of absolute certainty.[5] The methodological implications of bounded rationality lead to the development

5 Certainly, Simon's interpretation might be questioned, especially to the extent that one might keep an 'ideal rationality' as a Peircean regulative ideal, i.e., that view of rationality that we would converge to if the enquiry is ever finished. However, the post-Darwinian undertones of Kuhn's reinterpretation of Kantian ideals, seem to commit him to an even more revisionist view of what and how we might be able to think in a determinate historical moment. We thank the anonymous reviewer who cleverly pointed this out to us.

of heuristic search strategies which are designed to navigate the complexities of problem-solving within these constraints. The fallibility of heuristics is not viewed negatively; rather, it is seen as essential for effectively exploring the problem space under conditions of bounded rationality and combinatorial complexity.

While initially interpreted as endorsing a rigid, rule-based structure, the representation of discovery processes through computer algorithms should be understood within the historical context of Simon's work. The evolution of Simon's project from optimistic beginnings with the *Logic Theorist* (Newell & Simon, 1956) to a more nuanced understanding of the role and nature of rules in later years (e.g. Kulkarni & Simon, 1988) underscores a shift toward recognizing the fallible and heuristic nature of discovery.

This new dimension was manifest not only in the discourse surrounding thinking machines but also in the diminished emphasis on fixed rules. Simon explicitly rejects a central issue that is the rigid certainty Kuhn and Laudan attributed to the static approach. The erroneous conflation of computer algorithms with coercive rules obfuscates the inherently fallible nature of heuristics, constituting the kernel of problem-solving activities within the context of discovery. Consequently, this shift challenges the misconception that computer algorithms inherently embody a static, infallible approach to discovery. Instead, it underscores the adaptability and fallibility intrinsic to Simon's conceptualization of heuristic search as a distinct form of learning.

Of course, Kuhn's perspective underscores other limitations regarding attempts to formalize discovery within a *purely* logical or algorithmic framework. Kuhn's critique of the logic of discovery, with its emphasis on the communal and sociological dimensions of scientific progress, introduces additional layers of complexity to the discussion that go beyond the nature of an individual's heuristic search, as discussed by Simon to replace the ideal reasoners and decision-makers we usually find in management and economic contexts.

From our discussion so far, it follows that a deeper exploration of these themes is necessary. By examining the interplay between individual rationality, communal practices, and the heuristic nature of discovery, we can gain a more comprehensive understanding of the philosophical challenges and opportunities presented by any logic of discovery in contemporary science. To this end, Kuhn's proposals continue to be fertile ground, and to start this task, we return to Pablo Melogno's analysis in the next section.

5. Developing a developmental account of philosophy of science

Two other important features characterize Kuhn's static approach to scientific knowledge: first, the notion that scientific knowledge is fundamentally propositional

in nature; second, the idea that theory change must be understood in terms of a set of non-contextual coercive rules, given the rationality of the decision procedure. Rightly, Kuhn criticizes these features while arguing for a more accurate (and useful) developmental account of science and theoretical change. For instance, the importance of "examples, the assumption of common rules, and the acquisition of cognitive rules" (Melogno, 2019, p. 12) in the particular context of "scientific training" shows how this shift from propositional structures favors the construction process over products.

The perspective that evaluation criteria in science are coercive, akin to the rigor of a mathematical proof, aligns itself with a static approach that conceptualizes science as an individualistic endeavor. Under such a view of correctness criteria, seeing "science as a one-person game" is more natural. In such a static approach, theory assessment and individual evaluators are free from contextual influence, that is, far away and protected from the communal and historical contexts in which science is actually practiced.

Against such a static view, Kuhn's philosophy of science introduces a nuanced understanding of theory appraisal. He posits that evaluating scientific theories has a holistic character, which he later refines into a relational stance, particularly regarding the context of theory choice. This approach acknowledges that issues within a theory may be traced back to a specific subset of a theory's propositions, yet such attributions are inherently provisional and lack absolute certainty. Different subsets of these propositions might equally prove problematic. As highlighted by Melogno, this aspect of Kuhn's theory is crucial for accommodating the possibility of disagreement among scientists. It underscores the argument that theory appraisal is neither coercive nor reducible to the judgments of isolated individuals. Instead, scientific evaluation is a collective endeavor deeply embedded within the practices and norms of scientific communities. In contrast, as Melogno emphasizes, a way to understand a Kuhnian developmental account is contemplating Hasok Chang's proposal of shifting from propositional structures to scientific practices (Chang, 2012), a position that in many senses inherits and improves the Kuhnian view of science and its history. The centrality of the individual subject, once idealized with theoretically neutral observation skills, is now shifted to communities. As is Kuhn's, Chang's view represents a significant departure from static points of view since both the procedures for scientific inquiry and the criteria for theory acceptability are now fundamentally communal. This reconceptualization foregrounds the role of scientific communities in shaping the practices, standards, and evaluative criteria that guide scientific work. Then the "procedures" and the "criteria of acceptability" become communitarian, and theories do not take center stage:

> The premier subject of discussion in philosophy of science has been theories as organized bodies of propositions. This has led to the neglect of experimentation

> and other non-verbal and non-propositional dimensions of science in philosophical analyses. Many historians, sociologists and philosophers have pointed out this problem, but so far no clear alternative philosophical framework has been agreed upon to provide a language for fuller analyses of scientific practice. A serious study of scientific practice must be concerned with what it is that we actually do in scientific work. This requires a change of focus from propositions to actions. (Chang, 2012, p. 15)

Chang's view then forces us to move towards actions as a core concept to assess knowledge-making practices, a move that takes know-how as a more important feature than knowledge-that.[6] There is also another aspect of a developmental account that directly runs counter to the DJ dichotomy. From a historical perspective, it is not possible to make a clear distinction that allows one to speak of a starting point in "knowledge acquisition": "All narratives of scientific development begin in midstream, with the scientific process already underway" (Kuhn, 2022, p. 7).

One might argue, then, that taking these aspects into account could restrict any sort of fruitful examination of theory change. But, as Melogno points out, the criticism here is against a feature of a static approach that assumes that the explanation of theory change must focus on the reasons why a scientist chooses a theory. In a developmental account, the process of theory change must include the previous theory. Then, the issue (for a historian) is to explain "the transition from one body of knowledge claims to a different, though overlapping, set" (Kuhn, 2022, p. 7). From a methodological point of view, Melogno names this stance a "relational view of theory choice" insofar as "every choice takes place between rival theories" (Melogno, 2019, p. 16). In Kuhn's words, using the empty territory analogy:

> The primary process by which science advances is thus seen as the occupation of previously empty territory, the cumulative replacement of ignorance by knowledge . . . the story is told as though the old position was dispensable, as though the newly occupied territory would have been empty if only previous scientists had been more careful in testing their conclusions. (Kuhn 1984/2017, p. 69, quoted by Melogno, 2019, p. 26)

The last two features (being relational and processual) are directly linked with the contextual character of theory choice. More importantly, if theory choice is

6 The extent toward which epistemology needs to follow suit will depend on the kind of irreducibility one posits regarding the know-how/know-that dichotomy, at least for discovery purposes. Then the DJ distinction could be kept, focusing on a highly normative philosophy of science only on the matter of why certain practices and procedures justify good knowledge *claims* or at least are more likely conducive to them. We thank a reviewer for bringing this point to our attention. Hasok Chang himself has provided a more detailed view of his action-centered philosophy of science in his illuminating *Realism for Realistic People* (Chang, 2022).

inherently comparative, "there is no need to set an acceptability threshold that should be reached as an intrinsic property of the theories being evaluated" (Melogno, 2019, p. 16). Knowledge does not have an external foundation; rather "it is the community which fixes the standards of rationality for theory choice" (Melogno, 2019 p. 16). Two more points are crucial. Firstly, justification strategies (meta-normative claims, in a sense) are also up for discussion. That is, during a "scientific revolution not only does the theoretical content change but also the strategies of justification" (Melogno, 2019, p. 16). This dynamic nature of scientific progress highlights the fluidity of the criteria by which theories are judged, reflecting, therefore, the evolving *standards of rationality* within the scientific community. Finally, and related to the previous point, when a "theory change occurs, not every belief of the previous theory is rejected" (Melogno, 2019, p. 18). Why is this last consideration so important? Because it allows us to underline those scientific practices that are "constitutive" and explain stabilized practices. Even more, "the constitutive role is not the result of a stipulation or an individual decision but of the way in which these beliefs mold scientific practice" (Melogno, 2019, p. 19). They constitute the 'soft rules' of the game that any aspiring scientist needs to learn and act by. In Kuhn's words: "To refuse to accept them would be to decline membership in that tribe and thus to refuse the practice of science" (Kuhn, 2022, p. 10). This is one of the components and functions of what Kuhn originally described as the dogmatic nature of some beliefs regarding theory and practices for a scientific community (Kuhn, 1963). This dogmatic nature, far from being a pejorative attribute, plays a crucial role in maintaining the coherence and continuity of scientific practices, even as they evolve and morph into new theoretical paradigms and evidentiary standards.

These features of the developmental account are tied to an alternative view of justification. From this alternative view of justification emerges a philosophy based on an adequate *historiographical* perspective of science. Drawing on the Hoyningen-Huene analysis (2006), we can grasp some aspects of this alternative account of justification. Perhaps the main focus of Hoyningen-Huene's (2006) analysis is to point out that Kuhn is not just "describing" how a decision process about a theory choice is made in a particular community. Kuhn also wants to change how justification is understood. Therefore, it has to become an account of rational choice. Under this new scheme, choice is rational because it is "directed at the cognitive goals of science, that is roughly the invention and improvement of explanatory and often predictive theories" (Hoyningen-Huene, 2006, p. 127).

A key question, then, is how Kuhn understood justification in the context of theory selection. A clear contrast is made with logical empiricism, where justification uses formal logic and basic-protocol sentences (Hoyningen-Huene, 2006, p. 126). Kuhn's critique of the DJ distinction highlights the artificial and spurious nature of this separation, arguing that the resources employed in the justification of theory

choice often originate from what would traditionally be considered the context of discovery. This observation points to a more integrated and holistic understanding of scientific inquiry, where the boundaries between discovery and justification are not as clear-cut as previously assumed. The usefulness of the distinction gives way to its shortcomings, many of which are due to the social nature of science. We can summarize this point in the following way: Central to Kuhn's account is the notion that theory choice is a communal endeavor, with cognitive-epistemic values emerging from and being tailored to specific scientific communities. This perspective emphasizes the importance of sociological factors not only in the context of discovery but also in the realm of justification. More importantly, if the inclusion of sociological aspects in the justification process underscores the interconnectedness of epistemic and social dimensions in scientific practice, challenging the notion of a purely objective or value-neutral science, the distinction itself might be more akin to a heavily idealized pedagogical model. Kuhn (1977) was perhaps his first direct engagement with the complexities of theory choice from a refined view of how certain kinds of shared values within a scientific community lead its members to assess a theory as a better candidate to guide future research.[7]

As expected, we began by questioning an interpretation of justification and ended up with the question of the limits of philosophy of science and its relation to other disciplines. The (absolute) autonomy of philosophy is now called into question. Regarding the autonomy of philosophy of science, Kuhn explicitly defended that "philosophy of science and sociology of science cannot be practiced independently of each other" (Hoyningen-Huene, 1992, p. 491). Being communities, the basic agent in science is the main reason why the autonomy of philosophy of science is challenged. We should recall that the DJ distinction was motivated in part by the desire to secure the autonomy of philosophy. The relationship between philosophy of science and sociology of science, as Kuhn sees it, has to be made according to the questions posed by the historiography of science (Hoyningen-Huene, 1992). A new historiography of science is emerging that overcomes the "presentist" perspective, which portrays science as a linear, simple, and direct "cumulative growth of knowledge, in which later progress never substantially changes earlier knowledge" (Hoyningen-Huene, 1992,

7 In 1994, he notes explicitly that "Both the methodological relativism inherent in the developmental approach and the main criteria deployed in making cognitive evaluation[s] are thus of a quite traditional sort. The problems they present do not result from the developmental approach but from the strange way in which philosophers of science (and others) employ the terms *objective* and *subjective*, *rational* and *irrational*. The concepts which these terms denote need systematic philosophical scrutiny, from which reform is likely to follow" (Kuhn, 2022, p. 128). In a note, he points the reader towards his "Objectivity, Value Judgment, and Theory Choice" (Kuhn, 1977) and "Rationality and Theory Choice" (Kuhn, 1983). He also refers that a better treatment will be given in chapter 8, that he never got the chance to write. Yet from the abstracts and brief references, we observe that Kuhn seems to consider the notion of "historical progress" as the way to assess the rationality of theory choice (and, more speculative on our part, the need for new scientific disciplines).

pp. 488–489). This "deceptive image of science" presented by the old historiography oriented and guided the perspectives, questions, and problems in philosophy and sociology. Through a hermeneutic reading of historical sources, another view of science can be generated. Then, a new "methodological set-up" produces the kind of data for sociology and philosophy that generates an alternative perspective (Hoyningen-Huene 1992, p. 489).

6. Taking stock

This alternative perspective, informed by a more sophisticated historiography of science, underscores the dependency of philosophical analyses on sociological studies in capturing the multifaceted nature of scientific practice. By acknowledging the influence of historical, social, and communal factors on scientific inquiry, Kuhn's approach encourages a more integrated study of science that combines insights from philosophy, sociology, and history. This interdisciplinary orientation not only enriches our understanding of science but also highlights the limitations of attempting to isolate the philosophy of science from the broader context in which scientific knowledge is produced, validated, reformulated, and studied.[8]

To unlock the relevance of suppositions in a static approach, it is worth showing how they operate in making DJ relevant. A propositional view of knowledge is well-fitted for a philosophy of science that restricts itself to the context of justification. And it is more natural to consider it as independent of context and practices that made it possible. Additionally, a deductive methodology is better developed in "timeless statements" (Kuhn, 2022, p. 5). Following Melogno's analysis, we presented a link between how Kuhn proposes a philosophy of science that could understand the dynamics of science and their critics to the DJ distinction. We compared this proposal with an account from Laudan that is, in principle, very similar. But, in order to shed light on the differences, a more specific description of what is involved in a developmental account of philosophy of science is required.

In more specific terms, we can summarize the main topics of the previous sections as follows. First, "Kuhn attacks the idea of a 'logic' of justification" (Melogno 2019, p. 31). This logic is understood in terms of "timeless" criteria of evaluation. But Kuhn also rejects that DJ distinction draws an exclusive line with distinctive members and features. Evaluative and empirical aspects can characterize members of each category. Finally, Kuhn emphasizes that "psychological and sociological processes" cannot be

8 As a reviewer aptly noted in a first draft of this paper where we worded the relationship between sociology and philosophy of science as an 'interdependence', the extent towards the former *needs* the latter is debatable and worthy of a more detailed treatment than what we can say in this article. It also exceeds Kuhn's own rather brief treatment of the subject, though his remarks need to be considered.

ruled out in order to understand what is considered "context of justification" since this last activity is considered part of a communitarian practice of science (Melogno, 2019, p. 31). Then, a processual, relational, and contextual perspective are the characteristics of a developmental account of science where we cannot make a clear and exclusive distinction between contexts. But what is involved in this criticism?

> The point is not that discovery and justification are the same process, but that many of the considerations relevant to the first prove central to the second as well. Indeed, in the first stages of either, the overlap is usually so great that even the processes themselves cannot be told apart. (Kuhn, 2022, p. 9)

Laudan takes a similar view. But, it seems that Laudan is more concerned with distinguishing between the proper task of philosophy and the examples of how to implement it. And it is in the examples of how to apply an account of philosophy of science that we can learn a lot about how to understand that account. This is why Kelly (1987) focuses his defense of some contemporary projects of the logic of discovery on emphasizing the importance of the process of construction. Arguing against Laudan's weak 'artifact analogy' and using the same language, he playfully states:

> If there is a difference between hypotheses and vitamin pills so that quality control is philosophically interesting in the first case but not in the second then this difference may make a difference in the case of hypothesis generation as well. An obvious candidate for the difference that makes a difference is that hypotheses are the raw material for *knowledge* and epistemology is, after all, the study of knowledge. Clay pots and vitamin pills, on the other hand, are not candidates for knowledge, so their manufacture *and quality control* do not constitute epistemological concerns. (Kelly, 1987, p. 437, emphasis in the original)

Here, a significant difference between conceptions of discovery is identified in terms of how a philosophy of science account is to be used. Or, in other words, what is considered the proper task of philosophy?

In general terms, the issue is not that the theory change cannot be studied, but that the restrictions by which that study must be developed are *relational* (we must compare theories), *contextual* (we cannot rely on abstract, coercive rules and the process of theory appraisal is communitarian) and *processual* (any separation that suppose a clear origin is arbitrary). In line with Hoyningen-Huene's interpretation, it can be said that Kuhn is developing a more radical view of "justified decision" (Hoyningen-Huene, 1987, p. 509). A justification where psychological and sociological aspects have a role to play. The main issue now becomes what constitutes "good enough reasons" to accept or change a theory. In Melogno's words:

> [I]n the Kuhnian view, what history of science calls into question is not the very existence of the Discovery Justification distinction, but the specific picture of justification it entails. (Melogno 2019, p. 30)

This dynamic picture of philosophy of science and justification, in line with Laudan's critique, seems to work against some of the most important contemporary logics of discovery. Historically, Simon's project had been developed under the pattern of problem-solving activities, where an individual's strategies and decisions are on the spot. However, as we remarked, three considerations can be used to mitigate this criticism. Firstly, an important feature of the static approach was the coercive nature of the rules. This reliance on coercive rules is counteracted by the heuristic nature of Simon's project. Heuristics, by definition, resist the rigidity of coercive rules, embodying the flexibility and adaptability that Simon champions in his account of rationality as an essential part of the means by which even "simple" problems need to be solved. The second consideration we shall point out here concerns the relevance of individuals in a Kuhnian theory of change dynamics. It was negative in the sense that it explained divergence, not the positive aspect of decision. Decisions are explained in a community, and they might involve a very complex relation between the means available and the cognitive ends of the communities under study. We have here a complex picture between self-modifying micro and macro elements; to explain the complexity of communal decisions, many individual explanations are required (Garcia, 2023). The third consideration is even more general: Is bounded rationality essentially limited to individuals? There are some developments that question whether this was an essential feature of Simon's project (Hayakawa, 2000). It seems that bounded rationality, in general, beyond the limitation of their particular project or instantiation in models, can be an interesting complement to a dynamic account of philosophy of science, such as Kuhn's.

7. Final remarks

Through a reconstruction based on Pablo Melogno's analysis, in this paper, we delved into the nuanced discussion surrounding the DJ distinction, drawing mainly upon Thomas Kuhn's insights from the *Thalheimer Lectures*. Kuhn's philosophical exploration of the DJ distinction in these lectures provides a more explicit and elaborate framework for understanding the intricacies of this debate. By revisiting the DJ distinction, we aim to shed light on several critical issues, including the relationship between the philosophy of science and the history of science, and the question of the autonomy of philosophy in relation to other disciplines.

Central to our discussion was the concept of justification itself, which represents a key philosophical element within the DJ distinction. Hans Reichenbach's initial

formulation of the DJ distinction served as an essential reference point, positing that the task of philosophy can be discerned through the lens of this distinction. Reichenbach characterizes justification as a rational reconstruction, emphasizing the role of logical analysis as a fundamental component. This conception of justification, coupled with the assumption that the DJ distinction entails two separate phases, forms the backbone of what might be termed the "Reichenbachian" received view.

Following the methodological suggestions of Thomas Nickles and Paul Hoyningen-Huene, we adopted a multifaceted approach to understanding the DJ distinction. This approach enabled us to articulate more precisely the philosophical implications embedded within the DJ distinction. By examining the distinction through the lens of its various features, we sought to provide a more comprehensive account of the philosophical underpinnings and implications of the DJ distinction, exploring how this conceptual framework intersects with broader philosophical questions about the nature of scientific inquiry, the role of history in shaping our understanding of science, and the extent to which philosophical analysis can maintain its autonomy in the face of interdisciplinary challenges. Through this exploration, we aimed to contribute to a deeper and more nuanced understanding of the philosophical dimensions of the DJ distinction and its significance for the philosophy of science.

Kuhn's analysis, particularly as articulated in his first Thalheimer Lecture, delineates a stark contrast between a static approach and a developmental approach within the philosophy of science. This dichotomy is not abstract; it is rather deeply tied to the methodologies employed in writing and understanding the *history* of science. In the static approach Kuhn criticizes, foundationalism and the pursuit of infallibility or certainty are paramount, reflecting a rigid, unchanging view of scientific knowledge. DJ distinction is justificatory for the very task of philosophy itself. To further elucidate the implications of this dichotomy, we referenced a critical paper by Larry Laudan from the 1980s, which scrutinizes the philosophical significance of the logic of discovery projects prevalent during that era. Laudan's critique, particularly aimed at Herbert Simon's logic of discovery project, echoes Kuhn's sentiments by offering a "historical" reconstruction of the philosophical debates surrounding the DJ distinction and historical attempts at a logic of discovery. Laudan's analysis, through the lens of different scientific methodologies, underscores how the specific problems tackled by scientific communities shape their methodological preferences and epistemic values.[9]

By means of the distinction, Laudan extends his critique to the contemporary logics of discovery, particularly criticizing the notion of "manufactured artifacts" as

9 In 1984 Laudan expanded the argument that links epistemic values and scientific methodologies in an interesting proposal (Laudan, 1984).

philosophically irrelevant. A main part of this critique is grounded in the identification of infallibility as a central value upheld by proponents of the logic of discovery in the 17th century, a value that Laudan and Kuhn both challenge.

In examining the DJ distinction through the features identified by Kuhn and Laudan, we turned our attention briefly to some general remarks on Herbert Simon's project, characterized by bounded rationality and heuristic search within problem-solving spaces. Contrary to the static approach's emphasis on coercive rules and infallibility, Simon's project embodies fallibilism, marking a significant departure from traditional views.

However, beyond methodological differences, a deeper divergence between the static and developmental approaches lies in the communal aspect of theory change and theory choice. This communal dimension, pivotal in the developmental approach, underscores the philosophical implications of a historiography of science that acknowledges the dynamic, collective nature of scientific inquiry. The developmental approach, therefore, offers a nuanced account of justification intimately connected to the evolving practices and shared understandings within scientific communities. Bounded rationality and heuristic search, when combined with a more sophisticated account of scientific practices and their history, can become new elements in the philosopher's toolkit to a more encompassing picture of the very complex social system that science ultimately is.

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