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Artículo de Investigación

Pablo Melogno and Thomas Kuhn

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Abstract

This paper discusses seven of Melogno's papers on Kuhn from 2019 to 2024. It analyzes Melogno's arguments about the relationship between Kuhn's early and late historiography, his rejection of the discovery-justification distinction, and his move towards questions of meaning and semantics in the 1980s. This paper argues that Melogno made vital contributions to "Kuhn studies," carefully tracing developments in Kuhn's thought over time. However, it also engages critically with some of Melogno's central claims, like his argument that *The Structure of Scientific Revolutions* represented a sharp break from Kuhn's earlier historiographical work. Overall, the paper concludes that Melogno's research significantly advanced understanding of Kuhn's philosophy and deserves to substantially influence future scholarships on Kuhn.

Keywords

Pablo Melogno, Thomas Kuhn, Historiography of Science, Discovery-Justification Distinction, Semantics, Taxonomy, Kuhn Studies, Incommensurability.

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Resumen
El artículo discute siete de los artículos de Melogno sobre Kuhn de 2019 a 2024. Se analizan los argumentos de Melogno sobre la relación entre la historiografía temprana y posterior de Kuhn, su rechazo de la distinción descubrimiento-justificación y su direccionamiento hacia cuestiones de significado y semántica en los años ochentas. Se argumenta que Melogno hizo contribuciones vitales a los "Kuhn Studies", realizando desarrollos cuidadosos sobre el pensamiento de Kuhn a través del tiempo. Sin embargo, se analizan también críticamente algunas de las afirmaciones centrales de Melogno, como su argumento de que *La estructura de las revoluciones científicas* representó un rompimiento respecto a la obra historiográfica más temprana. El artículo concluye que la investigación de Melogno hizo avanzar significativamente la comprensión de la filosofía de Kuhn y merece influir substancialmente en futuros estudios sobre Kuhn.

Palabras clave
Pablo Melogno, Thomas Kuhn, historiografía de la ciencia, distinción descubrimiento-justificación, semántica, taxonomía, Kuhn Studies, inconmensurabilidad.

Paul Hoyningen-Huene Is a theoretical physicist with a PhD and philosopher of science by training. He was a Visiting Scholar at M.I.T. with Thomas S. Kuhn, a Senior Visiting Fellow at the Center for Philosophy of Science of the University of Pittsburgh, Professor for History and Philosophy of Science at the Universität Konstanz, and Professor for Theoretical Philosophy at the Leibniz Universität Hannover, Germany. At the moment, he teaches philosophy of economics at the Economics Department of the University of Zurich, Switzerland. His best-known books are *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science* (Chicago, 1993), *Formal Logic: A Philosophical Approach* (Pittsburgh, 2004) and *Systematicity: The Nature of Science* (Oxford, 2013).

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1. Introduction

Pablo Melogno was a friend of mine, and I miss him very much. I met him first in July 2017, on my first trip to Uruguay and Argentina, and then on various occasions in Uruguay, Argentina, and Spain. We had countless conversations on philosophy, Kuhn in particular. I learned from him to understand Heavy Metal, which he loved. I also met his lovely wife and his lovely children - I am very sorry and wish them all the power that they need now.

Pablo Melogno was undoubtedly the most important Kuhn scholar in South America. I counted 20 publications dealing with Thomas Kuhn. The oldest is from 2011; the most recent one is from 2024 (posthumously). In 2019, Pablo began to publish on Kuhn in English. Pablo took into account and analyzed the discussion among other scholars with Thomas Kuhn, such as Donald Davidson, Paul Feyerabend, Robert Merton, Howard Sankey, Brad Wray, me, and many others. He had a good understanding of the relevant Kuhn literature. I shall comment on seven English Kuhn papers only, out of roughly 75 publications altogether; my Spanish is much too deficient to responsibly read philosophical papers written in Spanish. I shall treat these papers chronologically.

2. "Stepping into the 60s: Thomas Kuhn's intellectual turn towards the Philosophy of Science" (2019)

This paper was published together with Agustín Courtoisie in the journal *Daimon: Revista Internacional de Filosofía* (Melogno & Courtoisie, 2019). Because the journal *Daimon* is not very well-known outside of South America (in fact, I do not know how well-known it is within South America), this paper did probably not make much of an impact. It concerns the interesting question of Kuhn's development between 1956 and 1962, that is between his books *The Copernican Revolution* (Kuhn, 1957) and *The Structure of Scientific Revolutions* (Kuhn, 1962). Interestingly, Pablo took up this topic again three years later (Melogno, 2022), with very substantial changes relative to the 2019 paper, and this time as sole author. Even more interesting, in his 2022 paper he does not cite his 2019 paper at all. The reason may be, although this is completely speculative, that his (very senior) co-author may have influenced the 2019 paper in a way that Pablo disliked later and that, therefore, he took up the same subject again as sole author. Given Pablo's own implicit negative judgment of the 2019 paper, I shall not discuss it here.

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3. "The Discovery-Justification Distinction and the New Historiography of Science: On Thomas Kuhn's Thalheimer Lectures" (2019)

This paper was published in *HOPOS*, which is a highly visible journal because it is the only international journal fully specialized in the history of philosophy of science (Melogno, 2019). With the publication of this paper in *HOPOS,* several things that were relevant to Pablo's (much too short) career as a philosopher of science come to the fore. First, Pablo now sought international English-speaking audiences in the philosophy of science. Pablo must have realized that it is not enough to reach Spanish-speaking audiences with publications in Spanish, nor is it enough to reach the audiences that one reaches with English papers published in journals that publish mostly in Spanish (like *Daimon*). It is not enough because publications may not only enhance one's own reputation, but may also invite criticism, and the more profound the criticism is, the more one can learn from it. And because philosophy of science is completely internationalized with English as the dominant international language, the bigger the audience, the sharper, at least potentially, the criticism is. I hope that I have contributed to Pablo's decision to publish in *HOPOS* because, during my first visit to Uruguay and Argentina in 2017, I gained the impression that South American philosophy of science was in danger of provinciality (that I had experienced in the late 1960s in Germany) and that my new South American colleagues and friends should target the international English-speaking scene.

Second, Melogno (2019) shows that Pablo reached a quality level in philosophy of science that made him fit for the international scene. This is remarkable because his intellectual environment did not strongly support this. Again, I know from my own experience how difficult it is for someone coming from a country in which, in international comparison, the culture of philosophy of science is not very strong, to reach the international level. Clearly, in 2019 Pablo got to this breaking point. The well-known sociologist of science Robert Merton has described - curiously with respect to Thomas Kuhn - the respective process as an interplay between self-selection and institutional selection. The individual must make (possibly bold) decisions (submit a paper to a prestigious journal, for example), and the institution must accept it (Merton, 1977). This interplay is essential to any successful career.

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Third, Melogno (2019) demonstrates his engagement with Kuhn's *Thalheimer Lectures*, which Kuhn had given in 1984 in Baltimore. The manuscript of these four lectures unofficially circulated on the internet ever since, despite the injunction "Draft: Not for Distribution, Quotation or Paraphrase" on the first page of each lecture. Pablo, together with Hernán Miguel and Leandro Giri (also my friends from 2017 on, I may add), published a Spanish translation of the *Thalheimer Lectures* in 2017, which was important because these lectures showed Kuhn's development after the 1970s (Melogno et al., 2017). Since the late 1970s, Kuhn had been working on a book that he was, unfortunately, unable to finish and that appeared only posthumously (Kuhn & Mladenovic, 2022). The *Thalheimer Lectures* were a condensate of Kuhn's stage in 1984, with some innovations in comparison to his published writings.

Fourth, Melogno (2019) discusses Kuhn's criticism of the "context of discovery" versus "context of justification" distinction. This is important because the longest discussion of the context distinction in Kuhn's writings is found in the *Thalheimer Lectures* (Melogno et al., 2017); Kuhn's early criticism of the context distinction in *The Structure of Scientific Revolutions* is absolutely enigmatic (Kuhn, 1970 [1962], last two paragraphs of Chapter I, pp. 8-9), In fact, it is so enigmatic that in my first paper on the context distinction (Hoyningen-Huene, 1987), I did not discuss the most enigmatic passage from *Structure*, nor did I do that in my book on Kuhn (Hoyningen-Huene, 1993). The reason was simple: I did not understand what Kuhn was trying to say. I dared to attack the enigmatic passage of *Structure* only in my second paper on the context distinction (Hoyningen-Huene, 2006, pp. 124-126).

Pablo puts Kuhn's discussion of the context distinction into the relevant philosophical context. The context distinction is part and parcel of a philosophical stance, which is nicely exemplified by logical empiricism, which is called "the static approach" (Melogno, 2019, pp. 154-160). This stance is in opposition to the position that Kuhn is developing in his *Thalheimer Lectures*, "the dynamic approach" (Melogno, 2019, p. 167). And Pablo is right in saying that "it is clear that the level of detail and philosophical elaboration that Kuhn displays around the DJ [discovery-justification] distinction is more thought through in the Thalheimer Lecture than in the previous texts" (Melogno, 2019, p. 171). Pablo presents the result of his discussion in unambiguous terms: "For Kuhn, the DJ distinction is unacceptable since it does not fit with the results of historical research" (Melogno, 2019, p. 175). Pablo was correct.

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4. "From Externalism to Internalism: The Historiographic Development of Thomas Kuhn" (2022)

This paper was published in *Foundations of Science*, which is a very visible international journal (Melogno, 2022). As far as I can tell, this paper is the first one in Pablo's work that displays in full strength his main approach to Kuhn: a developmental perspective, here applied to the early Kuhn (later Pablo is going to apply it also to the late Kuhn, see the next section). His question is: is there continuity or discontinuity between Kuhn's *The Copernican Revolution* (Kuhn, 1957, henceforth *CR*) and his *Structure of Scientific Revolutions* (Kuhn, 1962, henceforth *SSR*)? Pablo objects to the standard story that there is full continuity between *CR* and *SSR* (Melogno, 2022, pp. 373-374). Pablo identifies two main sources of discontinuity between *CR* and *SSR*. First, *SSR* makes use of a conceptual apparatus that Kuhn only developed after the publication of *CR*, like "normal science" or "incommensurability". Second, *CR* is essentially a piece of externalist historiography, whereas *SSR* is internalist.

I disagree with Pablo because I think that there is indeed a fundamental continuity between CR and SSR despite the seemingly discontinuous elements.[1] Before I discuss Pablo's two points, I shall present my view of the relationship between *CR* and *SSR*. Undoubtedly, there are significant differences between the two works, but the essential explanation for these differences is not the factors that Pablo invokes but the *genre difference* between *CR* and *SSR*. Ironically, in his earlier paper on Kuhn's development from *CR* to *SSR* (Melogno & Courtoisie, 2019), uncited in the present paper, Pablo was aware of the genre differences and called it a difference between "case-based historiography" and "structural historiography" (Melogno & Courtoisie, 2019, p. 28). I shall not pause at this point to criticize this denomination of the difference but will immediately come to the heart of the matter, namely the substance of this difference. To which genre does *CR* belong?

Clearly, *CR* is a historiographic study of a particular episode in the history of science. Historiography proper *always* deals with particular historical episodes, or, to express this in an older terminology, is an "ideographic" enterprise, that is, it is directed at the uniqueness of historical

[1] In my talk at the symposium in Pablo's honor on July 25, 2023, at the CLMPS conference in Buenos Aires, I asked at this point the question: Shall I criticize Pablo *now*, at *this* symposium triggered by his untimely death? After all, *De mortuis nil nisi bonum.* I guess that Pablo would have answered: "The fact that I am dead now is no reason not to criticize me, Paul".

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phenomena.[2] To call this enterprise "case-based historiography" is not only somewhat pleonastic but also misleading. To speak of a "case-based historiography" suggests that the historical episode in question is a *case of something* and that something is some general kind, and the investigation of the episode aims at illustrating or establishing that the episode belongs to this kind. However, this is exactly what historians do not aim at: they want to present the episode completely in its own right, as an individual and unique process or event, and comparisons with other processes and events may only come later, if at all.

It is worthwhile to note that Kuhn's specific historiographic approach is *not* especially Kuhnian,[3] but follows the so-called "new internal historiography" that developed slowly from the 1930s on (see, e.g., Hoyningen-Huene, 1993, pp. 19-24). It begun to be institutionalized in the 1950s, to which Kuhn contributed (see, e.g., Hoyningen-Huene, 2001) and has been the mainstream historiographic tradition in history of science roughly since the 1970s. Despite its name, this historiography tradition also incorporates external factors, as for instance *CR* demonstrates (see below). However, the main resource for the explanation of the development of scientific *content* is to be found internally to science, and not in external political, religious, social, or economic factors.

By contrast, *SSR* is definitively not a historiographic study of this kind. *SSR* presents a philosophical reflection upon the results of the new internal historiography, aiming at generalizations about the historical development. In Kuhn's own unambiguous words: "This essay [*SSR*] aims to delineate [a new] image [of science] by making explicit some of the new historiography's implications" (Kuhn, 1970 [1962], p. 3). Kuhn is especially interested in the "structure" of scientific revolutions, as *SSR*'s title indicates, that are an essential part of a general "phase model" of the development of the basic natural sciences (Hoyningen-Huene, 1993, pp. 24-27). As *CR* is a historiographic study done in the spirit of the new internal historiography, it is highly plausible to assume that *CR* presents one of the cases upon which *SSR* reflects.

However, Pablo disagrees, because he sees more discontinuity between *CR* and *SSR*. His first argument consists of the observation that *CR* does not use the language of *SSR* such as

[2] Note that *all* phenomena are unique, also those investigated by the "nomothetic" sciences. However, it makes a difference whether the reproducible aspects of a phenomenon take center stage, like in the systematic social and natural sciences, or the unique aspects, like in the historical disciplines.

[3] I am stressing this because, in another paper, Pablo speaks of "Kuhnian historiography", which may create the impression that Kuhn's historiography of science is somehow special or even idiosyncratic: (Melogno, 2023b, pp. 44-45). I shall come back to this issue in the next section.

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"paradigm", "normal science", "incommensurability", etc. This is undoubtedly correct, but this difference can be fully and satisfactorily explained by the genre difference between *CR* and *SSR*. This is immediately seen by inspecting Kuhn's later book-length historiographic study on black-body radiation (Kuhn, 1978). Like in *CR*, Kuhn does not use the terminology of *SSR* in this study although it now clearly exists, and Kuhn himself explains later the absence of *SSR* terminology in his historical writings by their genre difference to *SSR*, see (Kuhn, 1984).

Pablo's second argument against the continuity between *CR* and *SSR* consists of the claim that *SSR* features an "internalist approach", whereas *CR* has an "externalist framework" (Melogno, 2022, p. 378). I strongly disagree with the view that *SSR* has an "internalist" approach in contrast to *CR;* for the full and detailed argument with many references to Kuhn's works, see (Hoyningen-Huene, 1993, p. 226, fn. 18). In fact, *SSR* explicitly acknowledges the relevance of external factors, especially with respect to the timing of crises, but also with respect to the range of alternatives available to revolutionaries, and thus their relevance to the content of science:

> The need for drastic condensation has […} forced me to forego discussion of a number of major problems. […] I have said nothing about the role […] of external social, economic, and intellectual conditions in the development of the sciences. One need, however, look no further than Copernicus and the calendar to discover that external conditions may help to transform a mere anomaly into a source of acute crisis. The same example would illustrate the way in which conditions outside the sciences may influence the range of alternatives available to the man who seeks to end a crisis by proposing one or another revolutionary reform [footnote 4]. Explicit consideration of effects like these would not, I think, modify the main theses developed in this essay (Kuhn 1970 [1962], p. X; see also p. 69).

In footnote 4 to this quote of *SSR*, Kuhn writes:

> These [external] factors are discussed in *CR*. Other effects of external intellectual and economic conditions upon substantive scientific development are illustrated in my papers (Kuhn, 1959), (Kuhn, 1960), and (Kuhn, 1961). It is, therefore, *only with respect to the problems discussed in this essay* that I take the role of external factors to be minor (Kuhn, 1970 [1962], p. X, fn. 4, my italics).

Therefore, there is no break whatsoever visible between the historiography practices in his own historical work and the historical works that constitute the basis of *SSR*. As Kuhn says, "only with respect to the problems discussed in [*SSR*]", the role of external factors is minor (Kuhn, 1970 [1962], p. X). Why is this so? Because in *SSR*, Kuhn develops a general phase model of scientific

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development, and the parameters that define this model (the three different phases and the mechanisms that drive a scientific field from one phase to the next) are largely independent of potential external factors. In other words, it is the genre difference between historiographic studies and *SSR* that determines the difference in importance of the external factors.

In addition, Kuhn claims in *SSR* that the case of the Copernican Revolution is somehow special with respect to the importance of external factors: "[…] for the case of Copernicus in which factors external to science played a particularly large role" (Kuhn, 1970 [1962], p. 75). This clearly contradicts Pablo's claim that "in *SSR* the Copernican Revolution is understood as a process restricted to the internal functioning of a scientific community" (Melogno, 2022, p. 382). Still, in spite of the disproportionate strength of external factors in comparison to other examples, the Copernican Revolution is clearly representative of a scientific revolution in the sense of *SSR*: Kuhn refers in the same breath to "such paradigm shifts as the Copernican, Newtonian, chemical, and Einsteinian revolutions" (Kuhn, 1970 [1962], p. 66).

In sum, I disagree with Pablo regarding the relationship between *CR* and *SSR*. I think that the differences between these two books are fully explained by their genre difference, and no discontinuity in Kuhn's thought must be postulated to come to terms with their difference. Unfortunately, I cannot resolve my disagreement with Pablo.

5. "A Vindication of Structure in *Structure of Scientific Revolutions*: A Comment to K. Brad Wray" (2023b)

This paper (Melogno, 2023b) has been published in *Perspectives on Kuhn*, a volume that was edited by Leandro Giri, Pablo, and Hernán Miguel, based on the *II Colloquium in the Philosophy and History of Science Río de la Plata 2018* with the title "Kuhnian Studies: Past, Present and Future". The organization of this international colloquium in Buenos Aires and Montevideo with distinguished speakers, and the subsequent publication of its proceedings with Springer, show the successful ambition of Pablo, Leandro, and Hernán to be present on the international Kuhn scene. The conference featured talks and commentaries, and Pablo's paper was a commentary on Wray's paper, who in his talk (and paper) had defended Kuhn's notion of "structure" in Kuhn's *SSR* against an attack by noted historian Lorraine Daston. Daston had claimed that "probably no word strikes today's historians of science reading Kuhn (if they do) as more dusty and dated than the once

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glittering 'structure'" (Daston, 2016, p. 116). Why is this so? "Most historians of science no longer believe that *any* kind of structure could possibly do justice to their subject matter. The very idea of looking for overarching regularities in the history of science seems bizarre" (Daston, 2016, p. 117). In his paper, Pablo now seconds Wray in defending the concept of structure as something useful for the philosophy of science and the history of science, although his defense is somewhat different from Wray's.

I find the whole discussion between Daston, Wray, and Pablo somewhat awkward. If all parties took seriously the genre distinction between historiographic work and *SSR* that I developed in the previous section and became a little humbler, the conflict would disappear.

First of all, Daston is absolutely right in her description of the enterprise of historiography of science. It would, in fact, be rather surprising if a leading and very reflective historian of science would mischaracterize her own field. She is right in stating that "[m]ost historians of science no longer believe that *any* kind of structure could possibly do justice to their subject matter. […] Since roughly the 1990s, the focus […] has shifted from the streamlined to the dense and detailed; the professed aim has been to 'complexify' rather than simplify and to reveal variability rather than uniformity" (Daston, 2016, p. 117). This means that any kinds of generalizations over historical processes – Pablo calls them "historical regularities" (Melogno, 2023b, p. 45), if they exist at all, would not be useful for historians of science. Such generalizations would have no explanatory power for the intricate details of the respective historical process in which the historian is interested, because they are, if they exist at all, much too abstract.[4]

If other people, however, philosophers of science, for instance, are interested in such highly abstract generalizations – Pablo goes so far as to call them "formal, or at least they have a high degree of formality" (Melogno, 2023b, p. 46), why should historians of science attack them? Historians can sit back and relax, and possibly observe the philosophers' efforts, anticipating that the philosophers will either find nothing or, if they find something, it will be so abstract that it will be completely useless to the historians of science. But, there are two dangers for philosophers involved in this genre. First, they should be careful not to sell their product under misleading labels,

[4] This is a problem that philosophers regularly encounter when dealing with a kind of things of very heterogeneous character, like when asking the question "What is science?". Because the sciences are so different from one another, any answer covering them all can only be highly abstract and "thin". When writing *Systematicity: The Nature of Science* (Hoyningen-Huene, 2013), I often had the feeling of walking a very fine line between abstract and thin on the one hand and vacuous on the other.

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like "Kuhnian historiography", as Pablo does (Melogno, 2023b, pp. 44-45, 47, 48, 49). This will displease many historians because they immediately identify the philosophers' products as lightyears away from their own products, and therefore the label "historiography" as encroaching. Second, the philosophers should also be very reluctant to convey suggestions to professional historians on how historiography of science should be done. For instance, Pablo stated that it "would perhaps seem more useful and fruitful if the historians of the future – once again – became more interested in the skeleton of history and less attentive to the cells" (Melogno, 2023b, p. 49). Historians of science, scientists, and humanists are usually highly allergic to philosophers advising them on how they should practice their profession. Who knows more about the profession, the practitioners or the philosophers?

6. "Towards a Genealogy of Thomas Kuhn's Semantics" (2023)

This paper was co-authored with Leandro Giri, and it appeared posthumously in the prestigious international journal *Perspectives on Science* (Melogno & Giri, 2023). My remarks will be rather brief because I have not yet sufficiently studied the two pieces that are the subject matter of this paper, namely, Kuhn's Lowell Lectures of 1951 and his Notre Dame Lectures from 1980. The paper tries to reconstruct the genealogy of Kuhn's semantic theories from 1951 to 1980. The Lowell Lectures, entitled *The Quest for Physical Theory*, were edited in 2012 by Gorge Reisch (2021), whereas the Notre Dame Lectures entitled *The Natures of Conceptual Change* are only available in the Distinctive Collections Section of the Massachusetts Institute of Technology Libraries.

Essentially, I think that the paper has a good topic because both lectures are understudied. Furthermore, I share the hypothesis of the paper that there is more continuity in Kuhn than is assumed by the postulate of a "linguistic turn" in Kuhn's thinking (Melogno & Giri, 2023, p. 385). Instead, in agreement with Reisch (2021, p. xx), Melogno and Giri propose "that Kuhn's linguistic turn in his late work should be understood as renewing an interest that existed before *SSR* was published and not as venturing into an unexplored field" (Melogno & Giri, 2023, p. 387). The assumption here is that the "agenda of semantic concerns and problems linked to the change of meaning in scientific revolutions" was "set aside in SSR and then reactivated" (Melogno & Giri, 2023, p. 387). I am not sure that this characterization is correct. It is, of course, correct that in *SSR*

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Kuhn emphasizes perception and gestalt switches and their role in scientific revolutions, an emphasis that is later replaced by an emphasis on the linguistic aspects of revolutions. However, also in *SSR,* Kuhn characterizes scientific revolutions as "conceptual transformations" or as "a displacement of the conceptual network through which scientists view the world (Kuhn, 1970 [1962], p. 102). Also, his refusal of a neutral observation-language (Kuhn, 1970 [1962], pp. 125, 145) shows that in *SSR* Kuhn is fully aware of the semantic dimension of scientific revolutions. It is much more a downgrading of the possible perceptual aspects of scientific revolutions than a direct upgrading of the linguistic aspects of revolutions that happens in his post *SSR* work. So, I am somewhat skeptical that his work in the late 1970s and early 1980s can be described as "a return to his philosophical roots, which can be found in his Lowell Lectures" (Melogno & Giri, 2023, p. 387). However, these remarks are rather tentative.

7. "Kuhn's 'The Natures of Conceptual Change': the search for a theory of meaning and the birth of taxonomies (1980 -1994)" (2023a)

This paper (Melogno, 2023a) was posthumously published in *International Studies in the Philosophy of Science* which is a highly visible international journal. In my view, this is Pablo's masterpiece, and it will, or rather should, substantially influence future Kuhn studies.[5]

The main texts that Pablo analyses in this paper are Kuhn's Notre Dame Lectures from 1980 (see also above, Section 6) and Kuhn's unfinished book *The Plurality of Worlds: An Evolutionary Theory of Scientific Development*, contained in the recently published collection (Mladenovic, 2022). Pablo's main interest is, as the title describes it, the birth of one of the central concepts in Kuhn's late semantics, the concept of a taxonomy. The background of Kuhn's journey into

[5] The term "Kuhn studies" was recently introduced by Brad Wray: "These are proper scholarly studies of Kuhn's views, with the aim of understanding the development of his views and assessing the influences on, and subsequent impact of, his work"; they form "a new subfield in the history of philosophy of science" (Wray, 2024, p. 15). Wray credits me with founding this subfield: "The pioneering study of this sort is, without a doubt, Paul Hoyningen-Huene's *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*" (Wray, 2024, p. 15). Wray may be right that this book indeed triggered the field of Kuhn studies in the given sense, but I never understood my book as a contribution to the history of philosophy of science, i.e., as a part of Kuhn studies. Rather, my intention was, as the title says, to *reconstruct* Kuhn's philosophy of science. In his *On the Reconstruction of Historical Materialism* (Habermas, 1976), Jürgen Habermas nicely describes what the pertinent reconstructions aim at: "In our context, reconstruction means that a theory is taken apart and reassembled in a new form in order to better achieve the goal it has set itself" (Habermas, 1976, p. 9). Such work includes criticism and amendments. Also Kuhn did not understand my work as belonging to Kuhn studies in Wray's sense: "As a philosopher, Hoyningen is concerned not so much with the development of ideas as with the ideas developed" (Kuhn, 1993, p. xi).

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semantics was his frustration with the reception of his incommensurability concept, which he found very seriously lacking. "[V]irtually no-one has fully faced the difficulties that led Feyerabend and me to speak of incommensurability", said a resigning Kuhn in his paper in the symposium entitled "Commensurability, Comparability, Communicability" at the 1982 Biennial Meeting of the Philosophy of Science Association (Kuhn, 1983, p. 669). The root of the problem was that the existing semantic theories could not accommodate incommensurability, and hence, people thinking and theorizing in terms of these existing semantic theories were unable to understand what Kuhn was after.

Pablo rightly sees Kuhn's 1980 Notre Dame Lectures as the first installment of a trio of lectures that presented Kuhn's intellectual development from the late 1970s on that he tried to bring into the form of a book. The other two lectures are the Thalheimer Lectures of 1984 and the Shearman Lectures of 1987. In his paper, Pablo devotes a section to each of the three Notre Dame Lectures and structures and analyses their content. He then follows up on each individual topic in Kuhn's later lectures and his unfinished book. An instance of such a topic is Kuhn's "Aristotle episode", that is Kuhn's experience when reading Aristotle's *Physics* in the summer of 1947, which he first conveyed in the Preface to his *Essential Tension* (Kuhn, 1977, pp. xi-xii). Pablo investigates how the narratives of the episode vary in the body of literature under scrutiny, and takes these variations as possible indicators of changes in Kuhn's thought (Melogno, 2023a, pp. 88-90). This is a very subtle and fruitful hermeneutic procedure, uncovering, exactly in Wray's sense of "Kuhn studies", the historical development of Kuhn's thought. Other examples of topics in Kuhn's work that Pablo follows up include the invention of the Voltaic pile, Kuhn's criticism of descriptivist theories of meaning, the Johnny example (first described in works of Kuhn written in the late 1960s), the taxonomic structure of scientific language, the causal theory of reference, and many more.

In my view, Pablo's paper is a paradigm for Kuhn studies for years to come, such that the "partial overview" of "Kuhn's unpublished papers 1980-1994" that Pablo presents as Figure 2 of his paper can be completed. This map will reveal the subtlety and slow but steady progress of Kuhn's thought in the last period of his life.

8. "Normal Science: The Rise and Fall of Scientific Traditions" (2024)

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This paper was posthumously published in 2024 in the volume *Kuhn's The Structure of Scientific Revolutions at 60*, edited by Brad Wray, the most active participant in the discussion of Kuhn's work in recent years. The appearance of Pablo's paper (Melogno, 2024) in this volume demonstrates again that he had reached the level of the international community of Kuhn scholars because contributions to this volume were by invitation only. To put it frankly, I am somewhat disappointed with this paper because I learned nothing new, in contrast to some of his other papers. Pablo must have thought that his clear and comprehensive survey of normal science was useful because, although being "one of the core concepts in Kuhn's philosophy", normal science "has not received the same exegetic attention as paradigms, incommensurability, and scientific revolutions" (Melogno, 2024, p. 79). However, on the next page, he rightly declares: "The concept of normal science has been the target of numerous analyses and reconstructions" (Melogno, 2024, p. 80), and he references works by Hoyningen-Huene, Bird, Nickles, Worrall, Marcum, and Goodwin. Still, the number of pages written on normal science is probably smaller than the number of pages written on the other mentioned Kuhn topics.

9. Conclusion

It is a great loss for international philosophy of science that Pablo Melogno died so early. In a comparatively short time, he very substantially contributed to Kuhn studies, that is a careful examination of Kuhn's intellectual development, based on published and unpublished sources. These studies enable a better understanding of Kuhn's developing philosophical position about science and its development. As I believe that the subtlety of Kuhn's thought is still underrated in many circles, including the average philosopher of science, Kuhn studies may contribute to an improvement of this situation. Pablo was an important part of that.

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