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Theodore and Grace de Laguna's Philosophies of Science: Lessons about Popper, Kuhn, and the History of Philosophy of Science

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Abstract: I present the two philosophies of science developed by Theodore de Laguna and Grace Andrus de Laguna in America before the 1930s arrival of the logical positivists there. I also provide a contextualised comparison of these pre-logical positivist views with the post-positivist views of Karl Popper and Thomas Kuhn. We will see that the de Lagunas articulate the key influential aspects of the philosophies of science of Popper and Kuhn and, with the help of a more developed theoretical framework than that of Popper or Kuhn, address what came to be key challenges to these later thinkers' views. We will also see that "Kuhnian" ideas were ubiquitous in America during the 1930s and 1940s. These observations allow me to further support the recent thesis that the logical positivists were proposing a narrowing down and winding back of philosophy of science when they arrived in America. Indeed, it will be challenging to rationally reconstruct the history of the philosophy of science which developed later. It was still, even after Kuhn in the early 1960s, playing catch up with the speculative tradition to which the de Lagunas belonged. My observations also suggest that Kuhn was an important conduit through which ideas from the speculative tradition made their way into analytic philosophy of science.

Keywords:

American philosophy;
Grace A. de Laguna;
history of philosophy of science;
logical positivism;
Theodore de Laguna;
women in philosophy

1. Introduction

It is standard to view twentieth century analytic philosophy of science as largely developing out of, and in response to, logical positivism after its arrival in (North-)America in the 1930s. Particularly important was Karl Raimund Popper's proposal of what is widely taken to be the first deductivist epistemology of science–that is, the first version of the view that scientific inference is deductive inference based solely on empirical premises–in response to the logical positivists' inductivism. While the logical positivists and others realised the importance of deduction for evaluating empirical claims and denied synthetic a priori knowledge a role in doing so, Popper, the story goes, was the first to insist that ampliative inference and synthetic a priori knowledge have no role in doing so. No less important, on this story, was Thomas Kuhn's early 1960s, history-informed philosophy of science, which countered logical positivism's emphasis on logical analysis as the sole method of philosophy of science and problematised Popper's variant of deductivism. In addition to the development of history-informed philosophy of science, Kuhn's originality is commonly taken to be in distinguishing between normal and revolutionary science, giving exemplary solutions to scientific problems a primary role in driving the development of normal science, giving social factors a role in driving revolutionary science, and taking revolutionary science to be non-cumulative. Other figures, such as Paul Feyerabend, also helped to initiate this history-informed, post-positivist philosophy of science. [1]

Krist Vaesen and I, however, have drawn attention to American philosophy of science before the logical positivists' arrival in America. While only key pragmatists, most notably John Dewey, tend to be recognised as contributing to American philosophy of science during this period, we argue that there already were dozens of philosophers of science working within what was a distinct subdiscipline of American philosophy, that most of these were speculative philosophers of science, and that only a few philosophers of science identified as pragmatists. We also describe ways in which speculative philosophy of science influenced later analytic philosophy of science. In particular, we note that, when shorn of its speculative

[1] See James Ladyman, "The History of Philosophy of Science", in: Kelly Becker and Iain D. Thompson (eds.), **The Cambridge History of Philosophy, 1945-2015**, Cambridge University Press, Cambridge, pp. 189–209; Brian Hepburn and Hanne Andersen, "Scientific Method", in: Edward N. Zalta (ed.), **The Stanford Encyclopedia of Philosophy**, Summer 2021 Edition, https://tiny.pl/j9snxfc3 [15.10.2025].

side, Morris Raphael Cohen's **Reason and Nature** [2] provided, via its influence on his student Ernest Nagel, a paradigm for analytic philosophy of science; the paradigm set was for, among other things, the standard topics in general philosophy of science. [3] Our thesis about the existence of a pre-logical positivist, distinctively American brand of philosophy of science is supported by Sander Verhaegh. [4]

Speculative philosophers of science typically took philosophy of science to include the logic of science and speculative metaphysics. The latter was conceived of as providing visions of reality that are informed by, but also substantively independent of, science. A logic of science is roughly what we would call "an epistemology of science" and aims to provide an understanding of scientific judgement and inference as it is exemplified in systems of scientific knowledge. The logic of science is thus concerned with judgement and inference in scientific activities such as classification, explanation, and experimentation. [5] "Judgement" should here be taken to include any type of intentional state in which a positive or negative attitude is adopted towards the content of a representation. So, types of judgement include full belief and partial belief, where these are states in which contents are taken to be unqualifiedly true, and acceptance, where acceptance need only involve taking contents to be partially true.

A key concern of the speculative philosopher of science was which types of states are those of scientific judgement. In addition, even well before the 1930s, the logic of science included the other key issues familiar from post-logical positivist American philosophy of science, including the problem of induction, the problem of demarcation, the theory laden nature of observation, the nature of sci-

[2] See Morris R. Cohen, **Reason and Nature: An Essay on the Meaning of the Scientific Method**, Harcourt, Brace and Company, New York 1931.

[3] See Joel Katzav and Krist Vaesen, "The Rise of Logical Empiricist Philosophy of Science and the Fate of Speculative Philosophy of Science", *HOPOS* 2022, Vol. 12, No. 2, pp. 327–358, https://doi.org/10.1086/721135.

[4] See Sander Verhaegh, "The Reception of Relativity in American Philosophy", *Philosophy of Science* 2024, Vol. 91, No. 2, pp. 468–487, https://doi:10.1017/psa.2023.85.

[5] See Harold R. Smart, **The Logic of Science**, D. Appleton and Company, New York 1931; Joel Katzav, "Speculative Philosophy of Science vs. Logical Positivism: Preliminary Round", in: Sander Verheagh (ed.), **American Philosophy and the Intellectual Migration: Pragmatism, Logical Empiricism, Phenomenology, Critical Theory**, "De Gruyter History of Philosophy and Science", Vol. 1, De Gruyter, Berlin — Boston 2025, pp. 53–76, https://doi.org/10.1515/9783111335209-005; Katzav and Vaesen, "The Rise of Logical Empiricist Philosophy of Science…".

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entific explanation, laws of nature, reductivism in science, probability in science, idealisation in science, hypothesis choice in the face of exceptions or, in current terms, "the methodology of research programmes", and more. [6]

In earlier work, I provide some detail about the development, during the early decades of the twentieth century, of three research programmes in speculative philosophy of science. [7] I trace the programmes from Edgar Arthur Singer Jr.'s work at the turn of that century to subsequent work by, among others, Cohen, Grace Andrus de Laguna, Theodore de Laguna (Grace's husband), and Harrold Robert Smart. I appeal to the development of ideas by these authors to argue that the positivists were effectively proposing a narrowing down of philosophy of science and that this proposal to some extent won the day. While speculative philosophers had longstanding research programs on verificationism and other positions that positivists were investigating in the 1930s, the influential program that the positivists brought with them to America at the time tended to exclude, either as a matter of principle or practice, metaphysics, programs such as the one on scientific explanation, history of science informed philosophy of science, and philosophical hypotheses that are substantive and empirical. [8]

The present paper aims to further our understanding of American speculative

[6] See Katzav and Vaesen, "The Rise of Logical Empiricist Philosophy of Science…".

To keep things simple, I mostly use "hypothesis" to refer to the contents of scientists' attitudes while recognising that exactly what these contents are, e.g., theories, laws, or models, will vary according to conception of science and focus of discussion.

[7] See Joel Katzav, "Speculative Philosophy of Science vs. Logical Positivism...".

[8] My claims about what logical positivism tended to exclude are solely about the influential program from the 1930s. The claims are thus compatible with the fact that Carl Gustav Hempel broadened the program in the 1940s by discussing the problem of scientific explanation. So too, my claims are compatible with the observation that some logical positivists had an interest in the history of science in the 1930s. For this interest neither had an impact in America nor became part of the logical positivist program (see Katzav, "Speculative Philosophy of Science vs. Logical Positivism…"). In any case, the interest some logical positivists had in the history of science is compatible with the exclusion of history-based philosophy of science from philosophy. For example, Philipp Frank's historical work in the 1930s involved using logical analysis to show that historically significant physics presupposes no metaphysics and history to support the historical thesis that physics makes this increasingly clearer (see Philipp Frank, **Between Physics and Philosophy**, Harvard University Press, Cambridge (Mass.) 1941, https://doi.org/10.4159/harvard.9780674331976). He did not use history to develop and support philosophical claims. For further support for my reading of what the logical positivists excluded from philosophy, see, Katzav "Speculative Philosophy of Science vs. Logical Positivism…".

philosophy of science by examining the two logics of science developed by the de Lagunas in the first three decades of the twentieth century. I will present the theoretical framework within which these logics were developed and some detail about what they tell us about the general nature of scientific judgement and about how judgements of scientific communities are maintained and revised. I also aim to compare these logics with Popper's falsificationism from the mid-1930s and Kuhn's early 1960s philosophy of science, including to see how the earlier positions provide responses to key challenges to the work of Popper and Kuhn. The comparisons will, in turn, help to illuminate the origins of analytic philosophy of science and to evaluate the extent to which the history of anglophone philosophy of science is amenable to rational reconstruction. In order to show that my comparisons are meaningful, I contextualise the work of the de Lagunas, Popper, and Kuhn. The contextualisation examines the problem situation to which the de Lagunas, some of the other American philosophers of science working alongside them, and Popper were responding. [9] So too, the contextualisation situates Kuhn's work in relation to a broad swath of work by American speculative philosophers of science in the 1930s and 1940s as well as that of two Europeans, Ludwik Fleck and Michael Polanyi. Examining the problem situations driving Popper and his American counterparts allows me to evaluate whether my comparison of their positions and my subsequent conclusions about the significance of the introduction of his work into analytic philosophy are justified. Situating Kuhn's work in its American context allows examining the influences on his work and the significance of its introduction into analytic philosophy.

We will see that the theoretical framework within which the de Lagunas develop their logics of science is an evolutionary one that is most extensively developed in T. de Laguna's 1926 book **The Factors of Social Evolution** [10] (henceforth, **Factors**) but that is already exemplified by the characterisation of science in the couple's joint 1910 book **Dogmatism and Evolution: Studies in Modern Philos-**

[9] I will only examine positions similar to Popper's insofar as they are needed to illuminate the problem situation he shared with his American counterparts. I will, accordingly, not be considering how his views relate to those of figures who influenced him but, like Victor Kraft (see Victor Kraft, **Die Grundformen der wissenschaftlichen Methoden**, Hölder-Pichler-Tempsky A.-G., Vienna — Leipzig 1925), were not well known in America.

[10] See Theodore De Laguna, **The Factors of Social Evolution**, F. S. Crofts and Co., New York 1926.

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ophy [11] (henceforth, **Dogmatism**). [12] We will also see that the logic of science developed in this early book is plausibly thought of as a form of deductivism. Although a joint position, I will call it *Gdeductivism* because it is developed in the part of **Dogmatism**–part III–that is primarily due to G. de Laguna and because key parts of it are found in her earlier work [13] but not in that of her husband. [14] The second of the logics of science is explicitly deductivist and is found in **Factors** and two papers by T. de Laguna from 1930. I will call this position *Tdeductivism*. Gdeductivism and Tdeductivism share the further thesis that scientific development is not cumulative. Gdeductivism departs from Tdeductivism in claiming that scientific assessment is of partial truth, and thus of adequacy for specific purposes. Tdeductivism goes beyond Gdeductivism in claiming that scientific revolutions involve competitions between radical and conservative factions that are ultimately resolved by social factors rather than reasoned argument.

The comparative discussion will allow us to see that the de Lagunas develop all the key ideas associated with Popper and Kuhn's philosophies of science, including Popper's deductivism and Kuhn's history-informed view that scientific revolutions are not cumulative and are sociologically driven. The comparison will also allow us to see that the de Lagunas' positions substantially differ from, and indeed are in some ways more sophisticated than, those of their later counterparts. The de Lagunas have in their evolutionary epistemology a more developed framework for philosophy of science than Popper or Kuhn introduced into ana-

[11] See Theodore De Laguna and Grace A. De Laguna, **Dogmatism and Evolution: Studies in Modern Philosophy**, The MacMillan Company, New York 1910.

[12] Those reading **Factors** should be aware that its discussion of innate, race-related differences in intelligence between humans (see T. De Laguna, **The Factors of Social Evolution...**, pp. 99–133) uses language that reflects the prejudices of the time, though the discussion rejects the case for such differences.

[13] See Grace M. Andrus, "Professor Bawden's Interpretation of the Physical and the Psychical", *The Philosophical Review* 1904, Vol. 13, No. 4, pp. 429–444, https://doi.org/10.2307/2176910; Grace A. De Laguna, "The Practical Character of Reality", *The Philosophical Review* 1909, Vol. 18, No. 4, pp. 396–415, https://doi.org/10.2307/2177776.

[14] I take part III to be primarily due to G. de Laguna because of the book's preface. It states that one of the authors of the book had to withdraw from writing part-III and that, as a result, that part of the book does not continue the book's discussion of internal relations. The topic of internal relations is one that T. de Laguna publishes on shortly after the completion of **Dogmatism** (see Theodore De Laguna, "The Externality of Relations", *The Philosophical Review* 1911, Vol. 20, No. 6, pp. 610–621, https://doi.org/10.2307/2178010) but about which G. de Laguna never writes.

lytic philosophy of science, ready responses to some of the key, persistent challenges that Popper and Kuhn came to face, and important positions, such as the thesis that scientific assessment is of partial truth, that only started to appear within analytic philosophy of science in the late 1960s. Overall, this supports my claim that logical positivists were, to some extent successfully, proposing a narrowing down and regress in the development of philosophy of science. Indeed, the analytic philosophy of science that is commonly supposed to have grown up out of logical positivism was still playing catch up with speculative philosophy of science in the 1960s.

The contextualisation of the four philosophies of science that are the focus of this paper will, finally, allow us to see that the de Lagunas and Popper were responding to largely overlapping problem situations and thus that it makes sense to compare their views as to how successful they were. So too, we will see that the de Lagunas were far from being alone among American speculative philosophers of science in developing key ideas taken up by Kuhn. As a result, while noting the potential European influences of figures such as Fleck and Polanyi on Kuhn, I will argue that it is likely that he imported American speculative philosophy of science into analytic philosophy.

I will, in section 2, outline the de Lagunas' framework for understanding social evolution, with an emphasis on how it applies to science. In section 3, I will outline the de Lagunas' two views of the logic of science, beginning with T. de Laguna's later one. Section 4 will then compare these logics with the work of Popper and Kuhn. Section 5 will contextualise the four philosophes of science that are the focus of my paper. My conclusion is in section 6.

2. An evolutionary framework for the philosophy of science

T. de Lagunas' **Factors** aims primarily to articulate an evolutionary framework for understanding science and other social phenomena. According to this framework, social evolution is typically a two-stage process. The first involves differentiation between types of system and the second, subsequent one involves increasing integration of types of elements within systems. Scientific change is taken to be a species of social evolution the first stage of which involves increasing differentiation between types of systems of scientific knowledge, where each

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such type is characterised by its distinct elements, including methods and principles/concepts. An example of differentiation between systems of scientific knowledge is the differentiation between the special sciences. The second stage of scientific evolution involves the increasing integration of the methods and principles/concepts associated with each type of system of scientific knowledge. Biology, for example, developed a high degree of unity due to its success at conceptualising the cell. [15]

Importantly, **Factors** does not claim that social evolution always requires differentiation and integration. In some cases, such evolution can involve integration but no significant differentiation. [16] What differentiates social and biological evolution, according to T. de Laguna, is nevertheless partly that the latter includes a broader range of patterns of change. Biological evolution includes change that is not in the direction of integration or differentiation. [17]

The increased integration of types of systems in social evolution comes with an increase in their complexity. Thus, within systems of scientific knowledge, there is an increase in the number of elements with distinct but interdependent structures and functions. The increase in complexity, in turn, involves an increase in efficiency of functioning. In the case of scientific knowledge, there tends to be a higher degree of truth and certainty of application, as well as improvements in techniques introduced into human activities. [18] The reason increased integration leads to increased efficiency is that integration is functional, so that increased integration involves the different elements of a type functioning together to a greater extent. [19]

The results of social evolution are types that are (to varying degrees) adapted to their environments, which in science are fields of investigation. Adaptation in science is facilitated by, among other things, interdependence between types of knowledge. A particular system of knowledge succeeds in producing knowledge of

[15] See T. De Laguna, **The Factors of Social Evolution...**, pp. 34–36.

[16] See T. De Laguna, **The Factors of Social Evolution...**, pp. 43–44.

[17] See T. De Laguna, **The Factors of Social Evolution...**, p. 36.

[18] See T. De Laguna, **The Factors of Social Evolution...**, p. 37.

[19] See T. De Laguna, **The Factors of Social Evolution...**, p. 39.

its subject matter partly because it is assisted by other types of knowledge, as, for example, physics is assisted by mathematics. [20]

T. de Laguna emphasizes a number of further factors of social evolution that are particularly important for understanding science. I will describe two of these. First, he claims that while evolution's stages are processes of change, they can only occur against a relatively stable background. [21] For this reason, evolution in science is not cumulative in that it is simply the replacement of error with truth. Rather, it is a continuous process in which error is reduced. [22] Even in great scientific revolutions, such as the transition to a heliocentric view of the solar system, only some of the content of old theories is completely rejected. Much is reinterpreted and, thus transformed, incorporated in new conceptual systems. [23] Scientific evolution is thus continuous in that it is a transformation of knowledge against a background of continuity of knowledge and problems. [24]

Second, a key factor of social evolution is conflict. According to **Factors**, social evolution is invariably a change in valuations by those involved. In some respect, the evolution is thus judged to be better or worse. Further, "no social evolution takes place without conflict between conservative and radical tendencies". [25] Science too is subject to such conflict. [26] We will return to filling out the details of T. de Lagunas understanding of this conflict in the next section.

Factors' evolutionary view of science is already articulated in **Dogmatism**. In this book, the de Lagunas take the evolution of science to be a form of social evolution. [27] Further, they present the evolution of science as involving the differentiation of the system of scientific knowledge into a variety of special sciences with their characteristic types of knowledge but also as involving the increased integration of the systems of knowledge of each special science. More explicitly, re-

[20] See T. De Laguna, **The Factors of Social Evolution...**, pp. 40–41.

[21] See T. De Laguna, **The Factors of Social Evolution...**, p. 68.

[22] See T. De Laguna, **The Factors of Social Evolution...**, pp. 75–76.

[23] See T. De Laguna, **The Factors of Social Evolution...**, pp. 77–78.

[24] See T. De Laguna, **The Factors of Social Evolution...**, pp. 82–83.

[25] T. De Laguna, **The Factors of Social Evolution...**, p. 96.

[26] See T. De Laguna, **The Factors of Social Evolution...**, pp. 94–96.

[27] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 194.

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garding the differential side of evolution, the de Lagunas tell us that the special sciences gradually evolve out of common sense and that this evolution is an evolution of human judgement, including an evolution of distinctive types of concepts and methodologies. [28] For example, the adoption of the concept of evolution in biology meant not just the adoption of a representation of a certain type of change but also "no less than a whole new principle of classification, almost one might claim, of scientific procedure". [29] The integrative part of scientific revolution, according to the de Lagunas, involves developing increasingly integrated systems of concepts and associated methodologies within each special science. [30] The de Lagunas write that, as science develops, each special science

> becomes a system relatively independent of the great body of cognitive experience. The increasing determinateness of its peculiar field, the increasing definiteness of its peculiar presuppositions, impart a high degree of stability to its distinctive concepts. [31]

The idea that scientific change, including scientific revolutions, is not simply the replacement of error by truth but rather the reduction of error is also already found in early work by the de Lagunas. G. de Laguna clearly articulates it before **Dogmatism**. [32] In **Dogmatism**, the de Lagunas write that

> the progress of science is a true evolution, an organic growth, in which no part is wholly unaffected. Time-honored formulae, even if unrefuted, are narrowed in their field of application, or, by inclusion in more comprehensive generalizations, become possessed of a new significance. Thus, while two and two still make four and doubtless will continue to do so, the science of arithmetic has had a new birth and the general conception of number itself has been transformed, since the establishment by Cantor of the existence of distinct "transfinite" numbers. [33]

Finally, as we will see, **Dogmatism** does not put forward the thesis that science progresses because of social conflict between radical and conservative ten-

[28] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 158–161 and 198–201.

[29] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 199.

[30] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 198–201.

[31] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 200.

[32] See G. A. De Laguna, "The Practical Character of Reality...".

[33] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 18.

dencies. The logic of science in **Dogmatism**, i.e., Gdeductivism, instead explains progress by appealing to an evolving set of values that depend on how properties of science's conceptualisations relate to empirical evidence but also by appealing to the distinctive metaphysics of science.

3. Theodore and Grace de Laguna's deductivism

3.1. Theodore de Laguna's later deductivism

Factors does not, because it is concerned with social evolution as such rather than logic, say much about what differentiation and integration involve when it comes to scientific judgement. In particular, it tells us little about the form of scientific inference. T. de Laguna focuses on his logic of science, i.e., on Tdeductivism, in his 1930 paper, "The Way of Opinion" (henceforth, "Opinion"). "Opinion" starts presenting Tdeductivism by telling us that

> [r]eflection takes its rise, not from axioms and sense-data, but from habitual expectations and prejudices. We have always a multitude of general beliefs in accordance with which we interpret each new matter of fact; and though any one of these beliefs may at some time be called in question, this is always on the supposition of the acceptance of a host of others. Science, accordingly, can never be a system of judgments with one-way relations of implication. Our judgments support one another. And when, as occasionally happens, they contradict one another, there is no ultimate standard of imperishable truth by which they can be tested. The only standard is the vague and shifting standard afforded by our beliefs in general. [34]

T. de Laguna here offers a view according to which scientific judgements are beliefs about the world and have no a priori justification or support. They are all empirical beliefs. Some, further, are general, that is, beliefs in general hypotheses, and some singular, that is, beliefs in particular matters of fact. The general judgements must, since there are no judgement-neutral data that determine our judgements about what we perceive, be relied on if we are to interpret new matters of fact. Moreover, the resulting singular judgements about new matters of fact do not

[34] Theodore De Laguna, "The Way of Opinion", in: George P. Adams and William P. Montague (eds.), **Contemporary American Philosophy: Personal Statements**, The MacMillan Company, New York 1930, p. 404 [401–422].

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themselves suffice to undermine any general judgements they contradict; it is only on the assumption of "a host" of general judgements that another can be undermined by singular judgements about new matters of fact. Thus, according to TDeductivism, the relations of implication between singular judgements and general ones are not one way, so that the former generally either support or undermine the latter. Rather, scientific judgements form systems of mutually supporting judgements and how to interpret a new matter of fact depends on these systems. Further, when tests lead to a conflict between general judgements and singular ones, it is only "beliefs in general" that tell us how to resolve the conflict. Beliefs in general are contrasted with standards of truth–rules of acceptance–that do not evolve over time in response to our changing system of beliefs. An example of a belief that can be used to decide how to respond to exceptions might, as we will see T. de Lagua notes, be a general hypothesis about whether an apparent kind of counterinstance to a law really is a counterinstance or is instead due to interfering factors. So, T. de Laguna's point in saying that our beliefs in general are what we appeal to in order to decide how to respond to new facts is that there is nothing that is independent of our beliefs at a particular time that allows us to decide how to respond to such facts.

T. de Laguna, however, immediately adds that "[t]here is no reasoning except deductive reasoning". [35] This, given his rejection of a priori judgements, means that he explicitly adopts deductivism. Moreover, when he talks about judgements supporting or contradicting each other, he is talking about logical relations of support and contradiction. By implication, on his view, when general knowledge develops on the basis of new singular judgements about what we observe, this is because the singular judgements conjoin with background judgements either deductively to support or deductively to undermine a general judgement. As we have seen, the basis de Laguna offers for deciding how to respond to a new observation is the system of relevant, available beliefs.

Tdeductivism has more to tell us about the case of judgement revision in response to new, challenging evidence. It is not merely a matter of evidence and argument but also a matter of social factors. As T. de Laguna puts it, when scientists come to replace an old theory with a new one, we find

[35] T. De Laguna, "The Way of Opinion...", in: Adams and Montague (eds.), **Contemporary American Philosophy...**, pp. 405.

> a series of shifts from one orthodoxy to another, marked by a conflict of parties, liberal and conservative. We have recently seen the scientific world divided in this way over the theory of relativity. The same phenomenon regularly takes place on a smaller scale whenever any less profound revision of the accepted laws of nature is attempted.
>
> It would be a mistake to suppose that in such a case the settlement is reached by rational procedures alone—unless the term "rational" is stretched to cover all processes by which we form more and more comprehensive views of things. When exceptions to a law are noted and repeatedly verified, the law is not necessarily revised, much less given up. It is always possible to refer a discrepancy between expectation and observation to the action of unknown disturbing causes. A scientific law, be it remembered, describes what happens "other things being equal," and other things may easily not be equal. So long, then, as the principle continues to do its great work of coordinating our experiences, occasional exceptions do not weaken its authority. We accept them, as the conservative man of affairs accepts the evils incidental to the operation of our political constitution and machinery of trade. But when do the exceptions become more than occasional? When do they impeach the validity of the principle and make revision imperative? How do the weaknesses of the old theory compare in seriousness with those of the new theories that are advanced? Division on these questions is largely determined by sentiment and character and personal associations. And the division is healed and a new orthodoxy reached, not when all the difficulties are logically disposed of, but when it is generally felt that the discrepancies that remain are no more than we are bound to expect. [36]

Factors answers some of T. de Laguna's own questions about how the division between liberal and conservative elements is resolved when disputes are about the "great controversies upon fundamental principles". [37] After noting that exceptions to a fundamental principle tend to be handled by denying observations or appealing to unknown interfering causes, **Factors** tells us that

> if the exceptions become frequent, and especially if they begin to exhibit a certain regularity, the whole complexion of the matter changes, for the principle itself becomes charged with the fault. It may not be at once given up—in fact, it is extremely unlikely that it should; for the extensive correlation of detail that it formerly accomplished, it still accomplishes, and there is nothing as yet to take its place. But a condition of instability is produced. Attempts are continually being made to correct the principle in question so as to accommodate the troublesome exceptions; but too often the new

[36] T. De Laguna, "The Way of Opinion…", in: Adams and Montague (eds.), **Contemporary American Philosophy…**, pp. 411–412.

[37] T. De Laguna, **The Factors of Social Evolution…**, p. 94.

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> formulae fail to cover much that was satisfactorily accounted for by the old. A division between conservative and radical parties occurs, just as in the case of a moral or political issue. And, despite all differences of detail, the final settlement is reached in fundamentally the same fashion. Comparative shortcomings must be appreciated, not counted; and the importance ascribed to each is, in the last resort, determined by tastes and prejudices. [38]

Thus, T. de Laguna distinguishes between more conservative phases in research, where acceptance of fundamental principles persists in the face of exceptions, and more radical ones, where exceptions accumulate, principles are threatened, and alternatives are explored. Replacement, however, ceases to be extremely unlikely only when sufficient challenges to the fundamental principles as well as alternatives to these principles are developed. And, in the end, "tastes and prejudices" of different parties regarding the comparative virtues of rival principles must settle the matter of which is to be set aside.

Note, however, that T. de Laguna thinks it probable that competition between conservative and non-conservative attitudes is found in all decisions about which scientific hypotheses to believe, so he does not think that differentiation between beliefs only occurs in scientific revolutions. At the same time, he does not think that belief only evolves through differentiation in response to new evidence. **Factors** and "Opinion" recognise evolution that is basically integration and thus that need not involve substantial competition between hypotheses.

Note also that while T. de Laguna acknowledges that social factors have a role in determining what scientists believe and that, as a result, scientists' belief formation is not purely rational, he also thinks that these social factors contribute to scientific objectivity. On his view, scientific judgment has a degree of objectivity and deserves, if anything does, the title "knowledge." As he puts it, if the conclusions of the sciences do not have, in general, "a high degree of probability as they stand," there is no prospect that this will change. [39] Indeed, we must assume that

> [s]cientific knowledge has the best claim to the title that any beliefs can have. It is reflective, critical, subjected to continual review and to verification and correction on

[38] T. De Laguna, **The Factors of Social Evolution...**, pp. 94–95.

[39] T. De Laguna, "The Way of Opinion...", in: Adams and Montague (eds.), **Contemporary American Philosophy...**, pp. 411.

> every hand. The personal reference remains, but it is reduced to the last degree of tenuity. [40]

Thus, while there is a personal element driving decisions about what to accept in science, this element is minimised by the processes of science. By implication, the essentially social confrontation between conservative and radical factors within scientific communities reduces the effects of individual biases. Social factors are, accordingly, supposed to contribute to making scientific beliefs objective as well as items of knowledge. In this way, scientific beliefs, objectivity, and knowledge are essentially social phenomena. Further, it seems that it is neither feasible nor desirable to aim to ensure that scientific inference is based solely on evidence, argument, and the familiar epistemic values associated with evidence and argument, values such as simplicity and strength. On Tdeductivism, the social values which are needed to maintain the balance between conservative and non-conservative attitudes to scientific principles, e.g., valuing a range of attitudes to established opinions, have a role in increasing the reliability of science.

Further light is shed on T. de Laguna's claims that scientific conclusions are objective and highly probable by another of his 1930 papers, "On Keynes' Theory of Probability". [41] Objective probabilities are, according to this paper, to be given a frequentist interpretation. To say that a proposition has a specific objective probability is to say something about the relative frequencies of the properties the proposition expresses. For example, the proposition that there is a 1/6 objective probability that (D) "This die will fall on 6" is true is to be understood (roughly) as telling us that, if the die is thrown sufficiently repeatedly, D will be true in approximately 1/6 of the total number of throws. [42] When we talk about the subjective probability of a proposition, on the other hand, we are talking about probabilities relative to those of an individuals' assumptions that serve to support the proposition. The subjective probability of a proposition concerns the frequencies of properties it expresses on the individual's relevant assumptions. The relevant assumptions include assumptions about changing and stable conditions across events in-

[40] T. De Laguna, "The Way of Opinion…", in: Adams and Montague (eds.), **Contemporary American Philosophy…**, pp. 411.

[41] See Theodore De Laguna, "On Keynes' Theory of Probability", *The Philosophical Review* 1930, Vol. 39, No. 3, pp. 227–242, https://doi.org/10.2307/2179652.

[42] T. De Laguna, "On Keynes' Theory of Probability…", p. 232.

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volving the expressed properties. In the case of (D), for example, to say that it has a 1/6 subjective probability of being true amounts roughly to saying with regard to someone that, on their assumptions about the die and the circumstances in which it is being thrown, including, e.g., that it will not become worn or otherwise biased during the throws, D will on sufficient throws be true in approximately 1/6 of the total number of throws. [43] In current terminology, subjective probabilities are a species of conditional probabilities.

Accordingly, when T. de Laguna tells us that the conclusions of science have in general a high probability, we ought to interpret his claim in frequentist terms. He is just saying that a high proportion of scientific judgements are true. As for the claim that scientific judgements become more objective as testing proceeds, it is concerned with the accumulation of evidence rather than change in beliefs. So, when our concern is with a single probabilistic hypothesis, perhaps the idea is that, as those assumptions upon which its subjective probability for someone is conditional pass more tests within the scientific community, its probability becomes more objective. The more extensively the background assumptions supporting the hypothesis have survived tests, the more the frequency they ascribe to the hypothesis' truth can be taken to be a real-world frequency. Applying the same idea to the groups of (not necessarily probabilistic) beliefs that T. de Laguna is concerned with in correlating increased testing within a group with increased objectivity suggests his idea is that the more the assumptions that support the beliefs in a group have been tested, the more objective the beliefs. It turns out that T. de Laguna thought that what makes an hypothesis increasingly more than merely the opinion of one or more scientists is the extent to which its supporting assumptions have been tested in an appropriate social context.

In summary, Tdeductivism tells us that scientists' attitudes towards their hypotheses are full beliefs. These can generally be qualified as to how objective they are, where the objectivity of a belief is a function of the extent to which its supporting beliefs have survived testing. Beliefs become increasingly objective, and thus matters of scientific knowledge rather than just personal belief, as a result of the process of scientific investigation. Increasing objectivity goes along with increasing probability, where "probability" is conceived of in frequentist terms. The scientific process, further, includes belief differentiation and integration. The

[43] See T. De Laguna, "On Keynes' Theory of Probability…", p. 233.

process does not involve synthetic a priori judgements or judgement-neutral data. Nor does it involve ampliative reasoning or strict rules about how to respond to evidence. It is determined by deductive reasoning and relevant substantive beliefs. Moreover, belief revision in response to evidence does exhibit a pattern involving conflict between conservative and radical attitudes towards beliefs, conflict that, in the last analysis, is resolved by something other than pure reasoning. Indeed, all scientific evolution exhibits a pattern of conflict between more and less radical tendencies. Finally, scientific evolution is not a cumulative process. Rather, it is a process in which error is reduced.

3.2 The de Lagunas' deductivism in 1910

In "Opinion", T. de Laguna describes himself as only slowly coming to his view of scientific judgement. Nevertheless, the views about it that he and his wife articulate in 1910 in **Dogmatism** are already naturally interpreted as a form of deductivism. To be sure, **Dogmatism** offers no explicit commitment to deductivism and appeals to induction in some of its arguments. [44] However, the book already explicitly rejects the existence of a priori knowledge and sense data, leaving our unreconstructed judgements as the starting point of inquiry. [45] Moreover, it is not explicit about whether induction is to be reconstructed as a form of ampliative reasoning rather than a deductive one while, as we will see, its discussion of scientific inference solely appeals to deductive inference in evaluating general judgements. A deductivist interpretation of Gdeductivism is thus plausible.

In some ways, Gdeductivism also resembles Tdeductivism in the overall picture it offers of how scientific judgements evolve. I have already noted that the two views share a general evolutionary perspective according to which judgement evolution in science is a non-cumulative process that includes conceptual integration and responses to exceptions to generalisations. We will now see that

[44] See e.g. T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 147.

[45] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 153–154, 159–160, and 206; Joel Katzav, "The de Lagunas' Dogmatism and Evolution, Overcoming Modern Philosophy and Making post-Quinean Analytic Philosophy", in: Eric Schliesser (ed.), **Ten Neglected Classics of Philosophy**, Vol. 2, Oxford: Oxford University Press, Oxford 2022, pp. 192–214, https://doi.org/10.1093/oso/9780190097196.003.0010.

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Gdeductivism, like Tdeductivism, provides no context-independent rule governing belief evolution, including belief revision in response to exceptions. Not unrelated, how scientists respond to exceptions is holistic and not straightforward. Yet the two positions do differ in substantive ways.

In providing details about the choice between candidate scientific laws in response to exceptions, **Dogmatism** tells us that

> [t]he validity of a universal principle is not a matter of its own individual adequacy as a description of reality; nor, again, is its validity relative to the whole existing body of human knowledge (if, indeed, we can speak of such a thing). It may correctly enough be said that the validity of such a principle depends upon its place in the developing structure of our knowledge, if we remember that this place is not definitely determined, but is exceed'ngly variable. A law is not judged as true because it marks the limit of human knowledge and because we are not able to correct any given formulation of it. Its truth is always a matter of context. It is valid if we find a certain harmony between the character and degree of its abstractness and the character and definiteness of the conclusions in view of which it is asserted. [46]

What the de Lagunas are suggesting here, in line with Tdeductivism, is that choice of scientific laws or principles, conceived of as general scientific judgements, is to some extent a matter of holistic considerations. However, the way such choices are to be made plausibly differs from the proposal found in "Opinion". In "Opinion", the choice is a matter of the beliefs with which scientists find themselves and the conflict between conservative and radical social factors. In **Dogmatism**, there is at least a principle that, although contextual in that it provides guidance which varies with the state of knowledge in a field, plays a role in regulating law choice in the face of exceptions and has more and less conservative phases of scientific development as an effect. We continue to prefer a law within a given field of research only when its abstractness meshes with the kind and level of detail of its supporting evidence and body of theory. This means that laws will be given up when the kind and level of detail they enable in characterising phenomena no longer mesh with the kind and level of detail of available evidence. Laws will also be given up when the kind and level of detail they provide no longer mesh with that of supporting theory.

When explaining their view about the choice of scientific principles, the de Lagunas ask, "[o]n what grounds, for example, do we judge the validity of the princi-

[46] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 153.

ple of classical political economy, that men seek to gratify their desires by the least exertion?". [47] They respond that

> [m]ost assuredly we should not judge it to be invalid, because as a matter of fact we find exceptions to it. That men often rush onward in their pursuit of a coveted prize without pausing to choose the shortest way, that exertion once undergone as a necessary means to some desired end may come to be desired for its own sake, are facts which may very well be regarded as negligible in this connection. [48]

Instead, the de Lagunas think,

> [t]he truth of the conception of the "economic man" is questioned today, not because of its mere abstractness, but rather because it is too rough and ready an affair for the purposes of present-day economics. A more careful study of the operations of a market, a finer analysis of the phenomena of supply and demand, a deeper insight into the nature of value, due in part to investigations in allied sciences—all these are tending so to transform our ideas of the functions performed by the "economic man," that the classical description of him is no longer appropriate. [49]

Thus, the general principle that we tend to gratify our desires by minimizing exertion was supposedly being rejected not because it had exceptions. It was, rather, partly a matter of an overall improvement in the diversity and precision of available evidence. So too, the rejection was partly the result of increased conceptual sophistication in economics and related fields.

Gdeductivism also differs from Tdeductivism in not explicitly giving social values a role in the evolution of scientific judgement. Gdeductivism is, accordingly, compatible with the view that, while such factors may well actually influence scientific inference, they should not and need not do so. The only values that Gdeductivism explicitly gives a role in fixing scientific judgements are epistemic ones, not social ones. For example, we have just seen that Gdeductivism tells us that the abstractness of general judgements must mesh with the diversity and detail of available evidence. Varieties of fit with evidence are recognisable epistemic values.

Regarding the nature of scientific judgement, recall that Tdeductivism tells us that the attitude scientists are to adopt to their preferred hypotheses is belief. Sci-

[47] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 152.

[48] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 152.

[49] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 152–153.

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entists are, accordingly, concerned with what is true without qualification. Gdeductivism, on the other hand, adopts G. de Laguna's early view [50] that scientific judgements are judgements of partial truth. In **Dogmatism**, the de Lagunas affirm "the relativity of the judgment to the particular occasion". [51] By this it is meant that, when scientists accept hypotheses, they accept only that they are true enough for certain purposes rather than that they are unqualifiedly true. [52] Thus, the de Lagunas tell us that "[w]hat determines the applicability of a concept in any particular case may vary greatly" and that what makes the difference is "the exigency of the occasion". [53] In the case of laws of nature, we have already seen that the de Lagunas thought that the acceptance of laws in science is relative to the state of investigation in a field. But their claim was also that, when laws are accepted, the acceptance is only that the laws are true enough for the purposes of the field at the time. In discussing the theory of economic man, **Dogmatism** states that "[l]aws are revised not because they are false but because they are shallow". [54]

Because Tdeductivism tells us that scientific judgements are beliefs, it seems to exclude scientists accepting more than a single perspective on each scientific phenomenon. To accept alternative perspectives would, for the Tdeductivist, be to have inconsistent beliefs. Yet accepting that scientific judgements are inevitably of partial truth appears to permit accepting alternative perspectives about the same phenomenon. Indeed, not long after **Dogmatism**, G. de Laguna argues that different special sciences and domains within special sciences do provide us with legitimate alternative perspectives on our world and the individuals in it. [55]

Gdeductivism's claim that laws are partial truths asserted for specific purposes should, nevertheless, be substantially qualified. The claim might suggest

[50] See Andrus, "Professor Bawden's Interpretation..."; G. A. De Laguna, "The Practical Character of Reality...".

[51] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 150.

[52] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 150–151.

[53] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 150.

[54] T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 152.

[55] See Grace A. De Laguna, "The Limits of the Physical", in: George H. Sabine (ed.), **Essays in Honour of James Edwin Creighton by Former Students**, The MacMillan Company, New York 1917, pp. 175–184; Joel Katzav, "Grace de Laguna's Analytic and Speculative Philosophy", *Australasian Philosophical Review* 2022, Vol. 6, No. 1, pp. 6–25, https://doi.org/10.1080/24740500.2022.2221835.

that laws are supposedly not universal at all, that is, that they merely support resolving one specific explanatory or predictive problem. **Dogmatism**, however, explicitly rejects such an extreme position. Science develops increasingly universal laws and concepts, so that it comes to include laws that are for less and less specific purposes. [56] Consider the principles of physics. **Dogmatism** notes that there was a decrease in the purpose relativity of these principles as their evaluation and the evaluation of the principles of applied geometry came to be interdependent. [57]

It is worth, before concluding the presentation of Gdeductivism, noting a potential addition G. de Laguna made to it in "Cultural Relativism and Science", [58] published three decades after **Dogmatism**. One of her claims there, namely, that ethics is a source of criticism of all of science, including its methodology, seems to be a departure from Gdeductivism. [59] However, other claims can be thought of as supplementing Gdeductivism and taking a stand on what does some of the work that Tdeductivism ascribes to the radical, social tendencies within science. According to G. de Laguna circa 1942, what makes science unique is "its capacity for progressive modification through self-criticism". [60] She is clear, however, that "it is not the discovery of fact contradicting accepted theory" that drives science's self-criticism. [61] Instead, it is the assumption that the uniformity of nature manifests a system of laws connecting all phenomena without exception. This assumption repeatedly drives scientists to recognise hitherto assumed to be irrelevant phenomena as relevant and, as a result, "creates the demand for new means of communication and forces the modification of older systems". [62]

In summary, on Gdeductivism, scientific judgements involve accepting that hypotheses are true enough for specific purposes, e.g., the purposes of a given spe-

[56] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 148–150; Katzav, "The de Lagunas' Dogmatism and Evolution...", in: Schliesser (ed.), **Ten Neglected Classics of Philosophy...**, pp. 206–207.

[57] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 159.

[58] See Grace A. De Laguna, "Cultural Relativism and Science", *The Philosophical Review* 1942, Vol. 51, No. 2, pp. 141–166, https://doi.org/10.2307/2181158.

[59] See G. A. De Laguna, "Cultural Relativism and Science...", pp. 165–166.

[60] G. A. De Laguna, "Cultural Relativism and Science...", p. 151.

[61] G. A. De Laguna, "Cultural Relativism and Science...", p. 155.

[62] G. A. De Laguna, "Cultural Relativism and Science...", p. 155.

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cial science at a time, not believing hypotheses' unqualified truth. As a result, scientific judgement can be pluralistic in that incompatible judgements can be accepted within science. At the same time, purpose relativity in science does decrease over time. Judgement revision can be part of the process of conceptual integration within science or part of responses to exceptions. Moreover, judgement evaluation is (plausibly) deductive and, since it takes into account the state of knowledge in a field, holistic. Judgement revision is also not subject to context-independent rules but, when it comes to responses to exceptions, follows a rule that depends on an appropriate fit between the abstractness of the hypotheses to which there are exceptions and the kind and level detail of the evidence and theory relevant to the hypotheses' evaluation. To some extent, such revision depends also on developments in allied fields. What ultimately drives the development of concepts within science are not exceptions but science's own distinctive metaphysical commitment to explaining all phenomena in terms of a system of laws.

4. The de Lagunas and analytic philosophy of science

4.1 The nature of scientific judgement

Let me turn to comparing Tdeductivism and Gdeductivism with the philosophies of science of Popper and Kuhn. My comparison will proceed along three dimensions. In this subsection, I will consider the attitudes involved in scientific judgement. In the next subsection, I will consider scientific inference and dynamics.

Popper's falsificationist view of the appropriate attitude to scientific hypotheses is first developed in his 1935 book **Logic der Forschung** [63] and found in his augmented, 1959 translation of this book, **The Logic of Scientific Discovery.** [64] His position in 1935 and 1959 was that acceptance of a scientific hypothesis is not a matter of belief but only of taking it to be worthy of further empirical investiga-

[63] See Karl R. Popper, **Logik der forschung; zur erkenntnistheorie der modernen naturwissenschaft**, J. Springer, Wien 1935.

[64] See Karl R. Popper, **The Logic of Scientific Discovery**, Routledge, London — New York 2005.

tion.[65] This distinguishes Popper's view from Tdeductivism, which tells us that evaluating scientific hypotheses is a matter of deciding whether they are to be believed. Popper's falsificationism is further differentiated from Tdeductivism by how scientific judgements are to be ranked. Popper ranks hypotheses only according to the extent to which they have been corroborated. An hypothesis' degree of corroboration is supposed to increase with the severity of the empirical tests it has passed; the greater the risk of falsifying an hypothesis as a result of a test, the greater the severity of the test.[66] Tdeductivism, by contrast, ranks beliefs according to objectivity, the extent to which their supporting assumptions have passed tests in an appropriate social context, and correlates increasing objectivity with increasing probability.

Kuhn's view of scientific judgements in his 1962 book **The Structure of Scientific Revolutions**[67] (henceforth, **Structure**) has some similarity to that of Tdeductivism. Both are about scientific judgements as they actually are. Further, according to Kuhn, scientists are, and should be, mostly engaged in normal science. In normal science, scientists accept a paradigm, roughly, a solution of a specific scientific problem that serves within a scientific community as an exemplar for specifying and addressing other problems.[68] In this context, acceptance does and should amount to dogmatic belief. It is only during scientific revolutions that there emerge rival, more and less conservative attitudes to the dominant paradigm.[69] Kuhn, accordingly, closely recapitulates Tdeductivism's distinction between a conservative phase of research, in which acceptance of fundamental principles persists despite exceptions, and a phase in which such acceptance is threatened. There are, however, real differences between Tdeductivism and Kuhn's position here. Tdeductivism is not committed to a dogmatic form of belief within science.

[65] See Popper, **The Logic of Scientific Discovery...**, p. 438.

[66] See Popper, **The Logic of Scientific Discovery...**, p. 266.

[67] See Thomas S. Kuhn, **The Structure of Scientific Revolutions**, The University of Chicago Press, Chicago 1962.

[68] See Thomas S. Kuhn, **The Structure of Scientific Revolutions**, 3rd ed., University of Chicago Press, Chicago 1996, p. 10.

[69] See Thomas S. Kuhn, "The Function of Dogma in Scientific Research", in: Alistair C. Crombie (ed.), **Scientific Change: Historical Studies in the Intellectual, Social, and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present**, Basic Books & Heinemann, New York 1963, pp. 347–369; Kuhn, **The Structure of Scientific Revolutions...**, 3rd ed., pp. 24–25, 80, 144–145.

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T. de Laguna, recall, took belief in laws to be like the conservative's belief in "the political constitution and machinery of trade," in that they come with realism about the evils they involve. They thus presumably leave room for a degree of scepticism. Further, contra Kuhn, Tdeductivism proposes a role for conflict between more conservative and radical attitudes throughout science. This means that, according to Tdeductivism, we can expect to find a range of attitudes towards dominant concepts and methods in all stages of scientific development. Another difference is that Tdeductivism does not explicitly take a stand on the extent to which scientists' endorsement of principles is more fundamentally of specific applications, that is, of something like Kuhn's exemplars, rather than of more universal formulations. For Kuhn, it is endorsement of a paradigm that is fundamental.

Gdeductivism, like Tdeductivism and Kuhn's position, takes scientific inquiry to proceed from the actual judgements of scientists. Gdeductivism also recognises more conservative and more radical attitudes in science, albeit only indirectly as effects of its contextual principle for responding to exceptions. Gdeductivism, however, contrasts with Popper and Kuhn on the nature of scientists' attitudes to hypotheses. According to Gdeductivism, scientists are concerned with partial truth while all the other views considered here suppose that scientists are concerned with unqualified truth. Moreover, insofar as Gdeductivism discusses the gradation of scientific judgements, it is in terms of degrees of truth. It does not grade them in terms of objectivity. One point where Gdeductivism may be in between Kuhn's position and Tdeductivism concerns the extent to which attitudes to principles are to their applications rather than to more universal formulations. While Tdeductivism is not explicit on this matter, Gdeductivism tells us that, to some extent, what scientists endorse are specific solutions to specific scientific problems. Such a position has an affinity with Kuhn's view that acceptance of exemplars is what is primary in scientific practice, though Gdeductivism insists that Kuhn's claim be substantially qualified by the recognition that acceptance in science is increasingly universal.

4.2 Inference and the dynamics of judgement

According to Popper, general hypotheses are to be tested solely by examining

whether some of their predictions are contradicted by accepted results of experiments or practical applications. Moreover, the auxiliary assumptions needed to deduce predictions are not to include synthetic a priori assumptions. [70] As we have seen, he thinks that when the predictions of an hypothesis are fulfilled, the hypothesis is corroborated. He does not think that hypotheses are thereby ampliatively justified. Popper, like the de Lagunas before him, is committed to deductivism.

Popper adds that conventions, in the form of rules, govern acceptance in science. These are selected on the grounds that they promote learning from empirical criticism. [71] More specifically, the conventions must be such that they ensure that scientific hypotheses are exposed to criticism in every possible way rather than are protected from criticism. [72] Popper takes this to imply that scientists should use, among other rules, the following one: only explain away an exception by appealing to hypotheses that increase the degree of falsifiability of one's system of hypotheses. [73] Thus, Popper held that the conventions that govern the dynamics of acceptance in science, including the acceptance of exceptions, are conventions based on a priori considerations about how to facilitate learning from empirical evidence. In addition, at least some rules that govern this dynamics are context independent in that what they recommend does not depend on the overall state of knowledge in the field but only on the degree of falsifiability of new hypotheses. By contrast, Tdeductivism and Gdeductivism give a central place to scientific development that is not a response to exceptions. Both, recall, recognise that scientific evolution includes phases of integration of the elements of scientific systems that are not led by empirical evidence. Moreover, they view the response of science to evidence to be learnt through the long process of social evolution rather than to be based on conventions. Finally, they reject the existence of context-independent rules that determine how to respond to exceptions. Gdeductivism recommends different responses to exceptions depending on how hypotheses mesh with supporting evidence and theory, while Tdeductivism claims that it is just belief within a community of scientists at a time that guides responses. Tde-

[70] See Popper, **The Logic of Scientific Discovery…**, pp. 5–6 and 9–10.

[71] See Popper, **The Logic of Scientific Discovery…**, pp. 15 and 27–28.

[72] See Popper, **The Logic of Scientific Discovery…**, p. 20.

[73] See Popper, **The Logic of Scientific Discovery…**, pp. 61–63.

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ductivism even gives social, contextual factors a role in such responses.

Kuhn is not fully explicit about how his views relate to deductivism, but he compares his position to that of Popper and, in doing so, states that he shares Popper's anti-inductivism. [74] Kuhn does not, in the same comparison, identify a belief in ampliative reasoning as a point on which he differs with Popper. In any case, nothing he tells us about the dynamics of science rests on giving induction a role in fixing scientific beliefs, and sometimes his claims clearly exclude doing so. For example, the dogmatic attitude to paradigms during normal science is held irrespective of the specific details of the development of the paradigm and so independently of any ampliative support for it. Overall, then, Kuhn too seems to come close to adopting the de Lagunas' deductivism.

Regarding Kuhn's view of the dynamics of science in **Structure**, normal science supposedly leads to the discovery of new phenomena that resist accommodation within the accepted paradigm. [75] Mere resistance to accommodation does not itself lead to reconsidering the paradigm, however. As Kuhn summarises it, such reconsideration occurs "only after persistent failure to solve a noteworthy puzzle has given rise to crisis. And even then it occurs only after the sense of crisis has evoked an alternate candidate for paradigm". [76] Eventually, the new paradigm is adopted, and a new period of normal science ensues. There are, however, no strict rules that govern the transition from the preference of one paradigm to that of another. Instead, the transition depends on standards that are largely internal to paradigms. Moreover, the choice between paradigms is influenced by competing, conservative and revolutionary preferences. [77] Finally, because the replacement of one paradigm with another includes systemic conceptual change, the progress of science through its development is not cumulative. [78] Kuhn, accordingly, adopts a view of the dynamics of science that is, in key ways, close to that of the de Lagunas. Kuhn too takes hypothesis evaluation to be contextual, because he

[74] See Thomas S. Kuhn, "The Logic of Discovery or the Psychology of Research?", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Cambridge University Press, Cambridge 1970, p. 12 [1–24], https://doi.org/10.1017/CBO9781139171434.003.

[75] See Kuhn, **The Structure of Scientific Revolutions...**, 3rd ed., Chapter VI.

[76] Kuhn, **The Structure of Scientific Revolutions...**, 3rd ed., p. 145.

[77] See Kuhn, **The Structure of Scientific Revolutions...**, 3rd ed., Chapters VII–IX.

[78] See Kuhn, **The Structure of Scientific Revolutions...**, 3rd ed., pp. 84–85.

thinks it depends on the dominant paradigm and social factors. Further, Kuhn agrees with both the de Lagunas' positions that scientific revolutions involve the reconceptualization of phenomena and are, as a result, not cumulative. The actual process of revolutionary change described by Kuhn bears similarity to the processes described by Gdeductivism and Tdeductivism; all three descriptions incorporate the idea that substantial scientific change has a conservative and a revolutionary side. However, Kuhn's description is closer to the one found in Tdeductivism because Kuhn agrees with its detailed story about the need for an accumulation of exceptions and the emergence of rival theses before a thesis is problematized as well as that a conflict between conservatives and radicals that is resolved in a not purely rational way is ultimately what decides the outcome of a revolution.

Tdeductivism and Gdeductivism, however, differ from Kuhn's position in **Structure** in being developed within the framework of an evolutionary view of science while Kuhn's view is not presented as part of a more general framework; he merely suggests that a broader evolutionary framework is possible. [79] Further, despite claiming that there are aspects of science that are not rule governed, **Structure** claims that science does and should follow the normal science–revolution–normal science pattern. More specifically, **Structure** tells us that this pattern is without exception when it comes to developing new theories and virtually without exception when it comes to addressing exceptions. As Kuhn puts it, "the assimilation of all new theories and of almost all new sorts of phenomena has in fact demanded the destruction of a prior paradigm and a consequent conflict between competing schools of scientific thought". [80] Moreover, while science could in principle have developed in other ways, the only effective way of discovering new phenomena is via dogmatic adherence to a paradigm. [81] By contrast, the de Lagunas are more relaxed about the kinds of change found in science and, in particular, do not claim that the normal science-revolutionary science pattern is the dominant mode of development of science. They suppose that substantial development regularly occurs without revolutions. So too, they suppose that the process of evolutionary integration is key to scientific development and not just the process of

[79] See Kuhn, **The Structure of Scientific Revolutions…**, 3rd ed., p. 172.

[80] Kuhn, **The Structure of Scientific Revolutions…**, 3rd ed., p. 96.

[81] See Kuhn, **The Structure of Scientific Revolutions…**, 3rd ed., p. 96.

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differentiation between types of knowledge. For them, the introduction of a new hypothesis that transforms a science need not follow the normal science–revolution–normal science pattern.

Tdeductivism's supposition that there are more and less conservative attitudes towards dominant hypotheses throughout science leads to a further, related difference from Kuhn's position. This supposition implies that scientific development is not generally marked by two more or less rigidly separated responses to exceptions, the normal and the revolutionary. Rather, we should expect to find more of a continuum of kinds of change in scientific hypotheses and methods. Similarly, Gdeductivism suggests a less conservative view of scientific change than does Kuhn and does so by supposing that science's metaphysics drives the revision of available concepts throughout the development of science and by permitting, given the thesis that scientific judgements are of partial truth, incompatible perspectives on phenomena. Positions akin to Gdeductivism in this regard only start to emerge within Analytic philosophy with Feyerabend's work from the late 1960s. [82]

4.3. Potential challenges to the de Lagunas' philosophies of science

The first of the challenges to which the de Lagunas' positions provide ready responses is to Popper's falsificationism and concerns whether it offers an adequate account of the rational application of scientific knowledge in practical mat-

[82] See Franklin Jacoby, "Perspectivism in Science", *Internet Encyclopedia of Philosophy*, https://tiny.pl/r3wn471q [15.10.2025].

Kuhn does start to develop a more explicit evolutionary account of scientific development in the decades after **Structure** and in doing so comes to recognise further kinds of scientific development. For example, like the de Lagunas, he proposes that development in science occurs through continuous processes of judgement differentiation and not just during revolutionary differentiation (see K.B. Wray, **Kuhn's Evolutionary Social Epistemology**, Cambridge University Press, Cambridge 2011).

This section has focused on differences and similarities relating to judgement dynamics. I do not elaborate on important differences, especially between the de Lagunas and Kuhn, about whether science makes progress towards the truth.

ters, e.g., in setting policy or developing technology. [83] I will break down this problem into two parts. The first concerns whether Popper's position allows for an appropriate variety of attitudes towards hypotheses. Not all hypotheses that are worthy of further testing are worthy of being relied on in practice; our best hypothesis about the safety of a drug, for example, might still be in the preliminary stages of testing. So, it seems we need to distinguish between attitudes generally held towards hypotheses that are worthy of further examination and attitudes towards those that can be accepted for practical purposes. In the work on which we have been focusing, Popper merely tells us about accepting that hypotheses are worthy of continued testing. [84]

Tdeductivism, however, allows us to distinguish between the attitudes involved in subjective and objective beliefs. Both types of belief can be worthy of further investigation but the more objective a belief, recall, the more likely it is to be true. This provides us with a starting point for grading hypotheses as to their fitness for being applied in practical contexts. Beliefs with appropriately high objective probabilities can be those we have grounds to prefer in such contexts.

Gdeductivism permits a similar differentiation between attitudes to hypotheses. According to Gdeductivism, scientists' acceptance of hypotheses is acceptance that they are true enough for some purposes but not others. This allows distinguishing, as Gdeductivism indeed does, between hypotheses that are true enough for some practical purposes but not others. [85] Gdeductivism does not even exclude accepting that an hypothesis is not true enough for any practical purposes. So, Gdeductivism allows recognising a range of attitudes to hypotheses, spanning acceptance for no practical purposes to acceptance for some such purposes.

The second part of the problem of the rational application of hypotheses in practice concerns the justification of the attitude required for such application. If, as assumed by Tdeductivism, our use of general hypotheses in practical matters is predicated on believing that they are true, we need to address the further prob-

[83] See Geoffrey Stokes and Jeremy Shearmur, "Popper and His Philosophy: An Overview", in: Jeremy Shearmur and Geoffrey Stokes (eds.), **The Cambridge Companion to Popper**, Cambridge University Press, Cambridge 2016, pp. 9–10 [1–29].

[84] I take no stand on whether Popper or Kuhn offer adequate responses to the challenges they face. I am solely concerned with whether the de Lagunas have at least initial plausible responses to the challenges.

[85] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 149–150.

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lem of how available empirical evidence justifies our beliefs. Popper's eventual appeal to corroboration [86] does not help because corroboration is simply a report on the severity of past tests passed by the hypothesis and so, by itself, implies nothing about the future success of the hypothesis. Indeed, if we are to believe the truth of a general hypothesis on the basis of its past empirical successes, we must be in a position empirically to eliminate all the competing, gerrymandered or counter-inductive generalizations that are compatible with the successes. Yet it seems that an ampliative rule is needed to exclude such generalizations. [87]

This worry, however, begs the question against positions such as Tdeductivism. According to Tdeductivism, beliefs need not be justified by their success. Instead, the basis for believing a new hypothesis includes whatever background knowledge we have, whether general or not. So, we can legitimately deduce our new hypothesis from observations and background assumptions. Until some reason is offered for denying our background beliefs this role in deciding how beliefs should evolve, no challenge has been offered to Tdeductivism.

Although Gdeductivism is about the acceptance of partial truths and thus does not face the challenge of justifying accepting the truth of general hypotheses, we can reformulate this challenge so that it is about partial truths. Reformulated, the problem is what justifies accepting that those hypotheses we rely on for various practical purposes are true enough for the purposes to which they are put. But since the Gdeductivist supposes that our current systems of judgements are to be appealed to in deciding what to accept, Gdeductivists can adopt the Tdeductivist response to this problem.

Tdeductivism and Gdeductivism similarly lead to their own responses to challenges usually levelled against Kuhn's account of scientists' attitudes. One key worry is that **Structure** was mistaken in supposing that dogmatic adherence to a paradigm is necessary to ensure, and is in fact generally found in, normal science and its successful puzzle solving work. [88] We have, in effect, already seen that Tdeductivism mitigates this worry because of its rejection of dogmatism, and its sup-

[86] See Popper, **The Logic of Scientific Discovery...**, p. 182.

[87] See Wesley C. Salmon, "Rational Prediction", *The British Journal for the Philosophy of Science* 1981, Vol. 32, No. 2, pp. 115–125, https://doi.org/10.1093/bjps/32.2.115; Stokes and Shearmur, "Popper and His Philosophy...", in: Shearmur and Stokes (eds.), **The Cambridge Companion to Popper...**, pp. 1–29.

position of an interplay between more and less conservative attitudes throughout science. Similarly, we have seen that Gdeductivism takes metaphysics to drive relatively revolutionary attitudes in science and permits a pluralistic view about scientific perspectives.

Let us turn to considering responses to the challenges to Popper and Kuhn's views about judgement dynamics. Popper's falsificationism faces the worry that, in practice, scientists do not generally aim to test their hypotheses in every possible way and that this less than fully radical component of judgement revision has been useful in scientific practice. [89] The view in **Structure** faces the worry that, because of its insistence that there be periods of normal science characterised by dogmatism, it fails to capture the dynamic nature of science during much of its career, including to recognise the substantial non-revolutionary development of science as well as revolutionary changes when these are not in response to empirical challenges. [90]

Tdeductivism and Gdeductivism offer ready responses to the need for a degree of conservatism in judgement revision. Tdeductivism is explicit that science must include conservative tendencies, while Gdeductivism includes such tendencies in proposing that the partial truth of scientific judgements goes along with the willingness to live with falsehood and the associated focus on assessing hypothesis adequacy for purpose. So too, since both views use the same evolutionary framework, they permit a less constrained view of the evolution of scientific knowledge than Kuhn's. As we have seen, the framework leads us to expect a continuum of degrees of scientific change and substantial scientific development through the process of integrating existing scientific knowledge.

My final example of how the de Lagunas might respond to challenges with which Popper and Kuhn are thought to have struggled illustrates how the de Lagunas' broader philosophies supplement their deductivisms. **Structure** faced the

[88] See Darrell P. Rowbottom, "Popper on Criticism and Dogmatism in Science: A Resolution at the Group Level", *Studies in History and Philosophy of Science A* 2011, Vol. 42, No. 1, pp. 117–124, https://doi.org/10.1016/j.shpsa.2010.11.031.

[89] See Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Cambridge University Press, Cambridge 1970, pp. 91–196, http://dx.doi.org/10.1017/CBO9781139171434.009.

[90] See Thomas Nickles, "Scientific Revolutions", in: Edward N. Zalta and Uri Nodelman (eds.), **The Stanford Encyclopedia of Philosophy**, Spring 2024 Edition, https://tiny.pl/ph5bjcsp [15.10.2025].

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problem of explaining how science can be progressive given that a scientific revolution brings with it systemic conceptual change, and thus systemic loss of content, within domains of research. [91] This problem can perhaps be addressed by the theories of meaning developed by the de Lagunas. They held that logical relations within systems of concepts determine their contents and thus that revolutions bring with them systemic change in concepts' contents, but they also recognise ways in which contents can remain constant across revolutions and thus in which content can be retained. T. de Laguna argued that the content of a concept is not just a matter of its logical relations to other concepts but also a matter of its causal relations to external factors in the environment. [92] He accordingly allowed stability of content across revolutions where these left such causal relations intact. **Dogmatism**'s theory of meaning tells us that the logical relations between concepts differ in how much they contribute to concepts' contents, [93] thus allowing us to talk about substantial sameness of content across systems of concepts even if these only resemble each other in some of their logical interrelationships.

5. Context for the work of the de Lagunas, Popper, and Kuhn

5.1 The de Lagunas and Popper in context

I will start situating the philosophies of science discussed above by considering the work of the de Lagunas and of Popper. Here, we find the de Lagunas responding to a problem situation that largely overlaps with the one to which Popper is responding. So too, the Lagunas' deductivist responses are not alone in resembling Popper's response.

The main impetus the de Lagunas offer for developing their evolutionary philosophy of science is presented early on in **Dogmatism**. The de Lagunas are moti-

[91] See Nickles, "Scientific Revolutions...", in: Zalta and Nodelman (eds.), **The Stanford Encyclopedia of Philosophy...**

[92] See Theodore De Laguna, "The Postulates of Deductive Logic", *The Journal of Philosophy, Psychology and Scientific Methods* 1915, Vol. 12, No. 9, p. 225 [225–236], https://doi.org/10.2307/2013520.

[93] See Katzav, "The de Lagunas' Dogmatism and Evolution...", in: Schliesser (ed.), **Ten Neglected Classics of Philosophy...**, p. 198.

vated by their view that the epistemologies of modern philosophy, from that of Francis Bacon down through to that of Kant, had thoroughly failed. This failure includes the failure of the thesis that knowledge is ultimately justified by individual acts of infallible intuition, whether these be sense perceptions or a priori intuitions. Justifying knowledge by appealing to sense perception failed because, on the one hand, it is not possible to analyse general judgments into singular judgements about sense perception and, on the other hand, general judgements cannot be justified, as Kant hoped, by inferences from perception on the basis of synthetic a priori judgements. [94] "Opinion"'s later critique of induction fits well into this motivation, since induction is the form of inference that is supposed to justify general judgements on the basis of singular ones about what is perceived. The problem is, according to "Opinion", that "there is no axiom of induction; and if there were it would not be of the slightest service to us". [95] An additional motivation for the de Lagunas' project is their view that there is a need, made acute by Charles Darwin's theory of evolution, to develop a theory of knowledge that recognises a variety of types of human judgement and explains how these evolve until the ones characteristic of science emerge. [96]

The de Lagunas, further, see themselves as responding to the failures of modern philosophy after Hegelians, pragmatists, and others try, but at least partly fail to, adequately do so. [97] Among the European responses the de Lagunas argue against is conventionalism about scientific laws, a view they associate with Henri Poincaré. This view, according to the de Lagunas, tells us that the problem of providing incontrovertible justification for the laws of physics is resolved by recognising that they are selected by scientists' decisions rather than the natures of things and are, accordingly, merely conventions rather than descriptions of real-

[94] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 3–66; Katzav, "The de Lagunas' Dogmatism and Evolution...", in: Schliesser (ed.), **Ten Neglected Classics of Philosophy...**, pp. 201–202.

[95] T. De Laguna, "The Way of Opinion...", in: Adams and Montague (eds.), **Contemporary American Philosophy...**, p. 408.

[96] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, pp. 119–124; Katzav, "The de Lagunas' Dogmatism and Evolution...", in: Schliesser (ed.), **Ten Neglected Classics of Philosophy...**, pp. 201–202.

[97] See Katzav, "The de Lagunas' Dogmatism and Evolution...", in: Schliesser (ed.), **Ten Neglected Classics of Philosophy...**.

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ity. [98]

That the de Lagunas' views about the problem situation in epistemology at the turn of the twentieth century were not idiosyncratic is suggested by looking, for example, at the work of their teacher, James Edwin Creighton and of Singer Jr. These figures motivate their own philosophies of science by rejecting both synthetic a priori justifications of general scientific judgements and conventionalist views of laws. [99] So too, the positions explicitly advocated by American philosophers other than the de Lagunas sometimes are, and sometimes at least come close, to being versions of deductivism. For example, Cohen was not a critic of induction but did think that induction was to be understood to be a form of deductive inference. [100] W. V. Quine, who was a PhD student in America during the 1930s, adopted a holistic epistemology of science that is in many ways akin to that of Creighton and the de Lagunas. [101] Moreover, Quine acknowledges, relatively late in his career, that he had always agreed with Popper that observation "serves only to refute theory and not to support it". [102]

In his 1935 work, Popper is less systematic in his examination of prior epistemologies than the de Lagunas. For example, he does not examine Hegelian or pragmatist positions. But nuances of formulation aside, Popper too is motivated by what he takes to be the failure of the view that scientific knowledge is ultimately justified by judgements about what is perceived, a view he identifies with the view that induction justifies general judgements. [103] Moreover, he too agrees that this view fails in part because general scientific judgements cannot be reduced to singular ones and in part because general scientific judgements cannot be justified by synthetic a priori ones. [104] Conventionalist positions, such as that of Poincaré, are prominent among the responses to this problem situation that Pop-

[98] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**, p. 157.

[99] See J.E. Creighton, "Methodology and Truth", *The Philosophical Review* 1901¸ Vol. 10, No. 1, pp. 45–56, https://doi.org/10.2307/2176539; Edgar A. Singer, Jr., "Choice and Nature", *Mind* 1902, Vol. 11, No. 41, pp. 72–91, https://doi.org/10.1093/mind/XI.1.72.

[100] See Cohen, **Reason and Nature...**, p. 117.

[101] See Katzav, "The de Lagunas' Dogmatism and Evolution...", in: Schliesser (ed.), **Ten Neglected Classics of Philosophy...**.

[102] W.V. Quine, **The Pursuit of Truth**, Harvard University Press, Cambridge (Mass.) — London 1990, p. 12.

[103] See Popper, **The Logic of Scientific Discovery...**, pp. 4–7.

per critiques. [105] Further, the underlying problem that epistemologies of science should address, according to Popper, is the problem of demarcation, that is, that of identifying the distinctiveness of scientific systems in contrast with others such as those of logic and metaphysics. [106] Popper thus shares the de Lagunas' concern with the differences between non-scientific and scientific knowledge. Overall, his problem situation turns out substantially to overlap with the one to which American philosophers, including the de Lagunas, are responding. The fact that American philosophers and Popper are responding to a shared problem situation means that their positions are justly compared as to the extent to which they successfully respond to this situation.

To be sure, Popper departs from the de Lagunas in rejecting the idea that empirical investigation will be what reveals the nature of science and thus does not aim to develop an empirical, evolutionary epistemology. On his view, what the philosopher of science ought to do is to offer proposals for scientific inquiry, proposals that govern the acceptance of scientific claims and that differentiate scientific from other types of claim. [107] Popper here follows the conventionalist in assuming that scientists' decisions have a role in determining which laws are accepted but aims to specify decision rules that make laws informative about how the world is rather than purely conventional. But the de Lagunas' discussion of conventionalist views shows that they were well aware of such decision-based responses to their problem situation. So, the difference of their position from that of Popper is merely about how to respond to a shared problem situation rather than a disagreement about the problem situation as such.

5.2 The de Lagunas and Kuhn in context

Wray has already made a start at situating Kuhn in the context of American philosophy of science. Wray argues that Kuhn adopts many of his characteristic theses about science from his Harvard mentor, James B. Conant. These theses

[104] See Popper, **The Logic of Scientific Discovery…**, p. 11.

[105] See Popper, **The Logic of Scientific Discovery…**, pp. 57–58.

[106] See Popper, **The Logic of Scientific Discovery…**, pp. 10–16.

[107] See Popper, **The Logic of Scientific Discovery…**, pp. 14–15.

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comprise the following eight claims: the history of science can teach us about the nature of science; the development of science is to be understood as the development of conceptual schemes, that is, of closely interconnected systems of concepts that allow scientists to interpret and predict experience; the evaluation of scientific hypotheses is holistic to some degree or another; the replacement of one scientific theory by another during revolutions is a source of scientific progress; it is not plausible that the different sciences will come to conceptualise their domains using the same sets of concepts; it is a virtue of a conceptual scheme that it provides new predictions and not merely accommodates existing observations; there is no single method guiding all scientists in a semi-mechanical way; and the existence of exceptions to an established conceptual scheme is not enough to overthrow it. [108] Kuhn himself acknowledges that he came to adopt the view the philosophy of science should be informed by the history of science under the influence of Conant. [109]

But the first of the theses on Wray's list–the one about history-informed philosophy of science–was a distinguishing feature of American speculative philosophy of science. And work adopting this thesis and the rest of the views listed by Wray was ubiquitous in American philosophy of the 1930s and 1940s. [110] As a result, Conant and Kuhn would inevitably have been familiar with exemplars of it. This is supported by the observation that Kuhn had an avid interest in the philosophy of science in the 1940s [111] and that Conant's interest in, and familiarity with, the local speculative scene is clear in the bibliography of his **Science and Common Sense**, [112] which, despite being very short, includes Alfred North Whitehead's **Science and the Modern World** [113] and Stephen C. Pepper's **World Hy-**

[108] See K.B. Wray, "The Influence of James B. Conant on Kuhn's Structure of Scientific Revolutions", *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 2016, Vol. 6, No. 1, pp. 9–15 [1–23], https://doi.org/10.1086/685542.

[109] See Kuhn, **The Structure of Scientific Revolutions...**, 3rd ed., p. xiii.

[110] For further support for my claim that the philosophers of science discussed in this section are speculative philosophers see Katzav and Vaesen's (Katzav and Vaesen, "The Rise of Logical Empiricist Philosophy of Science...", pp. 327–358) overview of the community of American speculative philosophers of science.

[111] See Kuhn, **The Structure of Scientific Revolutions...**, 3rd ed., p. vii.

[112] See James B. Conant, **Science and Common Sense**, Yale University Press, New Haven 1951.

potheses: A Study in Evidence. [114]

Whitehead and Pepper's books amply illustrate the views Wray claims come from Conant, as do books by, among others, Charles West Churchman, [115] Filmer Stuart Cuckow Northrop, [116] and William Henry Werkmeister. [117] Everyone, of course, knew Whitehead. But Pepper was also well known in the 1940s. The book by Pepper that Conant cites was, for example, subject to extensive discussion in the two key philosophy journals in America, "The Philosophical Review" [118] and "The Journal of Philosophy". [119] Moreover, Kuhn moved to the University of California, Berkeley in 1956, [120] thereby becoming Pepper's colleague. [121] Churchman became the editor of the journal "Philosophy of Science" in 1947. Northrop and Werkmeister's views were covered extensively, and juxtaposed with those of the logical positivists, in a survey of American philosophy of science. [122] Conant and Kuhn could, accordingly, hardly avoid being familiar with historically informed philosophy of science that presents the theses Wray claims Kuhn took from Conant. It is thus not so important whether Kuhn adopted many of the views of Co-

[113] See Alfred N. Whitehead, **Science and the Modern World**, Cambridge University Press, Cambridge 1925.

[114] See Stephen C. Pepper, **World Hypotheses: A Study in Evidence**, University of California Press, Berkeley 1942; Conant, **Science and Common Sense...**, p. 537.

[115] See Charles W. Churchman, **Elements of Logic and Formal Science**, J.B. Lippincott Company, Chicago 1940.

[116] See Filmer Stuart Cuckow Northrop, **The Logic of the Sciences and the Humanities**, Meridian Books, New York 1947.

[117] See William H. Werkmeister, **A Philosophy of Science**, Harper and Brothers Publishers, New York — London 1940; William H. Werkmeister, **The Basis and Structure of Knowledge**, Harper and Brothers Publishers, New York — London 1948.

[118] See "The Philosophical Review" 1943, Vol. 52, No. 6.

[119] See "The Journal of Philosophy" 1942, Vol. 39, No. 19; "The Journal of Philosophy" 1945, Vol. 42, No. 4.

[120] See Alexander Bird, "Thomas Kuhn", in: Edward N. Zalta (ed.), **The Stanford Encyclopedia of Philosophy**, Spring 2022 Edition, https://tiny.pl/41rpqxb9 [16.10.2025].

[121] See Stephen C. Pepper, **Art and Philosophy at the University of California, 1919 to 1962**, An interview by Suzanne B. Riess, "Oral History Center", The Bancroft Library, University of California, Berkeley 1963, https://tiny.pl/cbw05mxv [16.10.2025].

[122] See James Collins, "A Quarter Century of American Philosophy", *Schoolman* 1950, Vol. 25, No. 1, pp. 46–80, https://doi.org/10.5840/newscholas19512513.

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nant as that both these thinkers adopted much of what were widely known and discussed, speculative views about science. In light of the fact that such views were not being put forward by the logical positivists in America, much of what Conant and Kuhn's proposed about science is likely to derive from American speculative philosophy of science at the time.

To illustrate the claim that Conant and Kuhn's shared hypotheses were "off the shelf", let me consider Pepper's work in more detail. Pepper's approach to developing an understanding of science is history-based and leads to the following theses: evidence is always interpreted and revisable. [123] Moreover, the assessment of evidence and hypotheses is holistic; our assessments are of systems of evidence and hypotheses. [124] The standards we apply in such assessment, further, are set by world hypotheses, that is, hypotheses that describe the structure of our world and are unrestricted in scope. Unfortunately, there is no single world hypothesis that is overall the best. Instead, multiple competing world hypotheses, including, among others, the mechanistic one and one that conceives of the world as an organism, are viable. [125] Pepper thus rejects the idea that science can be unified conceptually by appealing to any single set of concepts. He also, because competing world hypotheses come with competing standards for hypothesis acceptance, rejects the idea of a simple mechanistic account of hypothesis selection. Nevertheless, among the available standards for such selection, there is a recognition of the importance of predicting, rather than merely accommodating, evidence. So too, there is a recognition that interpretive schemes, relatively restricted hypotheses developed on the basis of world hypotheses, can survive the accumulation of exceptions. [126] Overall, the picture of scientific development that Pepper offers is one in which interpretive schemes that illustrate one or another world hypothesis are developed, elaborated on in dealing with growing bodies of evidence, and eventually replaced. [127] Although Pepper does not describe a general pattern for such replacement, he recognizes that important developments occur through scientific

[123] See Pepper, **World Hypotheses...**, pp. 31–32.

[124] See Pepper, **World Hypotheses...**, pp. 73–78.

[125] See Pepper, **World Hypotheses...**, pp. 326–330.

[126] See Pepper, **World Hypotheses...**, pp. 284–297.

[127] See Pepper, **World Hypotheses...**, pp. 186–187 and 299–303.

revolutions in which one scheme is replaced by another that presupposes the same world hypothesis as the replaced one while specifying it in a new way. [128]

This description of Pepper's position shows him subscribing to all that Wray finds is shared by Conant and Kuhn but also emphasizing the claim that world hypotheses are specified by scientific schemes and that this specification plays a role in driving the evolution of world hypotheses. Kuhn proposes a similar relationship between world views and their development in science. [129] The proposal is, indeed, a further example of an off-the-shelf hypothesis in Kuhn's work; it was also found in Whitehead [130] and Cohen, [131] among others.

Thus far we have seen what makes a comparison of **Structure** and American speculative philosophy of science meaningful but not why a comparison with the de Lagunas' work is particularly appropriate. In order to see this, we need more detail about what Kuhn's original contributions to philosophy of science might be supposed to be. Wray insists that Kuhn goes beyond Conant in a number of ways. First, Kuhn introduced the distinction between normal science and revolutionary science. [132] Second, he introduced the notion of an exemplar as the primary object of scientific acceptance. Third, he brought out the epistemic challenges associated with scientific revolutions. [133] He supposedly did so by appealing to the already noted theses that science is not cumulative, that competing hypotheses before and after a revolution conceptualise the same phenomena differently, and that revolutions bring with them different standards for evaluating hypotheses. Finally, Wray claims that Kuhn was novel in arguing that the internally driven social development of science plays a proper role in determining what scientists come to believe. It is this, Wray states, that makes **Structure** a contribution to the epistemology of science. [134]

American speculative philosophers of science writing in prominent venues

[128] See Pepper, **World Hypotheses…**, pp. 186–187.

[129] See e.g., Kuhn, **The Structure of Scientific Revolutions…**, 3rd ed., pp. 105–106.

[130] See Whitehead, **Science and the Modern World…**.

[131] See Cohen, **Reason and Nature…**.

[132] See Wray, "The Influence of James B. Conant…", p. 16.

[133] See Wray, "The Influence of James B. Conant…", pp. 17–18.

[134] See Wray, "The Influence of James B. Conant…", p. 19.

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during the 1930s and 1940s did, however, argue for an internal role for social factors in determining proper scientific acceptance. Theodore de Laguna is an example, but many others can be offered. Thelma Zeno Lavine published a paper in the "Journal of Philosophy" [135] in which she argues for developing sociological accounts of such acceptance. A few years later, Lewis Samuel Feuer argued, in the journal "Science and Society", [136] that socio-political factors inevitably make possible the development of new knowledge during scientific revolutions, while Churchman published a paper, in the journal "Philosophy of Science",[137] according to which moral principles ought to underpin all scientific inference. So too, some American speculative philosophers of science insisted that scientific revolutions bring conceptual change, along with associated loss of content, and methodological change. Pepper is a case in point from the 1940s. [138] Different world hypotheses bring with them a reinterpretation of phenomena, on his view. Another example is offered by Smart [139] and two more, as we have seen, by the de Lagunas. Nor, as Robert Daniel Carmichael's book **The Logic of Discovery** [140] illustrates, was T. de Laguna alone in explicitly emphasising the difference between the more conservative attitudes that are to be found in pre-revolutionary science and the more radical ones dominating in revolutions, and thus in recognising normal science. The view that the principles of science are solutions to specific scientific problems, basically Kuhnian exemplars, was developed extensively by Dewey. [141]

The de Lagunas' particular significance in this context is that they explicitly put forward all of the theses Wray says Kuhn took from Conant as well as those which Wray claims are original with Kuhn. Gdeductivism endorses all but two of

[135] See Thelma Zeno Lavine, "Sociological Analysis of Cognitive Norms", *The Journal of Philosophy* 1942, Vol. 39, No. 13, pp. 342–356, https://doi.org/10.2307/2017719.

[136] See Lewis Samuel Feuer, "Philosophy and the Theory of Relativity", *Science and Society* 1947, Vol. 11, No. 3, pp. 259–270, https://www.jstor.org/stable/40399843.

[137] See Charles W. Churchman, "Statistics, Pragmatics, Induction", *Philosophy of Science* 1948, Vol. 15, No. 3, pp. 249–268, https://doi:10.1086/286991.

[138] See Pepper, **World Hypotheses...**

[139] See Smart, **The Logic of Science...**

[140] See R.D. Carmichael, **The Logic of Discovery**, The Open Court Publishing House, Chicago — London 1931.

[141] See T. De Laguna and G. A. De Laguna, **Dogmatism and Evolution...**; Katzav, "The de Lagunas' Dogmatism and Evolution...", in: Schliesser (ed.), **Ten Neglected Classics of Philosophy...**

the "Kuhnian" theses. It qualifies the thesis that scientific principles are specific rather than universal, claiming that this is only sometimes so, and does not endorse the thesis of the socially driven resolution of scientific revolutions. Tdeductivism endorses all but the idea that scientific principles are specific rather than universal.

Further, although T. de Laguna met an early death in 1930, at the age of 54, [142] everyone in the American philosophical world knew his work and that of his wife. Pepper lets us know this when he reports, in a retrospective on his life, that the couple were excellent philosophers and yet that, as everyone knew but would not say out loud, she was the better of the two. [143] Knowledge of the de Lagunas' work was further facilitated by the small size of the American philosophy of science community–there were dozens of people working in the field, [144]by G. de Laguna's continued work in the 1940s and 1950s, including her 1942 presidential address to the Eastern Division of the American Philosophical Association, [145] and by particular attention paid to "Opinion". It was published in 1930 in the first of a two-volume collection of thirty-four papers. The collection aimed to make available the ideas of the leading philosophers in America and contributors were selected on the basis of a vote at all three divisions of the American Philosophical Association. [146] T. de Laguna was thus recognised as one of the leading philosophers in the country, alongside figures such as Cohen, Dewey, Roy Wood Sellars, and Singer Jr.

Contrary to Wray, then, those theses which he takes to originate with Kuhn are likely to have come from the broader American speculative philosophy of science community. Indeed, given this and that many of Kuhn's other positions also likely come from the same community, it is plausible to think of Kuhn as importing his views from speculative philosophy. We can even think of him as a speculative philosopher, albeit one who quickly becomes part of analytic philosophy as it broadens.

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[142] See Leopoldo Montoya, "de Laguna, Theodore de Leo (1876-1930), Philosopher", *American National Biography* 2000, https://doi.org/10.1093/anb/9780198606697.article.2001540.

[143] See Pepper, **Art and Philosophy...**, p. 83.

[144] See Katzav and Vaesen, "The Rise of Logical Empiricist Philosophy of Science…".

[145] See G. A. De Laguna, "Cultural Relativism and Science…".

[146] See William P. Montague and George P. Adams, **Contemporary American Philosophy: Personal Statements**, The MacMillan Company, New York 1930, p. 9.

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The more specific influence of T. de Laguna is suggested–I claim no more than this–by the prominence of "Opinion" and the particularly striking way that T. de Laguna combines the idea that social factors are epistemologically significant with a recognition of the existence of a normal science–revolutionary science pattern. My thought here is that it is possible that Kuhn adopted much of his position from what was on the shelf at the time but found a particularly clear variant of it in "Opinion". This would explain why Kuhn did not take on board all of the de Lagunas' views, especially the evolutionary side of these. While Kuhn's selectivity could be explained in any number of ways, e.g., by supposing that he demurred from some of the de Lagunas' views or that he did not recall all that he had found in their work, it seems that another option is that he just read "Opinion", which focuses on the holistic conception of science and the related conservative-radical divide.

As for the absence of explicit references by Kuhn to work by speculative thinkers like the de Lagunas and Pepper, it is predictable. **Structure** was written during a period when analytic philosophy was systematically and successfully marginalising American speculative philosophy. This involved more or less only hiring analytic philosophers at influential departments such as Berkeley, where Kuhn ended up, influential journals coming to focus exclusively on publishing analytic philosophy, and funding bodies not funding research in speculative philosophy of science. [147] In such an environment, one's career depended on towing the line and thus in disassociating oneself from speculative philosophy. Accordingly, it would be in Kuhn's interest not to acknowledge drawing key ideas from speculative philosophy of science. The invisibility of the philosophy of science of dozens of speculative philosophers of science from the period 1920–1950 strongly suggests that Kuhn would not have been alone in failing to acknowledge the American philosophers of science he had read.

I should, before concluding, recognise that I have focused on potential American influences on Kuhn, since my goal has been to make meaningful a comparison of Kuhn and other American philosophers of science. European figures, especially Fleck and Polanyi, have also been seen as potential sources for Kuhn's ideas in **Structure**. One can argue, given the similarities between the mid-1930s work of

[147] See Joel Katzav, "To What Extent Can Institutional Control Explain the Dominance of Analytic Philosophy?", *Asian Journal of Philosophy* 2023, Vol. 2, No. 45, pp. 1–14, https://doi.org/10.1007/s44204-023-00099-7.

Fleck and **Structure** and Kuhn's acknowledgement that he had read Fleck in 1949, that Fleck had a substantial influence on **Structure**. [148] Grounds of similarity and Kuhn's reading of Polanyi in the 1950s allow arguing that his work from the second half of the 1940s onwards substantially influenced **Structure**. [149] However, given the ubiquity of "Kuhnian" ideas in the context where Kuhn worked, it seems implausible to suggest that such European figures were particularly important influences on Kuhn. They were potential influences among many others.

6. Concluding discussion

We have seen that, in 1926, T. de Laguna proposes an evolutionary framework for understanding social phenomena and their development. He applies this framework to science with the result that, by 1930, he offers an explicitly deductivist philosophy of science alongside the view that conflict between conservative and radical factors drives the development of science. We have also seen that the evolutionary framework is, in effect, already applied by the de Lagunas in outlining Gdeductivism, their philosophy of science from 1910. Gdeductivism includes an account of judgement and judgement dynamics that recognises the pervasiveness of partial truth in science and can be interpreted as a form of deductivism.

The early development of Gdeductivism and Tdeductivism thus support my thesis that the logical positivists who arrived in America in the 1930s were proposing a, to some extent successful, narrowing down of, and regress in, the philosophy of science. In addition to the exclusion of metaphysics and the neglect of existing discussions of scientific explanation and the methodology of research programmes that I noted in earlier work, we can add that there was neglect of allied developments in the logic of science, specifically of significant variants of deductivism about science, discussions of the role of partial truth in science, discussions of the extent to which scientific principles are exemplars, and discussions of

[148] See Paweł Jarnicki and Hajo Grief, "The »Aristotle Experience« Revisited: Thomas Kuhn Meets Ludwik Fleck on the Road to Structure", *Archiv für Geschichte der Philosophie* 2022, Vol. 106, No. 2, pp. 1–37, https://doi.org/10.1515/agph-2020-0160.

[149] See Struan Jacobs, "Polanyi's Presagement of the Incommensurability Concept", *Studies in History and Philosophy of Science Part A* 2002, Vol. 33, No. 1, pp. 101–116, https://doi.org/10.1016/S0039-3681(01)00031-0.

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the role of social factors in judgement dynamics.

Even as late as the early 1960s, after Popper and Kuhn become part of analytic philosophy of science alongside logical empiricism, analytic philosophy of science is playing catch up with speculative philosophy of science from 1930. Kuhn's work in **Structure** lacked, as we have seen, a developed, theoretical background and did not include a worked-out account of scientific judgement. Popper's position included a framework according to which science is governed by conventions but nothing to compare with the worked-out framework within which the de Lagunas develop their positions. In addition, Tdeductivism and Gdeductivism are better able to recognise the complexities involved in the dynamics of science than were Kuhn or Popper circa 1960. Tdeductivism better accommodates the diversity of forms of judgement needed to differentiate between acceptance for the purposes of pursuit and acceptance for practical purposes. Gdeductivism, with its focus on assessment of partial truth, also allows such differentiation. Positions focusing on partial truth were still not part of analytic philosophy of science.

The above observations, coupled with the importance usually ascribed to the work of Popper and Kuhn within anglophone philosophy of science, put pressure on the idea that logical positivism first, and then analytic philosophy of science, comprised stages of progress in anglophone philosophy of science. Given that speculative philosophy of science was ubiquitous around the middle of the twentieth century, the observations about what was lost with the loss of work such as the work of the de Lagunas also put pressure on the idea that there is a rational reconstruction of the development of twentieth century anglophone philosophy of science.

We need, in addition, further to reconsider the origins of analytic philosophy of science. We need to recognise, alongside potential originality in Kuhn's work and potential European influences on it, the likelihood that it is to a substantial degree a conduit through which the history of science informed philosophy of science as well as a variety of other standard components of speculative philosophy of science came to be part of analytic philosophy of science. Kuhn is likely to be, alongside Nagel, an important conduit through which speculative philosophy of science survived in analytic philosophy of science.

We should keep in mind that we have very partially explored speculative philosophy of science and its impact. We can expect, given the ubiquity of such work

during the first half of the twentieth century, that the loss of knowledge of philosophy of science that came with the emergence of analytic philosophy of science was substantially greater than brought out here. For the same reason, we can expect more speculative positions and arguments to have found their way into analytic philosophy of science. The extent of the challenge to reconstructions of the development of analytic philosophy of science as rational and progressive is more substantial than indicated here.

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