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"Man errs as long as he strives": Justifying the quantum of action[+*]

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Abstract

"An act of despair" is the quotation most associated with Planck regarding his postulate. Not without a reason: it is representative of what concerns the simultaneously shocking and clarifying character of the quantum of action. On emphasizing such a feeling, this quotation seems to paint the scientific endeavor in vivid subjective, and thus irrational, colors. Planck's Nobel Lecture, however rich in expressions of awe, can interestingly and ironically lead us to a very different image of science; through his process of rationalization, he enunciates the evidences, methods, and values that had to be rigged for the quantum of action to show its meaning and potential. Laudan's perspective of science as a problem-solving, rational activity concedes an interesting background of this rationalization; articulating the model to Planck's words, one can see that, even though scientific hypotheses can sometimes be almost unbelievable and underdetermination can render science despairing, it is precisely in erring and wandering that rationality can reside. Planck's Nobel Lecture can be an interesting source for story-case teaching, because it associates cognitive aspects to those rendered as human, in a historical episode already welcomed in physics teaching.

Keywords: *Rationality; Underdetermination; Case-Based Teaching; Nobel Prize.*

[*+] "O homem erra porquanto se esforça": justificando o quantum de ação

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I. Introduction

To understand what and how we see, we can classically approach the phenomenon of vision. By understanding light as an electromagnetic wave, it is fair to say that our vision of nonluminescent objects, such as ourselves, is a product of reflection. The Sun emits these electromagnetic waves we call light; when interacting with the body we see, some of the waves are absorbed, refracting into the irradiated body. Part of the waves the Sun emits, i.e. some specific wavelengths, are reflected and then meet our eyes, which send signals to our brains to formulate representations. However suitable for the interactions we lead with everyday phenomena, this explanation does not exhaust the subject. For instance, why is it that the Sun is an emitter and our bodies, reflectors? Why do temperatures of bodies seem to rise when irradiated? Why do some bodies show differences in color with rising temperatures?

Such questions associate thermal effects with optical (i.e. electromagnetic) ones. There is a reason for that: thermal effects can themselves be interpreted through classical electromagnetics. Heat is a form of electromagnetic wave. Therefore, when subjected to rising temperatures, when receiving energy, bodies not only reflect the upcoming light but also start emitting radiation, sourced in their internal energy. But it is not necessarily the rising temperatures that make a body an emitter; in fact, bodies emit waves even at normal temperature conditions. The only difference is that, at such a point, this energy is invisible, for it is mostly at infrared frequencies, not captured by the naked eye. Hence, with rising temperatures, there is a dislocation of the spectral radiancy, which is to say, a body starts emitting energy in increasingly higher frequencies, passing through the visible part of the electromagnetic spectrum. That, for instance, explains the visible incandescence in iron and other materials that can keep up with increasing energies.

As can be seen, energy plays the utmost role in thermodynamics and electromagnetism, whereas matter and movement have been the paradigmatic concepts in mechanics. But energetic considerations surely have their place in mechanics as well. From thermodynamics, it is known that energy cannot simply and spontaneously be emitted; it has to have originated somewhere. Matter becomes an important part of this equation, for it is variations in it, either in its movement or its constituents, that source the energy emitted in the form of heat, visible light, or other wavelengths. Accordingly, it is paramount to understand that intricate relations between energy and matter take place in all bodies. The classical view of simple reflections and refractions does not provide the full account of emitting bodies; naturally, the methodological power of science has come a long way since Newton’s optics and mechanics, and new experiments and models have emerged since electromagnetism was consolidated as an area of Physics.

Although not a consensus in the early 20th century, presently we understand that all bodies are made up of particles which, depending on pressure and temperature conditions, form atoms and then molecules. Going back to the latent question about the interactions between energy and matter, it is essential to take into account that bodies are constituted of a great number of particles and that any study associating energy and matter must consider macroscopic effects as products of microscopic states. That is precisely where the interesting area of statistical mechanics, first developed in the end of the 19th century, takes place. It is from statistical accounts that another central concept in thermodynamics – entropy – starts bridging the gap between the discontinuities posed by matter and the apparent continuity of energy. Understood, in classical thermodynamics, as the measure of energy unavailable for work, entropy used to be perceived as an estimate of irreversibility. Nonetheless, from the discontinuities inflicted by matter, statistical mechanics introduces a discerning new twist into the definition of entropy: this disorder, a measure for energetic macrostates, is simply statistically valid, not an absolute.

Both renderings of entropy disputed the interpretation of thermodynamic phenomena by the end of the 19th century. In that very same period, a problem arose for physicists: the black-body radiation. The Newtonian definition of a black-body was that of a black-colored body, one that would not reflect the incident light, therefore a perfect absorber and not an emitter. Electromagnetism changed its definition: a black-body would be a body that absorbs all received electromagnetic waves; thus, whichever waves emitted by it would have the body's internal energy as the source. In classical electromagnetism, a black-body is then better considered as a perfect emitter.

Empirical laws were then arranged through classical electromagnetism and thermodynamics to understand the spectral radiancy of the black-body. Physicists came up with expressions for both high and low energies. However, for any given temperature, those expressions did not seem to agree. A better analytical study was in demand, and that was Max Planck's take on the subject. Despite having achieved such a feat with a very astute interpolation based on his experience with (classical) entropy and energy, Planck was not overjoyed about the apparent lack of physical meaning of his expression. Pursuing his venture, the physicist culminated with the quantum of action, although initially not ascribing to it any energy quantization.

It is no wonder that Planck is considered the one who solved the black-body radiation problem. From his quantum of action, however, it can be said that Planck actually sowed an anomalous tree, which would, in turn, reap the fruits of quantum mechanics. Reinterpreted by Einstein, the quantum of action would soon provide a new meaning to the concept of energy and challenge electromagnetic theory. This difficulty is made clear almost two decades later, in Planck's Nobel Lecture.

By analyzing Planck's Nobel Lecture, this paper aims to show his tortuous work on understanding the meaning of his interpolation and, ultimately, the birth and meaning of the quantum of action. It is important to ensure, nonetheless, that despite admitting the arduous work done by him and all his contemporary colleagues, the emphasis here is not on an irrational account of science, but rather the opposite. Indeed, Planck's pathways were full of oddity and consequent awe, but they show a permanent adjustment in theories, methodologies, and the furthermost values of science. Among other things, his accomplishment required resorting to statistical mechanics, a striking shift of perspective for such a conservative physicist, once inclined to energism, a doctrine critic of atomism.

Planck's Lecture is analyzed through Laudan's perspective of science as a rational, problem-solving activity. Laudan's view is especially suited for this task, as it is a theory of justification and here we take on a primary source that, for its very character of being a lecture given in the opportunity of a great prize, directed to the lay audience, and many years after the research had taken place, is basically a statement of justification, a rationalization. Even if we take into account that it is a source that has to be cautiously approached, there is great educational value in it, because it is a rare example of narrative that involves expressions of emotions as much as it does of reasonings. Because of those characteristics, we understand Planck's Lecture to be a valuable source for Eshach's (2009) model of teaching based on the Nobel Prize for this, which is an episode in the history of physics already present in undergraduate classes in Brazil.

To achieve those objectives, this paper first presents a position on the use of narratives and scientific autobiographies in history of science and science education. Knowing that these sources can prove to be challenging, we seek to defend how those challenges can be overcome with methodological tools and triangulation, especially when the source at use is as insightful to a scientist's mind as Planck's lecture is. Then, we present some facts about the Nobel Prize. Those facts are particularly relevant if a professor wishes to use Planck's Nobel Prize as the background for teaching, incorporating Planck's lecture in association with other historical studies, as we suggest later on the paper. After that, we present the core of Laudan's ideas on science as a problem solving activity (Laudan, 1977) and his reticulated model of justification (Laudan, 1984), whose concepts are used to analyze Planck's lecture. Finally, we consider how a case based on Planck's lecture and other sources can be achieved, using Eshach's (2009) model.

II. On the use of narratives and scientific autobiographies in history of science and science education

Scientific biographies can be a useful source for historians of science. Naturally, they cannot be taken without caution; if, on the one hand, they can convey the human part of a personal scientific endeavor, bringing to life aspects that are normally hidden in the coldness of other historiographical sources such as papers and conferences, on the other hand, they are normally focused on an single scientist and are subject to various biases, especially from the author. As any other historical source, they must be factually and methodologically triangulated. Nevertheless, biographies have been part and parcel of any historiographical work for which they are available (Söderqvist, 2020; Kragh, 1987).

Tradition seems to point otherwise in the case of scientific autobiographies and memoirs. As Söderqvist (2020) points out, "The lack of systematic reflections on scientific autobiographies and memoirs seems to suggest that self-life-writing has not been accepted by historians of science to the same degree as biography has." When narrating their scientific lives, scientists may be prone to center their achievements, romanticize scientific progress and reinterpret mistakes and difficulties, especially in the cases in which those autobiographies are late-life works. Mythizations and ideologies implicit in these sources should also be of concern by a historian (Kragh, 1987).

These subjectivities may be the cause for autobiographies being overlooked, but are not a sufficient justification. Biographies, papers, conferences and letters are themselves prone to bias and subjectivity as well – as are historical accounts done by historians. In agreement with Söderqvist (2020), it is just a matter of degree. Those sources should not be neglected in virtue of aspects that are more or less inescapable, especially when they can provide others, such as the scientists' view of themselves and their experiences, even if a revised one (Söderqvist, 2020). The same goes for science educators interested in taking such primary sources to their classrooms.

Scientific autobiographies are narratives and, as such, cannot be transposed directly to the educational environment without mediation. In devising a model for teaching and learning based on scientific stories, which he called Story-Driven Contextual Approach, Klassen (2006) identified five fundamental contexts of a story for educational purposes: practical, theoretical, social, historical, and affective. As he delves into the historical context, he suggests, based on Kragh (1987)[2], that internal histories – or "histories of science written primarily by scientists, some of who participated in the events about which they wrote many years later" (Klassen, 2006, p. 49) – are not fit for direct transposition to the classroom, on the same grounds described above: subjectivities, mythization, ideologies.

Based on Klassen (ibid) and his own practices, but aiming to use primary sources in education, Moura (2019) further organizes those contexts in scientific, metascientific, pedagogical, in a design he calls the contextualized reading of primary sources. He applied his model to approach Benjamin Franklin's studies in an undergraduate course, using Franklin's letters and excerpts from his works and from Priestley's book on the history of electricity. Even though the latter source would qualify as internal history, it proved to be of educational interest when carefully mediated. His model seems to offer a path through which internal histories can be used.

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[2] It is interesting to note that Kragh (1987) does not dismiss primary sources such as autobiographies and narratives, although he effectively draws attention to the fact that those sources should be carefully analyzed by the historian, for they can provide explanations that do not hold true after cross-examination with other historical sources and, indeed, transmit myths and ideologies. Besides, histories of scientists must too be examined in light of their aimed audiences and the historian must take into account that memories tend to fade with time. Nonetheless, by the structure of the book, one cannot state that Kragh (1987) demonstrates any prejudice against such sources, as he discusses, for instance, the possible objectivity in the history of science, the logic of historical explanations and, especially, when he invites the reader to reflect upon the fact that history of science is history – a form of study of a very human activity. Scientists' histories provide insights into the human part of science, even if they come with challenges.

Nobel lectures can be taken as forms of scientific autobiographies and narratives. Although their structures can vary, laureates tend to give their perspectives – albeit revised – on the research they have partaken and for which they have been honored. Naturally, it is expected that they convey a rationalized overview of the scientific practices of their time, especially when ministered many years after the achievements, in a case such as the awarding of a Nobel Prize. It seems, afterall, that this is the fitting path to go about a lecture given in such a rare moment of a scientist's life. Because of those attributes, it is natural that they cannot be taken as the most objective form of historical account; however, they still constitute historical sources and, taking the careful steps previously outlined, regarding contexts and triangulations, they are of importance to historians and educators (Eshach, 2009; Whitaker, 1979).

Planck's lecture harbors some differences from those of other contemporaries. Pierre Curie (1905) and Marie Curie (1911), for instance, focus on describing the methodologies they devised to understand radioactive phenomena, the hypotheses they provided, and, in some rare occasions, recount feelings of astonishment they had with their own research. Bohr (1922), on the other hand, centers his narrative in the reach attained by his atomic model on other parts of science. A strong sense of justification comes across their lectures, as should be expected – as does with Planck's. Yet, this is a lecture also full of Planck as a person – his emotions and emotional regulations, his impressions, his adjustments in values and methodologies. It is, therefore, a document that, to some extent, provides as much discovery as it does justification. Klassen (2006) argues that students often regard science as a finished product done by great minds, which is not motivational, and that internal and autobiographical history could be the culprits. Even though this is a valid point for a variety of narratives and scientific autobiographies, that is not the case for Planck's lecture.

Once again, it should be emphasized that associations with other historical studies on the subject are demanded. Since Kuhn (1984) provided a hypothetical history of the quantum of action, other specialists have been trying to understand Planck's growing involvement with statistical mechanics before and from 1901 on (Gearhart, 2002), and the coherence and incompleteness of Planck's hypotheses and interpretations (Darrigol, 2001), to make sense of how he understood his quantum of action early on. For this reason, a professor interested in using Planck's lecture for educational purposes – as is advocated in this paper – must be aware that his lecture is not clear on how he felt in 1901 as much as he perhaps did in 1920.

III. The Prize and the lectures

Huge either in prize-money or respectability, the Nobel Prizes are a mark of our time, and becoming a laureate writes one's name in history. Appraisal comes from both those who do or do not understand the meaning of a certain discovery. When laureates are announced every October, the media dedicates various reports and analyses to the achievements. The importance is even self-proclaimed: none but the King of Sweden himself hosts the Stockholm reception of the great names of a given year (Feldman, 2000).

The Nobel Prize is a fairly recent institution; instated in 1901, from the Will of the Swedish chemist Alfred Nobel, the prize was set by that very Will to be directed towards "those who, during the preceding year, have conferred the greatest benefit to humankind". Nobel's wish was that the fields of physics, chemistry, physiology or medicine, literature, and "to advance fellowship among nations, the abolition or reduction of standing armies, and the establishment and promotion of peace congresses" – peace, as it is now known, would equally share the annual interests of most of his inheritance. The Prize in Economic Sciences in Memory of Alfred Nobel was reinforced in 1968 and first awarded in 1969, as an initiative of Sveriges Riksbank, the Central Bank of Sweden, which is responsible for financing it[3].

Being such a massive contemporary institution, the Prizes are not immune to controversies. Quite the contrary, controversies have surrounded the Prizes ever since their first origins. Nobel was best known as the inventor of dynamite and other explosives; hence, the riches shared annually between such great benefactors of humanity's knowledge were partially built up from his worldwide patents and industries[4]. Dynamite can be used for good and has done its part in the engineering and mining necessary for the development of our modern world; needless to say, however, explosives played a pivotal role in the many wars battled in the 19th and 20th centuries. His industries provided explosives for both sides battling the Franco-Prussian War, just to mention one interesting example of their reach. Even on their beneficial side, explosives were responsible for a great number of deaths and maims, due to their mishandling and overall unsafety. Analysts have been contemplating and manifesting the connections between the man and his creatures – dynamite and prize – for a long time now, and guilt has never been out of the picture as one of the reasons for his Will (Feldman, 2000).

Controversies also arose after his death. His Will, "a masterpiece of legal ambiguities, vagueness, and omissions" (Feldman, 2000, p. 40), caught its executors off guard and challenged them to such an extent that it took almost five years to come up with the Prizes. The Will mentioned a foundation that did not exist; family members disputed part of the wealth; France and Sweden battled for the right to Nobel's estate; part of the Swedish media opposed the Prizes in virtue of the country's poverty. But none of this was as difficult as designating members of the committees necessary for such a burdensome task: keeping up with the world's knowledge production, curating nominations, and, of course, making the strenuous decision of granting the great values with the least of altercations. Nowhere on the basically single-paragraph Will was any sort of compensation specifics to those who would have to dedicate themselves to the assignment. The pointed institutions also puzzled analysts everywhere, since Nobel only lived in Sweden during his early childhood. In physics and chemistry, prizes would be decided by the Swedish Academy of Sciences; in medicine or physiology, by the Karolinska

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[3] From the Nobel Prize homepage. Quotations references are due to Alfred Nobel's Will. NobelPrize.org. Nobel Prize Outreach AB 2021. Sat. 28 Aug 2021. <https://www.nobelprize.org/alfred-nobel/alfred-nobels-will/>

[4] Nobel also had shares of his brothers' oil company, which greatly contributed to his wealth (Feldman, 2000).

Medical Institute; in literature, by the Academy in Stockholm, and the one in peace, by a Committee of five appointed by the Norwegian Storting (ibidem).

Even after its jumpstart, controversies have never ceased. Naturally, they are more frequent in peace, which is a booming political prize. Literature has been not only very Eurocentric but also dodging pressing explicit political issues; notwithstanding, pretense neutrality is a political stance in itself. And political considerations undoubtedly permeate even the scientific prizes, for there are no guarantees of science's neutrality and impartiality. Laureates like Fritz Haber and Otto Hahn have been heavily associated with war weapons. Aside from politics, polemic also emerges because of neglected names such as Dimitri Mendeleev, Lise Meitner, Chien-Shiung Wu, and Rosalind Franklin. It should be mentioned, too, that during the 1930s, yearly prizes in chemistry and physics were withheld and the funds were directed to swedish research (Sime, 2013). A great name as Enrico Fermi was awarded for a discovery that would soon after be understood as equivocal. Some economics laureates have advocated for the altogether halt of the very Prize in Economic Sciences. And these are just the best known of the many examples of controversies besieging the Prizes.

There is a structural problem in annually awarding the greatest benefits of the preceding year, as Nobel desired, since in none of the Prize's fields are there immediate astounding discoveries and masterpieces. Science, politics, and arts are life-long endeavors and the social importance of great feats of humanity can take years, perhaps decades, to establish themselves. Nobel Prizes are not granted posthumously, so many potential candidates died before being acknowledged. But one cannot ignore that the committees often avoid awarding some well-known great names. Planck had been considered since 1907, being finally conferred the Physics Prize of 1918 (in 1919), cracking open the windows for Einstein and Bohr, and later many others working in the new paradigm of quantum physics.

Paragraph nine of the Nobel Prize Statutes mentions "It shall be incumbent on a prizewinner, whenever this is possible, to give a lecture on a subject relevant to the work for which the prize has been awarded"[5]; it goes on saying that said lecture should be given before or no later than six months after the ceremony. This has not always been the case, however. Pierre Curie lectured in 1905, in virtue of the 1903 Physics Prize, and Planck ministered his masterclass in 1920, just to cite two examples. Notwithstanding, from the last decades on, probably because of the relative peace Western countries have been experiencing, most laureates have been showing up to the solemnity.

Addressed to the lay audience, Nobel Lectures are generally historical and ministered in didactic language. This means scientists try their best to communicate the importance of the discovery, work, or masterpiece for which they have been prized. In physics, one can see a pattern of appealing to history. That could be for justification of their work; it could also be

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[5] Information gathered from the Nobel Prize homepage. Quotation reference is due to Statutes of the Nobel Foundation. NobelPrize.org. Nobel Prize Outreach AB 2021. Sun. 29 Aug 2021. <https://www.nobelprize.org/about/statutes-of-the-nobel-foundation/>

because they suppose it is more interesting or didactic; at long last, it could be because they want to praise their colleagues, or maybe all the alternatives. Generally accessible, since they have been translated into English when necessary and are free to read at the Prize's internet homepage, these documents are some educationally valuable primary sources, a fact that has already been pointed out in the 1970's (Whitaker, 1979).

Planck's lecture follows this logic, as he mentions in its beginning

> *If I take it correctly that the duty imposed upon me today is to give a public lecture on my writings, then I believe that this task, the importance of which I am well aware through the gratitude felt towards the noble-minded founder of our Foundation, cannot be more suitably fulfilled than by my trying to give you the story of the origin of the quantum theory in broad outlines and to couple with this, a picture in a small frame, of the development of this theory up to now, and its present-day significance for physics.*

IV. Justification – Laudan's perspective on the progress of science

> *In appraising the merits of theories, it is more important to ask whether they constitute adequate solutions to significant problems than it is to ask whether they are 'true,' 'corroborated,' 'well-confirmed' or otherwise justifiable within the framework of contemporary epistemology* (Laudan, 1977, p. 14).

As can be seen, by shifting the perspective to problem-solving as an indicator for progress, Laudan intends to stray away from certain values that are philosophically challenging when it comes to analyzing the history of science, for a theory being "true" or "corroborated" depends on what is defined as fact in a given historical moment. On the other hand, in what seems (only superficially, as he defends) to be an undisputed allegation – that science progresses foremost in solving problems – Laudan (1977) designs a taxonomy for scientific problems and later, a model of justification able to rationalize how scientific consensus is ultimately achieved (Laudan, 1984).

Classifying scientific problems appears to be an easy enough task. Conceptually, Laudan states that problems often arise from a theory's internal inconsistency or the incoherence between various canonic knowledge. However, it is mostly problems of empirical nature that have been scrutinized by philosophy, albeit marginally. Starting from the latter, Laudan proposes a more thorough taxonomy, breaking up subclassifications for both kinds of problem and, for the former, even grasping some conceptual problems deemed metascientific by previous philosophers.

Admitting empirical problems are easier exemplified than defined, Laudan (1977, p. 15) tries a delineation anyhow:

> *More generally, anything about the natural world which strikes us as odd, or otherwise in need of explanation, constitutes an empirical problem [...] Empirical*

> *problems are thus first order problems: they are substantive questions about the objects which constitute the domain of any given science.*

Empirical problems come in three sorts: unsolved, solved, and anomalies. Unsolved problems can certainly be an indicator of the incompleteness of a theory, with which scientists are supposedly familiar. Yet, Laudan (ibid) understands such problems are ambiguous: nature does not speak unequivocally to inquirers; consequently, margins of errors and deficiencies in equipment that are typical of empirical research, for instance, often deviate a scientist's ability to recognize an unsolved problem. Also, some empirical problems take time to be understood as such in virtue of the inherent difficulty of assigning the area of investigation to which they belong. Laudan (ibid, p. 18) insists that "*unsolved problems generally count as genuine problems only when they are no longer unsolved*". It is in hindsight, by solving a problem, that an unsolved problem is recognized as it once was, endorsing the evaluation of the progress of a theory.

How is it possible, then, to perceive a problem as solved (and hence the previous existence of an unsolved one)? To answer such a question, Laudan (ibid) ascertains three fundamentals: the approximative character of problem solution, the irrelevance of truth and falsity to solving a problem, and the frequent non permanence of solutions. Laudan (ibid., p. 22) defends that a problem is considered solved

> *when [scientists] believe they understand why the situation propounded by the problem is the way it is. Now clearly, it is theories which are meant to provide such understanding and any reference to a solved problem presupposes the existence of a theory which purportedly solves the problem in question. So when we ask whether a problem has been solved, we are really asking whether it stands in a certain relationship to some theory or other.*

Differently from unsolved problems, anomalies can be openly recognized by scientists and have had their importance historically known to philosophers. Like some of his contemporary critics of the classical perspective, Laudan is keen to state that anomalies do not demand the abandonment of the theory to which they raise doubts. In order for a completeness of argument, he adds a point of controversy, emphasizing that anomalies are not necessarily incompatible to the theories to which they are anomalies. Critics of the classical view often "hold that an anomaly is only generated when there is a *logical* inconsistency between our 'theoretical' predictions and our 'experimental' observations [...] a far too restrictive notion of an anomalous problem" (ibid, p. 28). Understanding science as an interpretative endeavor, if an anomaly does not mean a theory should be abandoned, there can exist "*a class of nonrefuting anomalies*" (ibid. p. 29). His defense is that an anomaly can only be seen as such if there are competing theories, for one of which there is a solved problem that constitutes an anomalous problem for the other. "We must here satisfy ourselves with the observation that unsolved refuting instances are often of little cognitive significance" (ibid., p. 30).

Since Laudan is concerned with a rational account of science, he defends that it is not the number of anomalies that provoke theory choice – as Kuhn advocated – but rather their cognitive importance. Their cognitive importance can only be assessed in the context of competing theories, which means scientists tend to maintain their allegiance to a theory, even if it shows many lackings, in the case it is the only one available. Even in the case of competing theories, scientists still have to deal with the degree of discrepancy between the theoretical predictions and experimental results and the time it takes for a problem to be accounted for as an "epistemic embarrassment" (ibid., p. 40). In summary, anomalies take time, new theoretical perspectives, and methodological refinement to be understood as such, for it would constitute irrationality to simply discredit a theory otherwise capable of successfully explaining some known phenomena.

With conceptual problems, once again rationality is on the table. Historically, writers had been presuming conceptual problems as metascientific. "Rather than seeking to learn something about the complex nature of scientific rationality from such cases, philosophers (with regret) and sociologists (with delight) have generally taken them as tokens of the irrationality of science as actually practiced" (ibid., p. 47). Despite philosophers and sociologists, the history of science is rich in controversies that originated from conceptual problems that do play an imperative role in theory choice for scientists, which should grant them a more thorough analysis, instead of their complete neglect as metaphysical. To do it, Laudan (ibid, p. 48) defines

> *Conceptual problems are characteristics of theories and have no existence independent of the theories which exhibit them, not even that limited autonomy which empirical problems sometimes possess. If empirical problems are first order questions about the substantive entities in some domain, conceptual problems are higher order questions about the well-foundedness of conceptual structures (e.g., theories) which have been devised to answer the first order questions.*

Conceptual problems can arise from scientific theories, methodologies, and extra-scientific values. Theoretically, incompatibilities and inconsistencies may bring to light difficulties in both theories in appraisal, as has been seen in the taxonomy of conceptual problems. Methodologically, theories generated from different perspectives on the best way to inquire about nature are generally conceptually problematic – the mechanistic views of nature of Newton and Descartes being a good example. In terms of values, or worldview difficulties, many conceptual problems come from beliefs that are generally deemed as metascientific, ethics and theology included.

It is fair to say, then, that there are many instances from which comes the frequent dissensus in science. Laudan (1977) delineated their taxonomy and their origins quite interestingly. However, Laudan (1984) argues that, among all areas of academic expertise, hard sciences are reasonably and comparatively consensual. To explain how such a consensus can occur in an activity with so many variables open to criticism and dissent, Laudan (1984) continues designing his model of justification.

To explain this apparent contradiction, Laudan (1984) proposes a reform in the hierarchical model of justification, or the theory of instrumental rationality, which was a proposition of logical positivism. This model has been fairly criticized by post-positivist philosophers mainly for two reasons: (i) it is an overly rigid structure, rendering any dissensus as a cognitive problem of the individual scientist who happens to be at odds with the scientific community, and (ii) it does not account for the underdetermination thesis, which states that there are no logical guarantees that a certain set of methodological rules can determine a factual dispute, and likewise, a set of objectives of science is not able to definitively solve a methodological dispute. Thus, underdetermination runs through theories, facts, methodologies, and objectives. The factual existence of controversies in science, born in the criticism typical of the activity, would translate science into an irrational activity.

Laudan (1984) intends to rebut this relativism, defending that underdetermination in itself is not a sufficient argument to put in jeopardy the rationality of science. The three dimensions of science – factual, methodological, and axiological – exist for a reason, and although there are no logical guarantees that the invocation of one or another level of scientific activity is fully capable of resolving disputes, it can determine which of the claims (factual or methodological) best furthers the objectives of science. To do this, however, it is necessary to assume that the objective of scientists is not to seek the best or truest theory, but to propose the theory best supported by evidence and methods, assured by the critical scrutiny of a research community – even if it is impossible to suppress its underdetermined character.

But what is exactly the hierarchical system of justification? Logical positivism understood that disputes within the factual scope would be resolved in the methodological field; controversies at the methodological level, in turn, found their solution in the axiological domain. Granted its critics were reasonably right to demonstrate its rigidness and blindness to underdetermination, they neglected the model altogether, without perceiving its potential. The problem resides in its hierarchical structure, not in its dimensions.

Analyzing the factual domain, for instance, demands understanding not only the dynamics between hypotheses and procedures, typical of the factual and methodological levels, but also its relation to the axiological domain, which is in fact one of the characteristics neglected by positivists. As he defines, “Factual beliefs, thus shape methodological attitudes every bit as much as our goals do” (ibid. p. 39). The hierarchical conception, with its openly one-dimensional character, misses the fundamental role played by facts in the development and choice of scientific procedures.

It is at the axiological level, however, that Laudan (1984) finds space to criticize positivist and post-positivist assertions and construct his own model of justification. He starts by presenting the covariance fallacy, which “assumes that the presence or absence of consensus with respect to factual claims can be used to infer the existence of agreement or disagreement with respect to cognitive aims” (ibid., p. 43). He believes that is the case of Kuhn’s paradigms and revolutions, since adherents to different paradigms are supposed to share the same ontology and the same axiology. Laudan (1984, p. 44) invokes later work by Kuhn (1977) to show this contradiction

> *Precisely because (as Kuhn himself stresses in other contexts) cognitive values underdetermine methodological rules, and because those rules in turn sometimes underdetermine theory preference, it is entirely conceivable that two scientists may subscribe to precisely the same cognitive goals as yet advocate fundamentally different views about the furniture of the universe.*

The same goes for every dissensus. Underdetermination demonstrates that factual consensus – something very typical of science – does not mean axiological consensus. Scientists can work on the same canon and have differences of values, objectives, and world views. Consensus at one level does not necessarily entail consensus at the others.

As Laudan's aim is to show the rationality of science, he proposes using the best of both positivist and post-positivist arguments and develops a system of justification. Hierarchy no longer stands as a feature, but the dimensions of science remain. On the other hand, underdetermination is accounted for, with adjustments and justifications for all dimensions. He calls it the reticulated model of justification. Methods justify theories, as much as theories constrain methods; methods exhibit realizability of aims, as much as aims justify methods; theories and aims must be harmonized. With this model, it is possible to analyze which values are cognitive, utopian or falsely claimed, for they are, in some form, constrained to methods and facts and open to wide criticism. It offers, therefore, a tool for some sort of rationality vigilance in analyzing the scientific claims and practices.

It is important to note that Laudan's perspectives center in science's justification and, for that reason, are not consensual amongst philosophers and historians (Kragh, 1987). Justification is scientists' way of providing a rational explanation for their researches and discoveries, and tend to disguise more subjective elements that are of special interest to historians, whose aim is to put forward explanations for facts of the past. However, justification is an integral part of the scientific endeavor and has a relevance of its own, since it bridges the gap between the scientist and the scientific community. As we take on a primary source that is a personal narrative and, therefore, an object of justification, Laudan's view is especially suited for the task of the analysis of Planck's lecture.

V. Justifying the quantum of action – Planck's Nobel Lecture

As of the end of the 19th century, "the continuity of all causative connections"[6], a conception produced by the overwhelming power of the infinitesimal calculus, was a propositive idea of the engineering of nature for every area of classical physics, even as

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[6] As his Lecture is available at the Nobel Prize Foundation homepage, we will not be able to identify the pages of the quotations. <https://www.nobelprize.org/prizes/physics/1918/planck/lecture/>. However, reference is due to Planck (1967). All direct quotations without citation in this section come from this document.

disputing as they were of certain objects. Besides its conformity to electromagnetic theory, thermodynamics, and mechanics, continuity was also compatible with everyday experience. Any suggestion at odds with it would certainly not surface conceptually, but rather from an empirical problem. Planck's lecture, a rationalization of the setting of this disruption, is symbolic not only of the hardships discontinuity imposed on science but also on our very expectations of how nature itself behaves.

Emission and absorption spectra garnered great interest in the 1850's and 1860's. Empirical evidence pointed to a direct relation between a body's absorption and emission spectra. Studying those, German physicist Gustav Kirchhoff (Planck's predecessor in Berlin) (Kragh, 2000) noticed an interesting pattern: the absorption spectrum of a given substance was the reciprocal of its emission spectrum, the very line indicating the same wavelength in both spectra. This study allowed him to identify, for instance, substances existing in the Sun and opened up the possibility of understanding the composition of the stars. On the other hand, the same study predicted the existence of elements later discovered (Thomas, 1991).

Studying thermal radiation, Kirchhoff identified that there is a constant ratio between a body's power of emission and power of absorption of a given radiation at a definite temperature. The consequence of such a relation is that the greatest absorbers should be the greatest emitters. This specimen was called a black-body by Kirchhoff; for further investigations, Kirchhoff modeled a cavity, with a proportionately small hole, as a means to understand the black-body radiation. His studies later indicated that the ratio of the spectral emittance and its absorption coefficient would be a function only of the wavelength and the temperature – the spectral radiancy (Thomas, 1991).

In the following decades, experimental and theoretical physicists struggled to formulate an analytical expression for the spectral radiancy of the black-body. Strange expressions emerged and, as experimental methods were better refined, they culminated with one of the most famous problems in Physics – finding an expression that would accommodate both high and low energies of the empirically obtained curve for a given temperature. This empirical problem, labeled by Paul Ehrenfest in 1911 as the ultraviolet catastrophe, gave rise to a conceptual problem, that of the interactions between radiation and matter. On this note, Planck introduces his lecture:

> *When I look back to the time, already twenty years ago, when the concept and magnitude of the physical quantum of action began, for the first time, to unfold from the mass of experimental facts, and again, to the long and ever tortuous path which led, finally, to its disclosure, the whole development seems to me to provide a fresh illustration of the long-since proved saying of Goethe's that man errs as long as he strives.*

After outlining the state of the art on the black-body's spectrum, Planck presents his early take on the matter before the emergence of the "ultraviolet catastrophe", but nonetheless a problem in itself, that of an analytical expression for the black-body's radiancy. In the hopes of tackling it through classical electrodynamics, he approached the cavity as a setup of Hertzian oscillators. It is licit to say, therefore, that, in the face of an empirical problem, Planck started working on a theoretical model, that of the cavity being constituted by resonators. It seemed fruitful: this model was empirically tested and established the connection between the energy of the resonator and the energy of radiation; furthermore, this connection was independent of the nature of the resonator. He affirms having happily welcomed this result, for his model appeared to reproduce the thermal cavity and allowed him to approach black-bodies from the energy of the resonators, lowering the system's degrees of freedom down to one.

It was not long until Planck's expectations were shown to be limited by the hertzian resonator model, for there was no way to distinguish the emitted and the absorbed radiations, intended by Planck with this specific model. "The resonator reacted only to those rays which it also emitted, and was not in the slightest bit sensitive to the adjacent spectral regions". His expectations surrounding the analysis of entropy were also defied by mechanical arguments and gradually showed that "an important connecting element or term, essential for the complete grasp of the core of the problem, must [have been] missing". Planck's initial efforts demonstrate the intricate negotiations between theoretical and methodological assumptions and his openness to their permanent adjustments. Moreover, these adjustments started manifesting that the object demanded a new element, although it would only later prove to be one of an impressive kind.

As the dead-end electromagnetics appeared to turn out – especially in virtue of Planck's distrust of Lorentz's electron theory, which he acknowledges in 1920 would have simplified his attempts to approach the black-body – the physicist decides to turn to thermodynamics, an area with which he was more comfortable, in another theoretical adjustment, as can be seen. Afterall, entropy is a great conceptual tool to approach thermal equilibrium. The Second Law was his expertise, the subject of his doctoral dissertation (Kragh, 2000); hence, his take on the problem arose from the second derivative of entropy with respect to energy, since it had "direct physical meaning for the irreversibility of the energy exchange between the resonator and radiation".

Meanwhile, new empirical results came to light, bringing to surface the infamous ultraviolet catastrophe; the expressions for high and low frequencies did not agree and several ad-hoc attempts were conducted to find such agreement, resulting in very strange and complex new expressions, which Planck is adamant in classifying as unorthodox for nature's phenomena. His knowledge of the meaning of entropy's second derivative came in handy. Adjusting theoretical and empirical facts, taking the expressions obtained for both parts of the spectrum, and calculating their related second derivative of entropy with respect to energy (whose reciprocal he named R, a function of temperature and frequency), he comes up with two discerning, but associable results: in the case of large wavelengths, R is proportional to energy; for the small ones, R is proportional to the square of energy. "There was no better alternative but to make, for the general case, the quantity R equal the sum of two terms, one of the first power and one of the second power of the energy so that for small energies the first is predominant, whilst for the greater energies the second is dominant”.

His expression reasonably fitted the spectrum, even if in 1920 he mentions believing it still required a more exact confirmation. But in 1900 Planck’s worries went far beyond the expression’s power in resolving the problem. His axiological expectations were clearly challenged by it: certainly, his expression worked accurately, which is a sought-for value in every scientific law; its “true physical character”, however, made him classify it as a “happily chosen interpolation formula”, which did not fit his perspectives on nature. It is worthy to mention Planck’s worries from Laudan’s (1977, p. 16) point of view: “solving a problem can not be reduced to ‘explaining a fact’.” Planck’s quest makes him delve into “weeks of the most strenuous work of [his] life”, in which he, who previously addressed entropy in phenomenological fashion, i.e. thermodynamically, would have to somehow turn to statistical mechanics.

To approach entropy itself, and not its variation, he fixed the necessary “additive constant that energy and entropy disappear together”. That was not a novelty, as he assures that, before him, other scientists like Wilhelm Ostwald had taken energy – which is not an absolute – approximately as such to study thermochemical phenomena. The additive constant, on that occasion, was not even defined, as it would later be by the theory of relativity. There can be seen, therefore, a methodological adjustment, as Planck uses creativity and relies on previous methodologically accepted knowledge to design a new approach to the problem of entropy, defining the additive constant altogether.

It is important to note that, even before that, in 1899, Planck had already designed an expression for the spectral density as proportional to energy as a function of temperature and frequency. Associating his interpolation to that expression, two constants were necessary. One of them showed to be proportional to Boltzmann’s constant. To define the second, his consideration of energy and entropy disappearing together led him to a relatively simple combinatorial method, for which, Planck says with much satisfaction, Boltzmann vouched. Considering the system as constituted of a great number of oscillators, he delved into the system’s probability for a certain energy configuration, correlating it to entropy’s expression, allowing him to interpret it as a distribution of probabilities. But the emerging constant, the product of energy and time, “proved elusive and resistant to all efforts to fit [...] into the framework of classical theory”. As a computational resource, he deals with the total energy as a composition of finite elementary energies, which framed energy in probabilistic nature. The constant was thus named quantum of action.

> *Either the quantum of action was a fictional quantity, then the whole deduction of the radiation law was in the main illusory and represented nothing more than an empty non-significant play on formulae, or the derivation of the radiation law was based on a sound physical conception.*

The continuity of all causative connections was in jeopardy and Planck justifies its reality with subsequent theoretical and methodological developments made by his peers. Notwithstanding its soundness, accuracy, and simplicity, his radiation law and the quantum of action still brought him much discomfort. In 1920, cautious as he is about his justification, fertility is the one single value of which he speaks the highest, mentioning research on the specific heat of solid bodies and of hydrogen, the rotation of gas molecules, the determination of chemical relationships, the development of methods suitable for X-rays, and spectroscopy. New theories he particularly emphasizes are Einstein's light quanta and Bohr's single electron atom. Indeed, in a narrative full of awe, novelty, and adjustments in theory, methods, and values, Planck honors his primary objective of showing man errs as long as he strives.

As far as Planck's narrative goes, one can clearly see that the quantum of action was not a theoretical choice done in virtue of the canonical methods and values. Contrariwise, the scientific community faced an empirical problem; the accuracy of the expressions for the black-body's radiation was limited. It demanded that Planck borrowed the model of oscillators and, despite its methodological success, seeing its theoretical limits, he had to work on a shift of perspective, seeking firstly the phenomenological approach to entropy. His interpolation was accurate enough, but this was not sufficiently valuable, at least not for him. Challenged by his expectations of nature's simplicity (whatever it might mean, as he says), he maintained the labor of finding meaning to his radiation law. Once more, he shifts his theoretical perspective, addressing the problem statistically and, seeing the values of classical physics were not able to do the job, Planck solves the problem, already one of a theoretical status, by understanding the interactions between matter and energy from an elementary viewpoint.

When Laudan (1977; 1984) constructed his analysis, he was primarily addressing the communal, critical work done by many scientists; here, we analyze the partial resolution of a conceptual problem identified by one particular physicist, born from an empirical problem. However, it is fair to say that the latter was detected by the scientific community – meaning, the analytical formula for the spectral radiancy. Wien devised an expression that accounted for the higher frequencies and low temperatures; Rayleigh and Jeans developed, from classical electromagnetics, an expression for lower frequencies and high temperatures. Solving this problem created an anomaly, which resided precisely in the fact that electromagnetics could not explain Wien's expression.

It should not be missed that, as clear as it can be nowadays for those initiated in physics, the quantization of energy was not the straightforward conclusion one could draw from the quantum of action. In fact, it seems Planck did not consider the discontinuity of energy in the years following his proposal of the quantum of action, and the challenge it offered to classical physics. Even after resorting to statistical mechanics, he merely thought of it as a tool, only making peace with its interpretation of entropy in 1912 (Kragh, 2000). It is worth mentioning that that is precisely what made Einstein's resolution of the photoelectric effect something revolutionary, for understanding light as quanta was one of his axioms when addressing the problem, and approaching the radiation typical of Wien's equation as one would approach an ideal gas was his justification.

Planck was one of the many scientists who struggled with Einstein's light quanta. It is perhaps curious to note that Bohr, too, was one of the physicists who resisted the quantization of energy, although his very atomic model relies on quantized energy layers to prevent Rutherford's atom from collapsing. In Nobel terms, it would be impossible to recognize Einstein and Bohr, if it were not for Planck's quantum of action.

> *One would hardly have picked Max Planck (1858-1947) for taking the most revolutionary step in modern atomic physics [...] He downplayed his own bombshell insight, and it was Einstein who first pushed or perhaps even first recognized its revolutionary implications. Planck, though the Galileo of quantum physics, worried throughout his life about the dreadful break he had caused with classical physics and causality. Nonetheless, at age forty-two, he did make the break, did boldly plunge into unknown territory as other eminent physicists had not dared or been insightful enough to do. Both his conservatism and his radicalism reflect what everyone said about Planck: he was a model of integrity* (Feldman, 2000, p. 139).

Feldman's account seems very precise and Planck's conservative audacity certainly transpires in his Nobel Lecture, as he guides the audience into a journey of justified innovation, just as a new theoretical and axiological canon germinated. Integrity is also an important term that defines his masterclass, as he is thorough in constructing his narrative, theoretically and methodologically justifying every step of his quest. Displaying utter caution toward the topic, he rationalizes an integral history of the quantum of action, even if in the previous decades he was known to have qualified his great triumph as an act of desperation.

VI. Educational perspectives

Courses aiming to teach quantum mechanics are inevitably introduced by approaching the black-body problem. It was, afterall, a game-changer in physics, even though it was not immediately recognized as such by Planck. Teachers, professors, and books invariably point out that the empirical evidence collected in the 19th century defied classical electromagnetism, especially the part of the spectrum ruled by Wien's law of distribution, alluding to the famous ultraviolet catastrophe. It is no wonder anyone would choose to do as such; as has been advocated, the quantum world is so far off from our everyday experience, so far off any classical theory of physics, that this sort of introduction seems inevitable and even desirable.

Science education researchers, philosophers and scientists alike have been defending that the history of science should be widely taught (Matthews, 2012). It shows potential to humanize and socialize science, to introduce students to scientific reasoning and methodologies, to show the value-ladenness of theories, to discuss the nature of science. On the other hand, research also shows the difficulties involved in such an endeavor; issues involving how, what, and when to teach are always under consideration (Martins, 2007). It is fair to emphasize, however: what if one part of the history of physics were already traditionally welcomed in the classroom, as is the case of the black-body problem? In this case, what and when to teach seem to be solved problems, remaining nonetheless the question of how to teach it.

Some of the most used textbooks in Brazilian curricula introduce quantum mechanics by resorting to history to some degree. Eisberg and Resnick (1985) introduce their first chapter alluding to Planck's seminal paper and close the same chapter with a small historical section. Some historical introductions can also be seen in Bohm (1989). Brazilian author Nussenzveig (1998) intercalates content and history, just as Haliday, Resnick and Walker (2018) and Tipler (1978) do. Approaching the history of the black-body was never the primary goal of these textbooks; consequently, they show small historical lackings and problematic associations, which can lead readers to incorrect conclusions, such as Planck being the proposer of the quantization of energy. It is worth noting that textbooks are the final product of a very complex process and their authors should not be at fault for historical issues. Our point here is that they open the possibility for the educator who is guided by them to approach this history in their own manner, as the textbooks themselves choose to use history to introduce the subject. However, it is clear that they are not enough as historical sources.

Teachers and professors often struggle to find good historical sources for their classes. In this paper, we argue that Planck's Nobel Lecture is a very fruitful source that can be used with educational purposes. As Whitaker (1979, p. 242) argues, Nobel Lectures are "obviously authentic and very often at a reasonable level for a student"; as Eshach (2009, p. 1379) indicates, "Many of the lectures, in fact, are themselves gems of scientific exposition". It is, of course, paramount to be careful with most of them, for scientists, being in the spotlight for the highest laureate of their lives, can be too self-centered, too generous with their fellow colleagues, or even too technical. Also, one should take into account that most scientists receive their Nobel Prizes many years after their discoveries and time is known to change people's perspectives (Kragh, 1987). Yet, it is our position that Planck's lecture is, in fact, one of the gems to which Eshach alludes, for its content hardly deviates from his autobiography or from the letters to which historians have had access. More importantly: it shows a myriad of aspects about the tortuous paths involved in scientific reasoning and, ultimately, rationality.

Case-based teaching is a methodological teaching tool that differs from the traditional approach to teaching science. Normally, teachers resort to teaching scientific theories as rules to be applied in extensive exercises; by using open cases, however, science teaching and learning go from algorithmic to investigative. Story-cases, which are historical cases, can be full of characteristics not apprehended by rules, bringing to classrooms aspects of science often neglected by the traditional teaching: emotions, creativity, imagination. Those are characteristics overly advocated by science education researchers and, in a story-case teaching, do not demand that typical contents – theories and concepts – be relegated, but rather enriched (Eshach, 2009).

Using Planck's Nobel as a story-case, during a period of no more than two classes, can be a useful tool in undergraduate courses. However, it demands careful considerations and, in that case, triangulations. One should understand that Planck's narrative is his own perspective and was ministered almost twenty years after his works on the black-body problem, and for that reason, is more about him at that moment than it is about him in 1901. Hence, an educator must consider expanding the sources, in order to differentiate the man from the concept of the quantum of action. In this case, we suggest resorting to the researches of Darrigol (2001), Gearhart (2002) and Kragh (1999), especially to understand the role played by Planck's knowledge of and critiques to statistical mechanics before and in the years right after his proposition. Those are unexplored points in Planck's lecture.

It should also be noted that the mediation made by the professor should take into account special methodological considerations. Planck's lecture, although a valuable source for a case based on the Nobel Prize, cannot be taken at face value. Creating a story, even if an oral one, surrounding his lecture demands an understanding of the contexts essential for science teaching. Even though we raise objections to Klassen's (2006) proposed model in the case of avoiding histories written by scientists, the five contexts he explores are certainly a valuable tool for the construction of the narrative suited for the classroom. In Brazil, Moura (2019) adapted Klassen's model for the use of primary sources and his proposed model is also of value. Karam (2021) enumerates five criteria for selecting primary sources for educational purposes. They should (i) have insightful and short excerpts, (ii) have notations that can be translated into modern ones when necessary, (iii) be guided by clear educational objectives; (iv) be supported by good secondary sources, and (v) be able to be compared to the context present contemporary in contemporary textbooks. If the teacher or professor feels the need to write a story based on Planck's lecture and other sources, developments by Schiffer and Guerra (2019), Klassen (2009) and Metz et al (2007) show significant contributions to the logic and methodology of building such narratives, with very good examples.

For Eshach (2009), a good case-story based on the Nobel Prize should be able to (a) fit the curriculum, (b) involve a chronological plot, (c) use authentic material, (d) contain the scientific context of the discovery, (e) associate the social and political aspects of the nomination, (f) discuss if the discovery was revolutionary, and (g) bring about discussions on the nature of science. A story-case based on Planck's Nobel, using his Nobel Lecture, will certainly meet almost all the requirements, with deepening necessary in some of them, as should all historical sources used for educational purposes.

As has been said, the black-body problem is already one that is mostly addressed historically and is traditionally a way to introduce any physics class regarding the structure of matter, meeting requirement (a). Planck's lecture is an authentic material which is accessible to undergraduate students, allowing it to be read at full; besides, Planck chose to narrate not only his quest, but also his colleagues' posterior inquiries, in a historical, hence chronological manner, meeting requirements (b) and (c).

Some deepening in the scientific context is needed to meet requirement (d); even though Planck's lecture hints at various issues and textbooks present the challenge the black-body was to electromagnetism, it is really important to emphasize the intricate relations between mechanics, thermodynamics, electromagnetism, and statistical mechanics for the achievement of Planck's postulate. Kirchhoff proposed the problem in 1860, more than a decade before Maxwell's synthesis of electromagnetism, when studying emission and absorption spectra (Thomas, 1991). There was no physical explanation for these spectra, only indications that they demanded a more profound understanding of the structure of matter, which would only come in 1913, with Bohr's atomic model (Kragh, 1999). Thermal radiation and spectra were an important study during the industrial revolution, since they were a tool for understanding the high temperatures of thermal machines. The second law of thermodynamics, an integral part of Planck's resolution, also demands addressing, since its meaning was disputed by thermodynamics and statistical mechanics, the latter being at somewhat of a disadvantage at that point, although it would become the main resource for Planck's quantum of action. Electromagnetic waves were empirically detected and produced by Hertz in 1888, even if predicted by Maxwell in the previous decade (Harmann, 1982). The electron became a particle with Thomson's 1897 research, determining its mass and charge – which explains Planck's distrust of Lorentz's electron theory in 1899 (Kragh, 1999).

As for requirement (e), Eshach (2009) suggests delving into the social and political context of a nomination – the exploration of discussions amongst the members of the committee, the yearly indications and possible reasons for the laureation in a certain year. The very process of indications is a form of social and political discussion, even if an internal one. In the case of Planck's nomination, these data are not easily found, but some information can be gathered in the Nobel Prizes's homepage[7]. He had been indicated by various colleagues every year since 1907, receiving a record eleven indications in 1914. One can see, however, a stark preference displayed by the Nobel Committee to laureate empirical research in the Prize's first two decades, which can be attributed to the expertises of the members, as well as purely theoretical physics being somewhat of a novelty during that time. It is not to say that Planck was an overly theoretical physicist, which would be untrue. He was afterall working on the theoretical resolution of an empirical problem. Yet, nominating him would certainly open the doors to other more theoretical physicists, as Einstein and Bohr, and to a whole new realm of still shocking new phenomena – as it did, indeed (Feldman, 2000). The role played by the First World War should not be forgotten, either, since it is the reason Planck received his 1918 Prize in 1919 (year in which he still received six indications, Einstein and Wien along the indicators).

It is common sense that Planck's work set a revolution in motion, meeting requirement (f). The quantum of action would be shortly considered a measure of the quantization of energy,

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[7] <https://www.nobelprize.org/prizes/physics/1918/planck/nominations/>.

with Einstein’s work on Wien’s distribution law and its use as an explanation for the photoelectric effect. As has been said, the quantization of energy was not Planck’s conclusion. It is interesting that his Nobel Prize was in “recognition of the services he rendered to the advancement of Physics by his discovery of energy quanta”, showing the Committee made the same mistake as some textbooks still do nowadays. This is a very interesting point that can be made for teaching from a story-case: the ability to show how perspectives change with time, how the years and the circulation of knowledge can do the strange job of rearranging narratives of what was done in the past.

Eshach (2009) suggests using Rigden’s (2005) criteria for explaining the reasons a discovery would be called revolutionary: (i) it should contain a big idea that (ii) contradicts the scientific canon and that (iii) is typically rejected by the community until it is forced to do otherwise and (iv) survives scrutiny, becoming itself part of the physical canon. By these standards, could one call the quantum of action a revolutionary concept of Physics? For such a discussion, it would be necessary to show that some scientific concepts have a life of their own, a life that is mostly defined by other scientists’ works and less by their proposer. Einstein’s interpretation of the quantum of action is a bigger idea than Planck’s own interpretation, indeed. But the quantum of action is the seed for the quantization of light as much as it is for the quantization of angular momentum, being therefore the seed to quantum mechanics. Big ideas are not always born – in fact, most times, they are made.

Lastly, Eshach (2009) expresses that a good story-case should be able to address aspects of the nature of science. Much has been discussed on the term (Lederman, 1992; Lederman, 2007; Matthews, 2012), and here we would like to emphasize not exactly the nature of science, but a discussion of scientific rationality that is rather vigorous throughout Planck’s lecture. An inadvertent reader could be led to the conclusion that the frequent expressions of shock, awe, and other emotions would be an argument to the contrary – that science is not rational (or, at least, that Planck was not). Nonetheless, we would like to argue, following Laudan (1977; 1984), that the rationality of science resides precisely in its ability to adjust to the difficulties posed by nature.

Underdetermination demands a novel approach to scientific rationality, one that would take into account that science does not abide by a previous set of axiological and methodological rules. In this case, adjustments and value judgements are an integral part of the scientific practice, which can be understood as a problem-solving activity. It is true that Laudan’s perspective is communal, meaning it refers to a practice done within the scientific community; it is also true that we are proposing the analysis of Planck’s Lecture, especially the parts of his own, lonely investigation. That should not come as a contradiction, though. Planck’s adjustments are in fact special, since he is an example of thorough self-criticism. He starts off by indicating his first take on the black-body problem, through electromagnetism, and recognizing it was not as fruitful as he would have liked. “Nevertheless, the result meant no more than a preparatory step towards the initial onslaught on the particular problem which now *towered with all its fearsome height even steeper before me*". As soon as he learns of new empirical results, which generated an inconsistency between two different, but appropriate, distribution laws, he decides "to tackle the problem from the opposite side, that of thermodynamics, in which field *I felt, moreover, more confident*". Even after his successful interpolation, which rendered an analytical expression to spectral radiancy, Planck was not satisfied with its meaning. "I held the unshakable opinion that *the simpler* the presentation of a particular law of Nature, *the more general* it is". He then turns to statistical mechanics, even if just as a tool, to make sense of his expression, "until after some weeks of the most strenuous work of my life, *light came into darkness, and a new undreamed-of perspective opened up before me*".

We chose some quotations full of emotions to show how they do not necessarily mean an irrational practice, but the relations between man, theories, methods, and facts. This is his story, and although it is full of astonishments, it is also very justified, very rational, for it is always full of indications of the most prized values of science: consistency (with statistical mechanics), precision (with empirical results), scope (with other researches), and, most of all, fruitfulness (the quantum of action being pivotal in Einstein's light quanta and Bohr's atomic model). In the end, he acknowledges the inconsistencies the quantum of action brought to electromagnetism, something that would yield a new area of physics, showing why values must be taken not as rules, but as maxims, as Kuhn (1977) would point out.

Although we defend using Planck's Lecture, we must reiterate that this is a document that demands caution, not because it is inaccurate, but mainly because it is a rational reconstruction made twenty years after Planck's work. By then, Planck was a different scientific persona: he had already come to terms with statistical entropy and the quantization of energy. Throughout his life, Planck was mostly a conservative, inclined even to energism, and that prevented him (and the scientific community) from welcoming the latent interpretations the quantum of action kept. However, 1920 Planck is as much an interesting character as was his 1900 self. Moreover, his 1920 justification shows a development that was as much his as was of the physical sciences. The lecture is, therefore, a great subsidy to physics teaching, granted it takes into account that the narrator of the story was, in fact, a different man from the conservative he once was, one that understood he sowed the seeds of an anomaly and the revolution he inadvertently set in motion.

VII. Final considerations

On many occasions throughout his Lecture, Planck mentions the developments that took the scientific community nearer to the truth. What he meant by that can be better understood in his autobiography, in which he mentions the dichotomy of forging a truer scientific world view that, despite its great success, may never reach the realm of metaphysics. To him, that was the irrationality the exact sciences inevitably face; but despite that, science has made itself extremely reliable.

> *On the other hand, the very fact that science sets its own limits on the basis of scientific knowledge itself, appears well suited to strengthen everybody's confidence in the reliability of that knowledge, knowledge obtained on the basis of incontestable presupposition and with the help of rigorous experimental and theoretical methods* (PLANCK, 1949, p. 324).

His position seems fitting with the one here advocated. The rationality we understand as an important topic for the physics class is not one that derives from methods for discovering the metaphysical world, because that would be an impossible task for the context we suggest. But one must not dismiss the rationality of science because of ontological issues, whilst it can be approached in epistemological fashion. As Laudan (1984, p. 64) indicates

> *But beyond demanding that our cognitive goals must reflect our best beliefs about what is and what is not possible, that our methods must stand in an appropriate relation to our goals, and that our implicit and explicit values must be synchronized, there is little more that the theory of rationality can demand.*

Planck's practice, as expressed in his Nobel Lecture, shows these interrelations, when solving a problem and unexpectedly creating a new, fruitful anomaly that would yield a new area of physics. Quantum theory imposed great changes in the way physics and physicists see the universe. Firstly, one can consider the discontinuous processes of energy transfer. These quantum jumps cannot be explained by our classical laws. Secondly, quantum laws seem to control the probability, making some occurrences not predictable within certainty. Although that is curious and in opposition with deterministic laws, it is not exactly a novelty. In fact, as has been said, entropy has probabilistic characteristics. Momentum and angular momentum, just as the conservation of energy, are some examples of entities that do not lose meaning even in discontinuous processes (Bohm, 1989).

Teachers and students alike may wonder how rationality can be ascribed to Planck's quantum of action, if he – and the scientific community – did not explicitly pay careful attention to what it could mean before Einstein's light quanta. The meanings of the physical concepts are not always self-evident and usually demand time to be presumed as revolutionary. The concept of field, for instance, proposed by Faraday in the early 19th century, is nowadays understood to be a challenge to mechanicism, since it attributes to matter the ability to change its surroundings, whereas inert matter is the central aspect of mechanicism. However, the proposal of the concept did not prevent Maxwell from developing electromagnetic theory from a mechanicistic manner and ether was a part of the scientific canon until the early 20th century (Harmann, 1982). One could also suggest that Planck's resistance to the light quanta was also irrational; resistance, nevertheless, cannot be seen as a sign of pure irrationality, as it is often the result of major trust in the previous successful canon and its consistency with so many other phenomena. In 1920, finalizing his lecture, he reflected upon the inconsistencies between the idea of light quanta and electromagnetism, showing he had still been dwelling with the consequences of his quantum of action. But in 1949[8], it seems he had accepted it.

> *To cite a concrete example, such a fact is the velocity of light in empty space, and another is the part played by the elementary quantum of action in the regular occurrence of all atomic processes. These two facts, and many more, could not be incorporated in the classical world picture, and consequently, the classical world picture had to yield its place to a new world picture [...] In fact, the laws of classical mechanics continue to hold satisfactorily for all the processes in which the velocity of light may be considered to be infinitely great, and the quantum of action to be infinitely small. In this way we are able to link up in a general manner mechanics with electrodynamics, to substitute energy for mass, and moreover, to reduce the building blocks of the universe from the ninety-two different atom types of the classical world picture to two – electrons and protons* (PLANCK, 1949, p. 323).

Finally, we must mention the role played by statistical mechanics in the matter. Although nowadays a very important discipline in any undergraduate physics course, we reiterate that it was not a consensus at the time. As has been mentioned, Planck resisted resorting to it for as long as he could; and even as he did, he used it more as a tool than as an adopter of its interpretation of entropy, in what he called "an act of desperation" (Kragh, 2000). Once again, in hindsight, one could ask whether this, too, is not a sign of irrationality; afterall, how could someone use a theory without being in accordance with its ultimate meaning? There are plenty of reasons for that, but we would like to highlight two, in particular. First, because creativity is a fundamental resource for scientists; creativity and desperation are human, but they can only be celebrated as scientific if they show cognitive power. In Planck's words

> *To be sure, when the pioneer in science sends forth the groping feelers of his thoughts, he must have a vivid intuitive imagination, for new ideas are not generated by deduction, but by an artistically creative imagination. Nevertheless, the worth of a new idea is invariably determined, not by the degree of its intuitiveness – which, incidentally, is to a major extent a matter of experience and habit – but by the scope and accuracy of the individual laws to the discovery of which it eventually leads* (PLANCK, 1949, p. 325).

The second reason takes us back to Laudan, who recognizes that scientists can agree methodologically, even if axiologically they do not. Demanding that scientists should agree in everything but facts is actually demanding that science should be done by a set of rules, that scientists be dogmatized to understand the world the same way. It means science would ultimately lose its creative power, that it would be more of a religion. It may be strange to assume a former energitist would make use of a law from an atomistic theory to solve a problem, but methodologies are underdetermined by values as much as theories are underdetermined by

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[8] Max Planck died in 1947, but his autobiography was published in 1949.

evidence. In any case, as has been mentioned, Planck eventually agreed with statistical entropy, probably because of the role it also played on Einstein's work (but surely many years after 1905). His transition is also the historical account of how science accepted atomism and statistical entropy. As Kragh (2000) says, the history of the quantum of action is less of a history of the shortcomings of electromagnetism, and more of a history of the understanding of entropy.

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