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BOOK OF ABSTRACTS

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Helmut Pulte (Ruhr Universität Bochum-RUB, Germany

'Action' in Action. On the Rise, Development and 'Metaphysical Unloading' of a Controversial Concept of Rational Mechanics.

'Action' was used in rational mechanics around 1700 in ambiguous, often misleading ways. It became an important and controversial concept in connection with the 'principle of least action' of Euler and Maupertuis, which was also attributed (wrongly, in my opinion) to Leibniz.

The lecture examines different uses and meanings of 'action' in the 18th century and goes on to trace its changing meaning until the end of the 19th century. Using a catchword, one might speak of a 'metaphysical unloading' of the concept, the causes of which have perhaps not been sufficiently recognised and analysed until today.

Robert DiSalle (University of Western Ontario, Canada)

Space-time and experience

Einstein's special theory of relativity (1905) took on a remarkable new aspect when Minkowski (1908) formulated it as a theory of space-time. While this was unquestionably an essential innovation for the future development of special relativity, and in particular for the transition from special to general relativity (1916), it has also been regarded as a particularly abstract formulation of the theory, remote from the empirical motivations and applications that connected the theory to the world of experience. This was reflected in Einstein's early response to Minkowski's idea, and a recurring theme in interpretations of special relativity up to our own time. By revisiting Minkowski's conception of space-time in its original context, and comparison of it with other abstract reformulations of classical physical theories, we can see how closely Minkowski's formulation remained to its empirical foundation, and perhaps appreciate some important insights into the general problem of the empirical interpretation of mathematical theories.

Olivier Darrigol (CNRS, UMR SPHere, France)

Relativity principles before relativity theory

It is usually believed that the relativity principle had long been a basic principle of mechanics when around 1900 Poincaré and Einstein used it to frame a new electrodynamics of moving bodies and thus reached what is now called relativity theory. This is not exactly the case, because most physicists before Poincaré did not truly regard the Galilean relativity of mechanical phenomena as a principle: it was either an empirical law (for Galileo) or a theorem (for Newton and most of his followers). Yet it is true that in the later seventeenth century, Christiaan Huygens inaugurated a long tradition of deriving physical laws through constructive relativity principles. Plural is needed here, because since Newton there were two kinds of relativity involved: a Galilean relativity introduced by Galileo, and an accelerative relativity principle introduced by Newton and akin to Einstein's later equivalence principle.

I will show how relativity principles prospered in Euler's, d'Alembert's, and Laplace's hands, and then became the basis for a popular derivation of Newton's acceleration law in French physics textbooks of the nineteenth century. It turns out that Poincaré and Einstein were both aware of this tradition, that Poincaré borrowed from it the name Principe du mouvement relatif (which he later altered to Principe de relativité). In recognizing the architectonic role of relativity principles, both of them were remote descendants of Huygens.

Travis McKenna (University of Pittsburgh)

What is an `instance' of Newton’s second law?

It is common within the contemporary metaphysical literature regarding laws of nature for philosophers to grapple with questions about the relationship between scientific laws and the systems to which they applyby considering the way that schemata such as ‘All F s are Gs’ relate to ‘instances’ like ‘a is F andais G.’ For example, in the introduction to a recent paper dedicated almost entirely to discussing such schemata,Emery [2019, 1535] writes:

“What is the relation between a law and its instances? What is the relation,for instance, between Newton’s second law (f = ma) and those sequences of events in which applying a force f to a mass m results in the mass accelerating at a rate of a = f/m?”

In fact, Newton’s second law as originally articulated is true more or less solely of point masses with no extension. When we talk in generic terms of “applying force f to a mass m” as a clear cut ‘instance’ of Newton’s second law we are brushing aside important mattersof detail. ExtendingNewton’s laws to rigid and deformable bodies was an important challenge in the history of classical mechanics. One of the primary obstacle to this extension was the question of how to reconcile the fact that many forces, such as friction, act on the surfacesof bodies with the fact that point masses explicitlylack such surfaces.

Ultimately this challenge was overcome, in no small part thanks to Leonhard Euler. The key point, however, is that this required extra physical principles not in any sense ‘contained’ in Newton’s laws. These extra principles allow us to understand generic situations in which we “apply a force f to a mass m” loosely as ‘instances’ of Newton’s second law. In doing so, however, we are recognising that when combined with these extra principles, Newton’s second law supplies centraldynamic equations that our ‘mass m’ will obey at some scale.

This paper will examine the details of how this extension was accomplished and argue for the philosophical importance of those details.The main upshothere is that understanding the conceptual and physical innovations that allowed us to extend Newton’s second law to treat rigid and deformable bodies requires attention to more subtle details than ‘All F s are Gs’ schemata allow. AlthoughNewton’s second law applies to a great variety of systems that are not modelled as point masses, these systems are not ‘instances’ of the law in the narrow, logicalsense around whichthe contemporary metaphysical debate is framed.The morale: understanding the relationship between Newton’s second law and the systems to which it applies requiresthat we do more than reflect on the relationship between ‘All Fs are Gs’ and its instances.

Sandro Caparrini (University of Torino, Italy)

Remarks on J. L. Lagrange's Méchanique analitique

There is no shortage of books and papers citing J. L. Lagrange's Méchanique analitique (1788). Yet, though armed with a thorough knowledge of secondary literature, the modern reader is likely to struggle with every page. Like many classics, the Méchanique analitique is a labyrinthine microcosm.

The Méchanique analitique has a complex history of composition. Between 1756 and 1760 Lagrange wrote a few works that can be considered as intermediate steps in the evolution of the final treatise. In addition, between 1760 and 1788 he published several papers on mechanics. Most of them are extensive treatments of fundamental topics like fluid dynamics, rigid bodies, perturbation theory and vibrating systems. They have the same structure as the Méchanique: some methodological remarks, a historical introduction and a section on general principles, followed by pages upon pages of differential algebra. These works were carried forth verbatim into the treatise.

About a quarter of a century after the publication of the Méchanique, Lagrange produced a second edition (1811-15), taking into account the recent discoveries of a younger generation of mathematicians. The many revisions and additions nearly doubled the size of the book.

Clearly, this is a text of many layers. While some of these have been explored, others remain to be identified. Prospective explorers should be prepared to go through the text line by line and equation by equation. Their patience will be rewarded.

Among the unexpected discoveries is the fact that Lagrange had a working knowledge of the geometrical composition of directed line segments long before Grassmann and Hamilton, in the 1840s, explained the general principles of vector calculus. In the section of the Méchanique analitique on the kinematics of rigid bodies he worked out analytically the elements for a geometrical theory of systems of three radii vectores. He obtained formulae for what we now call the dot product, the cross product and the mixed product of vectors. It must be noted that these results first appeared in papers published almost twenty years earlier.

It is also worth noting that Lagrange had formulated the (so-called) Cauchy-Schwarz inequality in the 1770s. This was a by-product of his work on geometrical vectors. In the 1820s Cauchy generalized this formula to n variables.

Concerning the principles of mechanics, Lagrange proved in general the conservation of mechanical energy the principle of momentum, the principle of moment of momentum, the principle of virtual work and the principle of least action. (Before Lagrange it was generally acknowledged that these were theorems rather than axioms, but these theorems had been demonstrated only in the context of specific theories, starting from several different principles and using a variety of mathematical methods.) He also explored the connection between spatial invariance and conservation laws. Moreover, he demonstrated the important lemmas which state that the resultant and the resultant torque of the internal forces of a system of mass points are both equal to zero.

The second edition, retitled Mécanique analytique, contains much that was then new. Chief among the new insights is the recognition of the geometrical representation of angular velocities (vectors) and momenta (plane surfaces). Lagrange also gave, probably for the first time, a proof of the formal invariance of a given differential expression under an orthogonal transformation of coordinates.

Tzuchien Tho (University of Bristol)

Lagrange's "true metaphysics" and the foundations of analytical mechanics

Due to W.R. Hamilton, analytical mechanics is often historically interpreted in a way that highlights its role in the evolution of the least action Principle (or, more accurately stationary action) due to the methods of optimisation. This characterisation is indeed true for the historical developments of analytical mechanics before Lagrange (Maupertuis, Euler, et. al.) and after Lagrange (Hamilton, et. al.) but not true for the mature Lagrange who was very keen to distance his work from teleology. Though there is not scholarly consensus, J.L. Fraser (1983) has made a convincing claim that this rejection of his teleologically inclined elder colleagues was at the basis of his claim of having developed "the true metaphysics of their [mechanics] principles" [la vraye metaphisique de leurs principes]. Yet since Lagrange was typically laconic about "metaphysics", we do not have a clear expression of what this might concretely be.

This paper focuses on the method of virtual velocities which Lagrange used to replace the least action methods of his predecessors and examine the metaphysics that it could imply. In particular, focus is placed on the causal theory implied by Lagrange's methods and shows how this challenges not only teleological understandings but also the Newtonian mechanical one.

The paper begins by contextualising Lagrange's method of virtual velocities within the periods of his writing focusing on his 1764 work on the Moon's libration. We then move to contextualising these methods within the foundations of analytical mechanics, identifying the theoretical development of virtual velocity and virtual work methods that had come before. By doing this, we develop a sketch of the metaphysical assumptions of the method and use this to assess the deep differences between Lagrange's innovations and the standard understanding of the least (stationary) action principle. We finally make a case for a Lagrangian theory of causation that is local but not mechanical in the usual sense, and structural but not teleological as his near predecessors would have it. The reexamination of the foundations of analytical mechanics will have significant concepts to offer contemporary philosophy of physics.

María de Paz (Universidad de Sevilla)

Feigning Hypotheses: Non Newtonian Approaches to Gravitation

It is well known that one of the obscurities in Newton's natural philosophy is the way gravitation is transmitted. Although Newton claimed to not feign hypotheses to explain this issue, this did not prevent some of his successors from doing it. The aim of this talk is to explore two particular approaches to gravitation in the 18th century: that of the famous mathematician Leonhard Euler and that of a forgotten figure, Georges Louis Le Sage. Both natural philosophers were well aware of Newton's work in this topic and both were against an action-at-a distance approach. However, their solutions, although they could be regarded as action-in-contact approaches, are quite different in several respects. The metaphysical foundations in which they are based, the physical consequences, the mathematical approach and the methodology they developed are some of the topics we will present. By discussing approaches that today are no longer considered, we aim to show that the development of classical mechanics in the Eighteenth Century is far from that of a period of 'normal' science. Several authors were in fact discussing the foundations of science in metaphysical, physical, mathematical and methodological respects, although they all took into account Newton's work. By providing alternative approaches to natural philosophy they all contributed to develop concepts in order to handle problems and understand the way nature works.

Daniel Nieto (Universidad de Sevilla)

On methodology: Émilie du Châtelet between gravitation and vis viva.

Although the eighteenth century has been considered a period in which Newtonianism reigned -something that is not entirely accurate-, the question of scientific method is still pertinent. If we analyse the scientific situation at the time, we will find that the Cartesians used too many hypotheses and the Newtonians none or few. However, not every natural philosopher fits in these two labels. Among the ones who do not, we find Émilie du Châtelet, a philosopher and scientist who, in her Institutions de Physique, shows us a new method of scientific practice.

Châtelet uses a combined methodology. In her method, we find the first principles of knowledge introduced by Leibniz, i.e. the principle of sufficient reason and the principle of non-contradiction, the principle of continuity and the principle of the identity of indiscernibles. Using the first principles as a fundamental part of his methodology, Châtelet concludes that hypotheses are useful, as they can guide our thinking towards the truth. In this sense, she rejects both the idea that hypotheses only create fictions, as Newtonians would say, and the idea that hypotheses do not lead to any safe path.

In my talk, I want to show the way by which Châtelet arrives to the use of hypothesis through the first principles. I will also show how these principles are needed to create safe hypothesis which guide our thinking to the truth. Furthermore, the principles are also needed to avoid obstacles when we want to create a good hypothesis, so they help us to distinguish a good hypothesis from a bad one.

In order to frame my ideas, I will show how these hypotheses work when she had to choose between the Newtonian theory of gravitation or Leibnizian theory of vis viva. Through the analysis of Châtelet's work regarding hypotheses and motion, I will show that hypotheses are an integral part of the 'making of' science, and not a mere residual device that remains outside the theoretical prediction of phenomena.

Michael Veldman (Duke University)

Kuhn's "Newtonian Paradigm"and the Impenetrability of 18th Century Physics
Michael Veldman

Kuhn argued that scientific inquiry becomes a fruitful program of "normal science" when collective acceptancecauses a "paradigm"to coalesce out of a field of controversy. In Kuhn's work, Newtonian physics is somethinglike the paradigmatic paradigm, used to map out a canonical structurefor the evolution of science. ContraKuhn, in 18th century physics, there was no such thingas the 'Newtonian paradigm'. One reason is that the concept of force – central to Newtonianphysics – remainedcontroversial throughout the 18th century.Nonetheless, in the Introduction to Structure, Kuhn claimed that:

"Effective research scarcely begins before a scientific communitythinks it has acquired firm answers to questions like the following: What are the fundamental entities of which the universe is composed? How do these interact with each other and with the senses? What questions maylegitimately be asked about such entities...?"

The idea of a Newtonianparadigm has proven to be a compellinglens for philosophers interested in the history of physics. But if we take off those glasses for a moment, we will see that none of these questions had consensusanswers in the 18th century,and that Euler, Maupertuis, d'Alembertand others were working out their own foundations for what would only later become 'classical mechanics'.
Strikingly, Kuhn claimed in Structure that 18th century mechanics can be reduced to mere "applications" and "reformulations" of the Principia. A decade later, Kuhn distinguished two senses of 'paradigm': a 'local' concept, the shared example of successful practice, and a 'global' concept, also called the 'disciplinary matrix',which encompasses paradigms-as-examples, as well as symbolic generalizations (roughly,equations expressing laws) and "models".

We can use each of these proposed understandings of 'paradigm' to evaluate Kuhn's claims about 18th century mechanics. First, Newton's mathematical methods were quickly supplanted, so he did not provide paradigms-as-examples. Second, this lack of exemplarsstimulated a search for new fundamental principles, so Newton did not provide a complete set of symbolic generalizations.

Finally, I turn to the aspect of "models" which Kuhn says amounts to an ontology, a collection of concepts expressing a view of "what the world is like." The central conceptswere force and mass. Pace Kuhn, the question of the natureof forces was up for grabs for most of the 18th century. One aspectof the surrounding controversy traces back to Newton's own deployment of the concept of 'force of inertia', which puzzlinglycategorizes the cause of the persistence of a body in its state of motion as a 'force,'alongside pushes and pulls.Euler, for example,criticized this and other perceivedissues in Newton's conception of matter, and attempted to resolve them by means of a fundamental theory of the nature of forces and bodies that relied crucially on corporeal impenetrability. The upshot is that on none of the candidate understandings of paradigm can 18th century mechanics be simply subsumed under a Newtonianparadigm, which should prompt a reconsideration of the accuracyand usefulness of Kuhn's conceptual framework. In my view, the upshot is not simply that Kuhn was wrong. While the notion of a 'paradigm' is unhelpful here, it providesus a way into the 18th centuryby throwing into relief the conceptual disputes over forces, body, and physical principles that persisted throughout its whole duration.This continuing debate over force, body, and bodily action formed the background for much-studied late-century philosophy, such as Kant's attemptto place mechanics on 'metaphysical foundations.' And arguably, it was exactly this conceptual ferment that yieldedcomprehensive alternative approaches to mechanical theory, delimiting the concepts and physical applications so as to make it possible, at the end of the 18th century, to first define a "Newtonian paradigm" for classical mechanics.

Adán Sus (Universidad de Valladolid)

Dynamical symmetries, physical possibilities and the emergence of Galilean/Newtonian spacetime

The notions of symmetry and physical equivalence are with no doubt related. Nonetheless, careful attention shows that the relation is not without problems. The temptation of identifying, in spacetime theories, states of affairs that are related by a symmetry transformation should be contained, first, by noting that there is not a unique notion of physical symmetry and, second, by realizing that in some situations states connected by a symmetrytransformation are countedas distinct physicalpossibilities.

In this talk I will address the problem of how to provide an account of the relation between symmetry and physicalequivalence, which goes through the question of how to characterize the notion of physical symmetry. To this effect, I will revise different strategies for providing such a characterization and discuss whether they fulfill reasonable desiderata associated to the notion of physical symmetry. Then, I will confront them to the idea of introducing the notion of physically relevant symmetrical backgrounds in order to define physical symmetries. I will argue that the physical relevance of such structures is gained by their connection to the dynamics of isolated subsystems and the possibility of defining proper conserved quantities.

This discussion has a particular impact on the old question about what the right spacetime setting for Newtonian physics is, on the one hand, and about the allegedly different status of inertial structures in Newtonian and relativistic physics. Building on work by Saunders, Knox and Wallace, I will explore the possibility of understanding the spatio-temporal structures in Newtonian physics as emerging from certain features of the dynamics of subsystems.

Referring to symmetrical backgrounds in the interpretation of spacetime theories can be reminiscent of a substantivalist or geometric approach. Nonetheless, I will argue that this does not have to be the case by discussing the prospects of accommodating this characterization of physical symmetries to a dynamical approach to spacetime theories that also integrates elements belonging to the neo-Kantian trascendental tradition.

Javier Anta (Universidad de Barcelona / LOGOS)

A Historical-Cognitive Approach toward the Concept of Phase Space

The concept of phase space is one of the most powerful epistemic tools in classical mechanics (CM) since it allows us to describe mechanical behaviors via the geometry of the space of possible position and momentum values. In this talk I argue from a historical-cognitive perspective that the intellectual genesis of the CM concept of 'phase space' constituted not a homogeneous-linear episode but an entangled historical process grounded on the development of visual-representation practices in classical mechanics.

Although the historiography of physics often cites Liouville's 1838 famous paper as the origin of this concept (e.g., Nolte 2018), the truth is that it was precisely Jacobi in his 1843-1844 “Vorlesungen über Dynamik” lectures (Clebsch 1866) who first applied Liouville's results to Hamiltonian mechanics by presupposing the conservation of a certain property defined over collections of solutions of differential equations. Although Jacobi stated that Liouville's theorem implied a certain conservation, he made no explicit reference to any ‘space’, ‘trajectory’ or ‘conserved volume’. This Jacobian analytical treatment of mechanical systems relied on symbolic reasoning about classes of solutions of Hamiltonians, which precluded conceiving or imagining the existence of an abstract spatial framework. Jacobi's results in the 1840s were pivotal in Boltzmann's first paper in 1871, but it was precisely in his second paper of 1871 wherein he used graphical resources (the now called 'Lissajous figures') to describe the dynamics of mechanical systems in a two-dimensional space. In Boltzmann’s celebrated 1872 paper he originally referred to the point representing the mechanical state of a molecular system as a ‘phase point’.

I argue that the possibility of cognitively assimilating the notion of phase space depends on having, on the one hand, an intellectual context prone to abstract spaces (as fostered by Klein's Erlangen program in 1872 and by the vast proliferation of non-Euclidean geometries since the 1850s); and on the other hand, on having certain key visual representation practices in mathematical physics. As far as visualization practices are concerned, it is worth mentioning Poincaré's development in 1889-1890 of a graphical representation technique where a plane embedded in the space of possible position-momentum values is used to assess the dynamic behavior of possible solutions. Note that the use of this 'first-return map' presupposes a proficient (though still implicit) cognitive exploitation of the concept of phase space by Poincaré. Other visualization techniques correspond to those displayed by Gibbs in his classical Elementary Principles in Statistical Mechanics in 1902, wherein he employed the Grassmannian term 'extension' (but not ‘space’ or ‘volume’) to refer to the property which is conserved via Liouville's theorem. These Gibbsian tools allowed to conceive (and also visualize) the evolution of a set of Hamiltonian trajectories of a mechanical system as an incompressible liquid flowing in the position-momentum space.

The central idea is that the development of mathematical visualization practices during 1872-1902 such as Boltzmann's Lissajous figures, the Poincaré Section or the Gibbs diagrams enabled to cognitively generate vague image schemas (e.g., Lakoff & Nuñez 2000), then accurately conceive and finally technically define a phase space like the one implicitly formulated by Jacobi as soon as in 1843-1844. Finally, in this last period of conceptual consolidation (1902-1918) intervened authors such as Paul and Tatiana Ehrenfest, who defined rigorously for the first time the CM-notion of 'G-space' or 'Phasenraum' in 1911; or Rosenthal and Plancheral, who for the first time explicitly employed the English expression 'phase space' in 1913. The rest is history.

Laurent Goffart (CNRS)

Are kinematic parameters encoded within the brain activity while a gaze movement is being achieved toward a visual target?

Two types of eye movement are made while one tracks a target moving in the visual field. The first type is an abrupt step-like movement (called saccade) that rapidly rotates the eyes toward the target location and brings its image within the central visual field. The second type is a slower movement (called pursuit) whose velocity approximates that of the target. From the retinal excitation to the contraction of extraocular muscle fibers, distinct and parallel visuomotor channels are involved in generating these two movements. Most of the time, the eyes do not rotate as fast as the target; the target image slips on the retina and catch-up saccades punctuate the oculomotor tracking. The performance during which gaze moves continuously and as fast as the target is not spontaneous but requires training.

During the last six decades, numerous studies investigated the neuronal processes driving the changes in the orientation of the eyes in response to a moving target. High-resolution recording techniques yielded time series of numerical values from which magnitudes such as eye movement amplitude, duration and velocity were calculated. Some models proposed the existence in the brain of processes that would reduce the difference between internal signals encoding gaze and target directions (for guiding the saccade) and the difference between signals encoding the eye rotation speed and the target speed (for accelerating the slow pursuit component). Lastly, during the pursuit maintenance, a process would sustain the eye velocity while the target image is more or less stabilized in the central visual field. This cybernetic formalism guided electrophysiologists who studied the correlations between the activity of neurons and various kinematic parameters of the eyes and target (position, distance, amplitude, velocity and even acceleration). A one-to-one correspondence was often assumed between notions belonging to the physical world and the inner functioning of the brain.

However, contrary to the receptacle (space) within which the object is moving, the brain medium is not empty, neutral, homogeneous, isotropic or uniform. The neurophysiology unravels clusters of various kinds of cells between which multiple channels transmit the retinal signals with unequal conduction speeds. Before converging onto the motor nerves and exciting the appropriate muscle fibers, the visuomotor transmission consists of flows of activity that are distributed across several neuronal regions. Within these neuronal networks, the neural image of a small target spot does not look compact and rigid but dynamic and expanded, spatially and temporally. Yet, despite this tremendous complexity, animals exhibit the ability to capture an object, at the location where it is and at the time when it is there.

During my communication, I shall report examples illustrating attempts to “cerebralize” kinematic parameters and explain their limitations. Instead of embedding within the cerebral medium, notions that are classically used to describe the motion of a rigid body in the external world, an alternative option remains possible. A saccade can be viewed as the outcome of a process that restores an equilibrium between visuomotor channels exerting mutually opposing tendencies whereas the slow eye movement as a sustained imbalance.

Marij van Strien (Bergische Universität Wuppertal)

Overcoming Newton in the Twentieth Century: The Development and Rhetorical Uses of the Concept of Classical Mechanics

During the 1920s and 1930s, many authors, physicists as well as non-physicists, argued that the framework of classical physics had turned out to be too narrow and restrictive. They argued that central elements of classical physics were refuted in the recent developments in physics, in particular relativity theory and quantum physics. This was frequently seen as bringing an end to the strict mechanism and determinism of the physics of the nineteenth century, and even as a liberation from an oppressive worldview.

As Richard Staley (2005) has shown, the terms 'classical mechanics' and 'classical physics' itself date from the early twentieth century. This means that the physics of the eighteenth and nineteenth century only became classical in retrospect, when contrasted with the 'modern' physics of the early twentieth century;Staley therefore speaks of the 'co-creation' of classical and modern physics.

Whereas Staley has traced the earliest uses of the terms 'classical mechanics' and 'classical physics', in particular in the context of Einstein's special relativity theory in 1905 and at the Solvay conference on quantum physics in 1911, Gooday and Mitchell (2013) have argued that it took somewhat longer before the term 'classical physics' really became established as a general term for non-relativistic, non-quantum physics, until the 1920s–1930s.

This means that the period in which the term 'classical physics' became established was at the same time a period in which it was widely argued that classical physics had now (finally) been overcome. This raises the question in how far the image of classical physics which was formed in this period is accurate. Both the terms 'classical mechanics' and 'classical physics' may from the beginning have referred to a distorted image of the mechanics and physics of the eighteenth and nineteenth century: in particular, the physics of this period may have seemed more mechanistic, deterministic and uniformin retrospect than it actually was.

This talk aims to show how the image of classical mechanics developed during the 1920s and 1930s. I will look in particular at the rhetorical use of the terms 'classical' and 'Newtonian'. The term 'classical mechanics' accomplishes a few things: it signifies the enduring relevance of past science, while at the same time signifying that this is indeed past science, and that a revolution has since taken place (Clarke 2014). Furthermore, it suggests a unified framework, and thereby tends to create a homogenized, unified image of the past: I suggest that it is partly through this term that we conceive of the eighteenth and nineteenth century as a stable period in mechanics, in which Newton's laws provided a fixed framework, rather than as an active and developing field of research in which there were a diversity of approaches.

Amaia Corral-Villate (Universidad del País Vasco)

On particle disappearance conclusions in classical mechanical configurations

The context of this talk is provided by the particle disappearance results concluded in recent discussions on three infinite particle configurations in strict classical mechanical settings; namely, those by Alper & Bridger (2002), Pérez Laraudogoitia (1998) and Shackel (2018). In fact, even if these configurations provide a good context in which to clarify the basis operating in each setting and to contrast the corresponding evolutions, the analysis that follows may also be applied to the infinity machine introduced by Black (1951) in which only one particle is in place, despite the fact that the author did not claimany disappearance resultin this case.

The objective of the talk is therefore to clarify the basis on which particle disappearance results may justifiably be claimed, and consequently determinewhether those particular configurations fulfil the required characteristics. The key to identify these conditions and the consequently justified disappearance result is shown by Corral-Villate (2020) to be given by the application of the fundamental classical mechanical principle of mass conservation in the formulation by Earman (1986), and the classical mechanical requirement that world lines be continuous.

The result of this analysis leads to conclude that given that both the discussions by Alper & Bridger (2002) and Shackel (2018) imply the violation of the fundamental principle of mass conservation, in neither of these configurations is the particle disappearance claim justified. Contrarily, in the systems by Pérez Laraudogoitia (1998) and Black (1951) the fundamental principle of mass conservation is fulfilled, and thus the particle extinction conclusion is in these cases justifiable. Furthermore, the world line continuity condition seems in fact to require such disappearance.

Aldo Filomeno (Instituto de Filosofía, Universidad Católica de Valparaíso)

Non-causal explanation of lawful behavior from mathematical constraints

In this talk I would like to assess whetherand how mathematical facts can constrain the space of what is physically (or 'naturally') possible. The idea, well-known but puzzling, is that logical or mathematical necessity constrains the physical world by constraining the space of physical possi- bilities.

It is usually considered that physical possibilities are a proper subset of logical space, but the source of physical modality is usually thought to be different, sui generis. Still, empiricists have considered such notion of physical modality elusive and mysterious (and it could be said that today it remains a mystery). Now, in the history of physics (notablyin the history of mechanicssince the XVI century to this day) we find (disputed) attemptsto maintain that physical necessityjust is mathematical necessity, which (arguably) would solve the mystery. Along this line, today inthe philosophy of science, the current primitivist account of laws seems to expect some sort of "mathematical inevitability" of the final laws of a theory of everything, especially when stated by physicists (cf. Maudlin 2ooy).

How to make sense of such a thesis, viz. that physical necessity just is mathematical necessity? This task has turned out hard to spell out, and has even been ignored in the literature in philosophy of science and metaphysics. However, in this talk I argue that there are two places in which we might find support for this thesis on the source of physical necessity, i.e. of the laws of nature. On the one hand, in the disputed attempts in the history of mechanics, from Euler or D'Alembert, to current improvements, such as those studied and proposed in (Darrigol, 2o14, 2ooy). On the other hand, in the recent literature of non-causal explanations of physical phenomena.

Regarding the former,in the setting of classicalmechanics, we have available a large varietyof attempts to derive the mechanical laws of motion from the fewest and allegedly most plausible principles (which e.g. often are the principleof sufficient reason or the principle of inertia). Althoughthey have been disputed for good reasons, all these foundational attempts already hint at the thesis of the necessity of classical mechanics. The recent attempt by Darrigol (2o14, 2ooy), if successful, would constitute a further milestone in this project.

The latter, the alleged distinctively non-causal explanations of physical phenomena, might inde- pendently contributeto ground the plausibility of such project.I cite a variety of examples found in this literature: the double pendulum, Ko¨ninsberg bridges, Lorentz invariance, and renormaliza- tion group techniques (Batterman, 2oo1, 2o18; Morrison, 2o1y; Lange, 2o1y; ?) If the explanation of such physicalphenomena is distinctively mathematical, this would highlight the modal influenceof mathematics in the material world. No possible world can violate mathematical truths. As it is sometimes said, even God could not make 2+2 be differentfrom 4. If there really are distinctively mathematical explanations of physical phenomena, in which the underlying causal network is ir- relevant and need not exist, as Marc Lange argues (Lange, 2o1y), this could help us to understand how certain physical events cannot occur, thus constraining what is physically possible, and thus delimiting a state space.

This is of course insufficient to resolve the issue. But it is one step to supporta currently neglectedaccount of laws of nature: an account in which laws get their modal strength from mathematical modality. Thus, in this talk I consider that, between pure contingency and mathematical necessity, there is nothing – there is no primitive physical modality.

In addition to this first step, one could pursue different paths; this way of understanding physical modality can be complemented in different ways. One way is to complement it with the statistical explanations (a la 2nd law of thermodynamics) of emergent patterns(see e.g. (Filomeno, 2o19, 2o21) and references therein), which are also non-causal. Then, in the resulting view, the only law-like as- sumptions are the general constraints that are usually imposed in the study of physical systems; for instance, the boundary conditions or the kinematical conditions. Cf. (Adlam 2o22; Chen and Gold- stein 2o22; Romero). How to ontologically understand such constraints was one of the challenges to solve. Perhaps the aforementioned non-causal explanations of physical patterns by mathematical constraints might fill this role.

Joshua Ben Itamar

Berkely's criterion of a mechanist view and his attack on the mechanist view

The article will review the common criteria of a mechanist view in the 17th century and the beginning of the 18 th century.

An attempt will be made to present the criterion that Berkeley adopted, as well as his uniqe attack on the mechanist view
According to the criterion of a mechanist view as impliedfrom berkeley's writings, a mechanist view is one that explains the natural phenomena, assuming material causes only.

Berkeley claims that the material world as perceived by us is a collection of sensual ideas. These ideas exist in our mind , and cannot exist out of our mind. Berkeley also claims that the ideas are passive. An idea is merely a sign for another idea, and not the cause of the formation of another idea. Since Berkeley attacks the mechanist view, he is defined as an anti mechanist.

Berkeley claims that the term "Force", which many physicists think that it represents a cause of phenomena as velocity changes, dose not represent a real quality. Berkeley is defined as an empiricist. But in order to preserve scientific achievements and the ability to connect between phenomena and to predict, he claims that we have to use the term "Force" although it dose not represent any real idea or a real quality. The term "Force" represents in his opinion a mathematical fiction that enables us to connect between phenomena and to predict. This view of Berkeley is called " Scientific Instrumentalism".

Science does not deal with material causes according to Berkeley's view. The universal mind (God) is the real cause of natural phenomena, "planting" the regular ideas referred to as '' nature''. Still minds and God should be dealt with in metaphysics. Therefore Berkeley's attack on the mechanist view is the result of his view that only in metaphysics we deal with real causes, and these causes are spiritual.

In Berkeley's opinion, there is no difference between the picture of the world which follows from his philosophy, and the common sense picture , the one that the ''man in the street'' holds. Moreover, Berkeley claims that no scientific achievement suffers as a result of his philosophy.
Berkeley is unique when compared to most of the mechanists, since he claims that a scientificexplanation does not support material causes, and the term ''Force'' in his opinion represents a mathematical fiction.

Although Berkeley executes a full mathematization of the term "Force", he does not adopt mathematization as the criterion of mechanist views.

As far as his scientific instrumentalism is concerned, Berkeley preceded Ernst Mach about 200 years. The instrumentalist view became more acceptable when Quantum theory was developed. This view may be seen as a step towards the full mathematization of physics, such as was done in Quantum mechanics.

Shaul Katzir (The Cohn Institute for history and philosophy of Science and ideas, Tel Aviv University)

The use of the Principle of Vis Viva before Helmholtz

In his recent comprehensive and thorough Helmholtz and the Conservation of Energy, Kenneth Caneva has claimed that the principle of (conservation of) vis viva had been evoked only to a system returning to an earlier state, or to one without Newtonian forces. In this talk I intend to argue that this was not the case, and that the principle had been used also to analyse systems of material bodies under central forces that do not return to their initial state. In other works, it was used also when the vis viva proper (mv2) was not conserved but the sum was constant (as defined for example by Lagrange). I shall rely on texts in the tradition of the French Analytical Mechanics form Lagrange to Coriolis. Among others, the discussion would show that contra to Caneva's claim, mechanics was connected to fields of "physics" (e.g. in Ampère's work on electrodynamics), and that the principle of vis viva (with or without the word "conservation") and the analytical tradition contributed to the emergence of the principle of conservation of energy. In a more abstract level it would show that a common term, like energy, is not always needed for employing its conservation, as the lack of a common word for the vis viva and our potential (quantité d'action of Navier) did not prevent the employment of the law of vis viva.

Francesco Nappo (Politecnico di Milano)

How Maxwell Discovered the Maxwell Equations

The popular story – the one still taught in many university-level physics courses - about how J. C. Maxwell arrived at the Maxwell equations goes roughly as follows. Having collected the main experimental laws of the electromagnetic science of his time (namely Gauss' law for electricity, Gauss' law for magnetism, Faraday's law for electrical induction, and Ampère's law for electromagnetism) Maxwell noticed that they jointly violated the principle of conservation of charge. Because of this, and possibly motivated by considerations of symmetry, he integrated Ampère's law with an additional term: the 'displacement current', understood as a rate of change of an electric field over time. By means of this bold postulation, which experiments by Hertz and others surprisingly confirmed, the path was open to the electromagnetic theory of light.

A different, and more 'scholarly' version of this story – one that many authoritative historians of science have embraced (see, e.g., Siegel 1991; Achinstein 1991; Harman 1998; Hon and Goldstein 2020) – attributes a central role to the mechanical model of molecular vortices in Maxwell's paper "On Physical Lines of Force" (1861-2). On this view, Maxwell supposed that electromagnetic forces are described at the micro-physical level by a system of quickly rotating vortices, the basis of magnetic action, which are kept in motion by intervening "idle wheels", representing electric forces. The "displacement current" was then Maxwell's additional posit to give physical coherence to his mechanical hypothesis, while also allowing for an explanation of the phenomena of electrostatics. The fact that the mechanical hypothesis was consistent with all known observation was allegedly taken by Maxwell as "evidence of the hypothetico-deductive character" (Siegel 1991:169) in support of the modified (Ampère-Maxwell) electromagnetic law.

Neither of these stories takes seriously the distinctiveness of Maxwell's approach to physical inquiry. In this talk, I will defend the controversial view that the introduction of the displacement current and the subsequent reduction of light to electromagnetism was the result of Maxwell's ingenious application of concepts of mechanics in accordance with the "method of physical analogy" that he had introduced in the early electromagnetic article "On Faraday's Lines of Force" (1855-56). On this account, the mechanical model of molecular vortices is understood as merely a metaphor to (rather than a hypothesis about the nature of) electromagnetic forces; this metaphorical (or analogical) character is precisely what allows for the crucial conceptual innovation in Maxwell's definition of the displacement current (including his taking both the divergence-free and the curl-free component of the electric field E in its formulation). My reconstruction of how Maxwell arrived at the Maxwell equations illustrates the role of notions in mechanics to the formulation of the electromagnetic equations, at the same time calling for a reevaluation of the originality of Maxwell's cognitive approach to physical inquiry.

Alberto T. Pérez (Universidad de Sevilla)

Einstein 1905 versus Poincaré 1906: a comparative study of two of the founding articles of the special theory of relativity

It is normally considered that the foundational article of the special theory of relativity is the one published by Einstein in June 1905 in the German scientific journal "Annalen der Physik" entitled "On the electrodynamics of moving bodies". But Einstein's article appears in a context in which various physicists are seeking a solution to the problems posed by electromagnetic theory and its apparent incompatibilities with Newtonian mechanics. Two stand out amongthem: Hendrik Anton Lorentz and Henri Poincaré. Lorentz was one of the leading figures in theoretical physics at the turn of the century and had developed what was then known as the theory of the electron. Lorentz's theory reconciled Maxwell's field ideas with the already evident existence of charged particles, in particular the electron, formulating a conceptual synthesis that has survived essentially intact to this day. The incompatibilities of Lorentz's theory with classical mechanics were addressed by him in a series of articles that culminated in the publication in 1904 of one entitled "Electromagnetic phenomena in a system moving with any velocity less than that of light".

The French mathematician and physicist Henri Poincaré published his results on Lorentz's theory in 1906 in the Italian magazine "Rendiconti del Circolo Matematico di Palermo". The work was entitled 'On the dynamics of the electron'. Although published in 1906, this article was written by Poincaré in July 1905, almost simultaneously with Einstein's, and a summary of it was presented by the French mathematician himself at the Paris Academy of Sciences that same year.

The Einstein and Poincaré papers are strikingly similar in content.Both scientists pose thesame hypotheses, deduce from them the same consequences, and arrive at the same mathematical expressions for several of the questions raised. If there are differences between the two, they are more of a conceptual nature, in the physical interpretations that each one makes of the results obtained. These differences in interpretation are especially relevant in thecase of times and lengthsas measured by observers in relative motion.

In this presentation, a comparative study of the articles by Einstein and Poincaré is carried out, emphasizing the similarities and differences in interpretation regarding the fundamental mechanical concepts.

Alessio Rocci (Applied Physics research group and Theoretische Natuurkunde, VUB; International Solvay Institutes)

Merging chemical mechanics with thermodynamics: From the work of Clausius and Gibbs to De Donder's affinity

According to Clifford Truesdell, Thermodynamics deserves a place among the main disciplines of mechanics. He also noticed that around the end of the age of classical mechanics, after the classical works of Sadi Carnot and Rudolf Clausius, "Duhem […] made notable but only partly successful efforts to unite mechanics and thermodynamics". Indeed, following Josiah Willard Gibbs, in his Traité d'énergétique, Pierre Duhem tried to introduce the "mécanique chimique", i.e. chemical kinetics, and the dissipative systems represented by chemical reactions by extending the concepts of non-holonomic systems and the theory of potentials. Starting from Gibbs's and Duhem's work, the Belgian mathematician Théophile De Donder investigated the irreversible processes with the methods of mathematical physics and provided a precise mathematical form for chemical affinity. De Donder invented also the extent of reaction, introducing the time variable and realizing Duhem's attempt to unite mechanics and thermodynamics creating a sort of mathematical chemistry based on classical mechanics. Our talk focuses on the concept of affinity and its formalization obtained by De Donder. His work stimulated a conceptual change creating an overlap between physics, chemistry and mathematics, which would produce a sort of revolution for thermodynamics because it started to address new important characteristics that are fundamental during chemical processes. Although De Donder's work does not properly belong to the age of classical mechanics, it highlights the interdisciplinary relationship between the concepts of classical mechanics, mathematical physics and thermodynamics. De Donder's efforts as a teacher at the University of Brussels and his passion as a mentor for his collaborators were two fundamental pillars for the creation of the so-called Brussels school of thermodynamics. Our work is part of the Solvay Science Project, a joint project involving the Vrije Universiteit Brussel (VUB), the Université Libre de Bruxelles (ULB) and the International Solvay Institutes, that aims both to investigate the history of the Solvay Institutes and the history of physics and chemistry through the lens of the Solvay Archives conserved at the ULB.