
On the relativity of motion

Relitigating debates on the boundary between fact and method

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ABSTRACT

In Part One of this dissertation, I develop a philosophical framework for distinguishing methodological commitments from empirical content in physical theorising. I argue that contingent facts cannot be universal in scope without violating the principle of sufficient reason, and use this to motivate what I call a *cosmological stance*: the view that it is fruitful to consider the universe as a whole a single object which lacks any external material reference. I establish two additional results. (1) Joseph Delboeuf's neglected argument from the *relativity of magnitudes* constitutes a defensible apriorist foundation for Euclidean geometry and is relevant to the modelling of cosmology, and Bertrand Russell's relative-angles objection to this argument is fallacious. (2) The classical law of inertia conflates a methodological component, constitutive of the framework of mechanics, with an empirical component that demands a causal explanation. I use this distinction to clarify Mach's intervention: his critique of absolute space rests not on his empiricism or phenomenalism but on a principled revision of the classical conception of causation, which I call *causal holism*. I also clarify the distinction between the purely formal and conjectural parts of Mach's critique: *Mach's principle* and *Mach's hypothesis* respectively.

In Part Two, I use these results to assess the two relativity theories and a family of classical Machian gravinertial models. I argue that special relativity, on its kinematical interpretation, reinstates precisely the class of causally inexplicable default behaviours that Mach's principle aims to eliminate, and that general relativity undermines the coherent notion of unmediated relative motion at a distance on which Mach's hypothesis depends, leaving Einstein's Machian programme in an ironic predicament. I then recover *Reissner's hypothesis*: the proposal, first clearly articulated by Hans Reissner in 1914, that gravity arises necessarily as a byproduct of a relative inertia theory, and argue that it represents the most natural theoretical consequence of combining Einstein's equivalence hypothesis with Mach's hypothesis. I provide a systematic classification of twentieth-century Machian gravinertial models and show that the internal-motion models of Reissner, Barbour, and Cook implement Reissner's hypothesis more directly than the electromagnetic-analogue approaches of Schrödinger, Treder, Assis, and Sciama. I conclude by exploring how Cook's derivation of Newtonian gravity from the zitterbewegung of Dirac fermions opens a conceptual bridge to quantum mechanics and special relativity, outlining the contours of a unified relational research programme connecting gravity, inertia, quantum mechanics and cosmology.

AUTHOR'S DECLARATION

I declare that the work in this dissertation was carried out in accordance with the requirements of the University's Regulations and Code of Practice for Research Degree Programmes and that it has not been submitted for any other academic award. Except where indicated by specific reference in the text, the work is the candidate's own work. Work done in collaboration with, or with the assistance of, others, is indicated as such. Any views expressed in the dissertation are those of the author.

SIGNED: JONATHAN FAY

DATE: 6 MAY 2026

PREFACE

It is difficult to capture what this thesis is about from a single vantage point, and when I am asked to sum up my topic I find myself emphasising very different aspects according to whom I am speaking. On the one hand, this thesis is an exploration of what has been called ‘Mach’s principle’, including an in-depth dive into its philosophical foundations, and the consequences that its associated research programmes have brought up for theoretical physics. On the other hand, the thesis also seeks to develop a more or less idiosyncratic philosophical stance, including the revival of a certain form of Euclidean apriorism, an affirmation of an obscure 1915 hypothesis seeking to explain the existence of gravity, and a rejection of much of contemporary metaphysics (insofar as universal *a posteriori* propositions are widely accepted as possible). A third, less ambitious reading is possible (and this is often what I will emphasise if I am trying to keep the favour of my superiors) according to which the thesis compiles and catalogues a vast amount of historical information that contextualises our knowledge of the development of some of our current ‘best theories’, sharpening the edges of the narrative, as it were. Those who wish to read the thesis according to this third interpretation are perfectly permitted to do so, but they should keep in mind that I would never have done all this research if it were not in the interests of pursuing the more ambitious goal; and in some cases it seems likely that no one else would have either.

Due to the interdisciplinary nature of this work, it has also been difficult to place it within a single field. Much of the work is historical, yet I do not particularly wish to box myself into the historian’s standards of impartiality. The philosophical method that is used at various points when I give my own analysis, is also somewhat non-standard since it involves very selective engagement with current philosophical literature and instead draws primarily on historical research to motivate ideas. At the same time, parts of the thesis may also tentatively be read as research in theoretical physics, but in this case the methods are also non-standard since speculations are drawn from historical and philosophical analysis, rather than engagement with contemporary literature.

I should also bring attention to another key departure point from standard practice that this thesis takes. The bewilderment I have been met with by certain academics when discussing my ideas: that I might make claims about nature *itself* and not this or that theory, has led me to think that the description of my thesis as a work in the philosophy of science might be inaccurate, and it may better be described as a dissertation in natural philosophy. That people are taken aback by this should not be too surprising, as it has long been a complaint of academic philosophers that they make systems of thought—rather than the world—their object, forgetting that these systems were only erected in the interests of clarifying our

relationship to the world; this tendency expresses itself today most notably in what I have called *formalism fetishism*.

These remarks notwithstanding, I am incredibly grateful to the philosophy of physics community for all the support, help and company I have benefited from throughout these last few years. My experiences travelling and conversing with philosophers and historians of physics have repeatedly reminded me of the value of having a community of people with similar interests to discourse with. Moreover, I am grateful in particular to many individuals who I have found to be open-minded, and who engaged with my ideas despite the peculiarities of my character and specific interests. The support I have found from this community was a continued source of encouragement throughout the years. Among the more senior academics I mention the following, who at various times have shown interest in or support for my work, or whose interactions I have found encouraging. These include: Alex Blum, Matthias Schemmel, Michel Janssen, Jürgen Renn, Iulian Toader, Carl Hoefer, Antonio Vassallo, María de Paz, José Ferreirós, Helmut Pulte, Yemima Ben-Menahem, Wayne Myrvold (who for some reason I cannot have a conversation with without it devolving into heated debate, though I am all the more grateful for such exchanges), Eric Curiel (a similar situation), Tzu Chien Tho, Guido Bacciagaluppi, and Roger Penrose. I also give thanks to the folks at the University of Oxford, who have been welcoming to me on my many visits and participations in their seminars despite the fact that I was not a student there: Harvey Brown, Simon Saunders, Jeremy Butterfield, James Read, Oliver Pooley, Chris Timpson and Sam Fletcher.

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I thank my doctoral advisors: Karim Thébault has been a mentor to me since the first days of my postgraduate studies at Bristol, and has overseen the evolution of my ideas from their embryonic form all the way to the state in which I present them here. Karim has helped me through his continuous efforts to nudge me away from my innate tendency to try to reinvent science from the bottom up, and towards the goal of becoming a respectable scholar with career prospects. The present dissertation can be seen as a kind of compromise between these. James Ladyman has also been a valuable mentor and guide to me throughout my studies, and I am particularly grateful for many stimulating conversations ranging from the foundations of geometry to Plato.

Beyond that I am grateful to certain people I have met outside of academia whom I have

connected with over shared interests. These include: Dennis Braun, Manuel Uruena, Jan Preuss and Chris. I should also give special thanks to Alexander Unzicker, whose contrarian physics videos on YouTube were a formative influence on me since my teenage years and were likely the starting point of some of the core ideas that have developed into the present thesis; I am also thankful to Alexander for his recent enthusiastic support of my work. I credit the equally contrarian writings of Bernardo Kastrup with motivating my decision to move from the study of physics to philosophy.

I am very grateful to my friend Claudia Longo for producing the beautiful sketchbook illustrations that accompany the title page of each chapter of the thesis.¹

Finally, I give thanks to those who have donated online to financially support my research, including a generous donation from Glenn Hinze-Tully, a substantial donation from Dan Girshovich, as well as smaller donations from Francis Fay, Manuel Uruena, Nicholas Landell-Mills and Maxime Le Vigoureux. Additionally, I thank the Royal Institute of Philosophy for gifting me a bursary grant.

Jonathan Fay, 12 March 2026

¹I encourage my readers to visit her website at: <https://www.claudialongo.com/>.

STATEMENT OF PUBLICATIONS BY AUTHOR

1. Most of what has become the first chapter of the thesis was published as:
 - Fay, Jonathan. "On the relativity of magnitudes: Delboeuf's forgotten contribution to the 19th century problem of space." *Studies in History and Philosophy of Science* 106 (2024): 165-176.
2. Most of chapter 4 as well as certain sections (3.1 and 3.2.4) of chapter 3 were published in a single paper as:
 - Fay, Jonathan. "Mach's principle and Mach's hypotheses." *Studies in History and Philosophy of Science* 103 (2024): 58-68.
3. Much of chapter 6 has also appeared in a collaborative paper with Antonis Antoniou, however the sections included in the thesis are entirely my own. This paper is currently under review by *The European Journal for the Philosophy of Science*, and is available on the PhilSci archive under the name:
 - Antoniou, Antonis, and Fay, Jonathan. "Whence the desire to close the universe." (2026)
4. Parts of chapters 7 and 8 (specifically sections 7.1 and most of chapter 8) are from a paper which is currently being reviewed by *Studies in History and Philosophy of Science*, and is available on the PhilSci archive under the name:
 - Fay, Jonathan, and Braun, Dennis. "On Reissner's hypothesis: A review of 20th Century relational models for the unification of gravity and inertia." (2025).Although the ideas developed from collaborative work with Dennis Braun, the written content of this chapter is my own.



Figure 1: *Urizen in the Eclipse*; modification of William Blake's original 1794 relief etching and water-colour *The Ancient of Days*.



Figure 2: At the grave of Ernst Mach, Munich. Photograph taken on 21 June 2025. Left to right: Jonathan Fay, Alexander Unzicker, Jan Preuss, Ernst Mach (funerary stele with urn finial), Dennis Braun, Manuel Uruena. Taken with a Lomo Lubitel 166B.

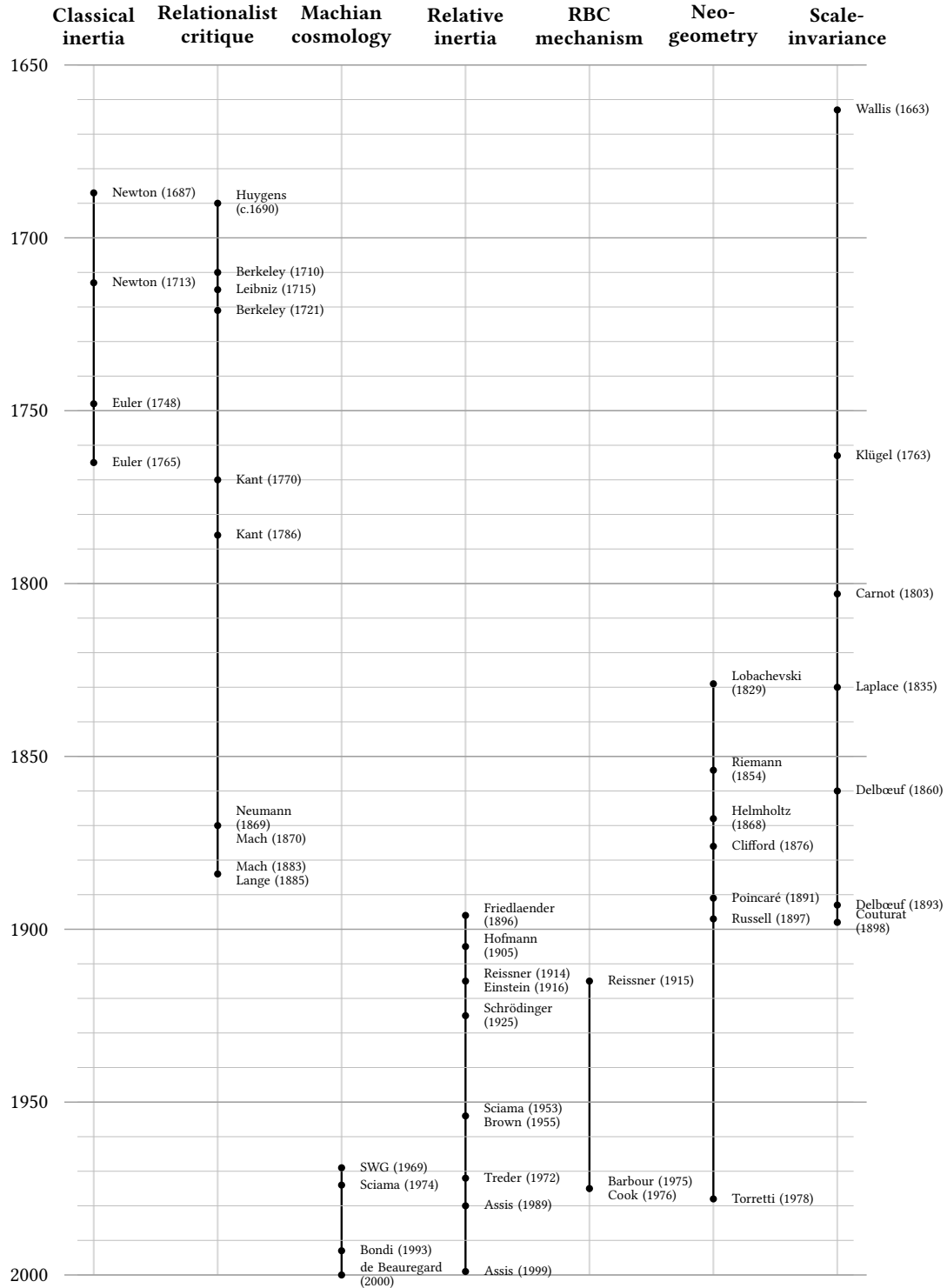


Figure 3: Timeline of ideas depicting the principal strands discussed in this dissertation. Vertical spacing is proportional to elapsed time between 1650 and 2000. Most key papers or books are indicated, with some omissions due to crowding. From left to right the strands are discussed in chapters: 3, 3-4, 6, 6-8, 7-8, 2, and 2 respectively.

For Núria

My whole world's coming apart

—John Maus

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INTRODUCTION

Motivation

Much effort has been put, over the last century, towards seeking a more unified picture of the natural world. In theoretical physics, we are told, the main challenge is to find a way to reconcile general relativity with quantum mechanics. It is somewhat telling then that both of these theoretical frameworks were established over a hundred years ago. In the intervening time physics has changed substantially. There has been a general trend towards greater technical sophistication and use of increasingly esoteric mathematics. But all this has not, it would seem, brought much lasting clarity to the question of *how* a greater unification of science might be achieved, or indeed of *whether* further unification is even desirable and *what* it might look like.

The present project is my earnest attempt to shed new light on what I see as some of the most interesting, and compelling riddles about the natural world, as well as about the relationship that our minds have to this world, which I believe bears on the nature of mathematics. In approaching these questions I engage selectively with recent literature, but my deepest debts are to figures from the past, including: Poincaré and his sense of harmony and careful attention to epistemological issues, Einstein and his single-mindedness and focus on what he sees as physically important principles or hypotheses, and Sciama for his insistence on elucidating the physical content of theories and scepticism towards abstract mathematics. By engaging with older texts, my hope is to bring out new perspectives that sit somewhat outside of the conceptual horizon of current research due to historically contingent choices that were made many decades ago. My strategy will involve re-examining old controversies, and showing why the accepted conclusion may need to be called into question. As we will see, many of my solutions to these puzzles are not unique to me but have arisen before, often in the works of lesser known historical figures, the account of these figures and of their associated debates will serve to anchor my discussion of the issues. The various puzzles I will discuss are of course held together by a common theme, and the answers I give will, I hope,

be seen to naturally thread together such that an internally coherent picture begins to shine through.

The questions that I will address in this thesis concern the nature of space, time and gravity. My investigation will be limited to these and I will not be proposing some kind of unification with quantum mechanics, although I hope that the framework I will provide may be better suited to this sort of thing in the future. As the title suggests, the thesis is centrally concerned with the question of the *relativity of motion*. It is around this central question that the various other topics I will engage with, including the foundations of geometry, the law of inertia, the conservation of energy, and the role of gravity are naturally connected. The main novelty I bring to all of these debates in essence comes from giving careful attention to the distinction between the universe as a whole and its subsystems. I argue that debates concerning relativity and symmetry have historically been insufficiently attentive to this distinction, and that this issue affects the theories of special and general relativity just as much as it did classical mechanics. The claim upon which much of this thesis rests is that it is, coherent to consider the universe as a whole as a single object, and that doing so opens up the possibility of a form of *rationalism* which allows us to clarify what the aims of theoretical physics ought ultimately to be. The reasons for this will be spelled out in the first chapter.

Methodology

The method of the thesis combines historical research with philosophical analysis and the articulation of heuristics for further theory development. The structuring philosophical move, developed primarily in the first half of the thesis, lies in a careful examination of the roles played by the concepts of law, causality, matter, inertia, space, time, symmetry and relativity within scientific theorising; and from this examination there emerges a strict distinction between two kinds of principle: those that are *constitutive* of scientific description itself, and which therefore belong to its method, and those that constitute *actual* or *empirical* claims about the world, and which are therefore subject to the contingency proper to all matters of fact. I argue that several principles typically treated as facts about the world, including the law of inertia, the homogeneity of space and time, the principle of conservation of energy, and various relativity principles, are primarily methodological commitments, and that the failure to observe this distinction has led to more genuine confusion in the foundations of both classical and post-classical physics than has been so far acknowledged.

This distinction motivates what I call the *cosmological stance*, which takes the universe as a single coherent system and thereby rehabilitates certain broadly Kantian commitments.

According to the cosmological stance, empirical inquiry tells us only about contingent matters of fact, and contingency is the exclusive domain of *matter*, i.e. that which participates in the chain of cause and effect. Statements about the material cosmos as a whole, by contrast, are *universal*, and must be analytic, a priori and necessary; these can be grounded only in the methodology of science or of thought in general.² Symmetry principles and relational constraints for the universe as a whole are therefore not mere postulates but rationally grounded commitments, however these do not translate to the actual symmetries of subsystems. I argue that the cosmological stance is what has implicitly underlain Mach's principle, that it is essential to any rationalist approach to cosmology, and that it is necessary if science is to *explain* the natural world rather than merely *describe* it.³ The first half of the thesis as a whole serves to ground a relationalist research programme in this view of science, providing the conceptual framework within which the second half engages the hypothetical and empirically rooted dimensions of physical theorising.

The historical research is conducted with close attention to primary sources, and with particular attention paid to figures and debates that have received little sustained analysis in contemporary philosophy of physics: the argument traceable to John Wallis, and later to Joseph Delboeuf, in defence of Euclidean geometry on grounds of scale-invariance; the roots of Mach's principle in Mach's holistic view of causation; and Hans Reissner's early twentieth-century proposal to derive gravity as a necessary consequence of the relativity of inertia. Rather than treating the past as a prelude to present orthodoxy, I adopt throughout a contextual and reconstructive approach, interpreting arguments within their original conceptual settings and re-examining them on their own terms. The aim is to identify historically contingent choices that have been uncritically inherited, and to recover lines of inquiry that were prematurely abandoned.

The second half of the thesis turns to concrete physical theories. Chapters 5 and 6 examine special and general relativity respectively, assessing their conceptual foundations and outstanding difficulties in the light of the framework developed in the first half. The final two chapters adopt a heuristic and model-oriented strategy, which is concerned with the identification, classification and critical assessment of certain Machian approaches to gravity and inertia that have on the whole been overlooked by contemporary physicists and philosophers. The guiding concern throughout is to distinguish the genuine *physical* content of

²This split between method on the one hand and the natural world given in experience on the other is reflected in the two-part division of the thesis chapters.

³Note that it is of course somewhat ironic to associate Mach with rationalism since he was a professed empiricist. This tension will be addressed in chapter 4, in which I locate the rationalist potential of Mach's ideas in his *holism*.

theories, that is, that which constitutes a hypothesis about nature, from what is merely formal structure. This concern is driven by the conviction, argued for in the first half of the thesis, that the laws of nature ought ultimately to be *explicable*, and that we ought not accept seemingly arbitrary postulates as brute universal truths.

Novel contributions

The thesis makes four central novel contributions. The first is the articulation and defence of the *cosmological stance*, together with the strict distinction it grounds between constitutive methodological principles and empirically grounded physical hypotheses. The second is a defence of Euclidean apriorism in the context of the 19th century philosophy of geometry. The third is a clarification of the underlying motivations behind “Mach’s principle” and of the distinction between the methodological and hypothetical parts of Mach’s idea. The fourth is the recovery and articulation of *Reissner’s hypothesis*: the proposal that gravity exists as a necessary consequence of the relativity of inertia, which I regard as the central theoretical contribution of the thesis, and was the guiding idea that initially motivated my research.

Among the methodological and philosophical contributions, I develop a critique of universal laws of nature on the basis of the principle of sufficient reason; I provide a methodological critique of the rejection of the principle of conservation of energy in general relativity, connecting this to a broadly Kantian view of causation; I develop a distinction between two conceptions of the classical law of inertia, the one entirely a priori and rooted in the methodological framework of classical mechanics, the other embodying an empirical fact and therefore demanding a causal explanation; and I argue that Mach’s critique of classical inertia rests not on empiricism alone but on a holistic revision of the concept of causation, according to which there is no notion of default behaviour in the absence of causes, but rather inertial motion is simply motion caused by an environment which is in some respect homogeneous.

Among the historical recoveries, I recover Joseph Delboeuf as an important figure in the history of nineteenth-century debates concerning the foundations of geometry, and provide a defence of his argument for Euclidean apriorism on the basis of the relativity of scale, together with a refutation of Bertrand Russell’s ‘relative angles’ objection; I recover Hans Reissner as the first to clearly articulate the idea that gravity may be derived from relative inertia; I reconstruct the Einstein–Wheeler argument for spatial closure and uncover the influence of Hermann Weyl’s dialogue “Inertia and Cosmos” upon Wheeler in this period; and I provide a systematic classification of the twentieth-century classical Machian models, argu-

ing in this context that the internal-motion models of Reissner, Barbour and Cook uniquely realise Reissner's hypothesis.

Among the more interpretive and technical contributions, I argue that Einstein's approach to special relativity ultimately undermined his own Machian aims, since the standard interpretation of this theory stands philosophically in direct opposition to Mach's principle; I provide a preliminary philosophical defence of the proposal that special relativity may better be understood as a consequence of the wave-nature of matter than as a feature of spacetime structure; I argue that just as special relativity inadvertently undermined Mach's principle, general relativity inadvertently undermined Mach's hypothesis by rendering unmediated relations at a distance untenable; and I provide a proof of a null result for the dynamical emergence of electrostatics within the context of Maxwell's theory.

Structure, questions and overview

The thesis is divided into two parts of four chapters each. Part I addresses the method of science, and Part II addresses the application of that method to the interpretation and development of physical theories. Each chapter is organised around a guiding question, stated at its opening, which the chapter seeks to answer:

1. Why does nature conform to laws?
2. What is Euclidean geometry grounded on?
3. Is the law of inertia a fact or a definition?
4. What is the cause of inertial motion?
5. Why are the laws Lorentz-covariant?
6. Is relative motion at a distance even possible?
7. Why does gravity exist?
8. Is zitterbewegung the source of static interactions?

Chapter 1 examines the boundary between methodological commitments and empirical content in physical theories. I argue that insofar as laws are taken to be empirically informative, rather than mere definitions held a priori, it is incoherent to require that they be universal, since this generates an explanatory demand that cannot be met, and therefore violates

the principle of sufficient reason. The chapter then turns to the law of causality, which I argue captures and defines the concept of matter, and explains why the material world must be conceived spatio-temporally, both space and time being necessary for the representation of causation. From this perspective the abandonment of a fixed background in general relativity is methodologically unsound; a scheme that restored such a background for methodological purposes would treat energy conservation as a regulative a priori principle, the image in physics of our presupposition of causality. The chapter closes with an analysis of the relativity of motion, drawing a distinction between a general, conceptual form of spatial relativity and the more restricted relativity principles applicable to dynamically isolated subsystems.

Chapter 2 engages the foundations of geometry by returning to the second half of the nineteenth century, when these issues were actively contested. I argue that the programme of the neo-geometers, according to which Euclidean geometry is just one among many candidate *forms* for phenomena, met with serious opposition, and that Delboeuf's argument from the *relativity of magnitudes*, almost entirely forgotten today, retains both genuine merit and continuing relevance to physics conceived at cosmic scale. After setting the scene with epistemological themes in Poincaré's philosophy of space, I contrast the approaches of Helmholtz, Riemann and Poincaré with that of Delboeuf, noting a precursor in John Wallis and a striking convergence with Leibniz. I analyse Delboeuf's approach as an attempt to fully disentangle the *physical* from the *methodological* aspects of geometry, recovering a strict separation between space and matter that early modern philosophers had taken for granted. The attempt succeeds, I argue, but only on condition that we can consider the universe as a whole in the manner established in the previous chapter. Central to Delboeuf's account is his insistence that geometry rests on an abstract conception of space, rooted in the *process of abstraction* and in the *striving after universality* that characterises our description of nature, not on any empirical property of a physical medium.

Chapter 3 traces the development of the concept of inertia from Newton through the eighteenth and nineteenth centuries, focusing on the persistent tension between the empirical inaccessibility of absolute space and its apparent necessity for the formulation of mechanics. I examine how early critics of Newton struggled with the status of rotation and the ontological standing of force, and how Euler, Kant, Laplace and Poisson attempted to reconcile the necessity of absolute space for mechanical explanation with its epistemological inaccessibility. The central concern of the chapter, however, is the ambiguous status of the law of inertia itself. Uncritical adherence to conventions rooted in the *method* of science can lead to dubious metaphysical commitments which obscure the boundary between what is methodological and what is genuinely physical, and the recurrent critique of absolute motion that

runs through this history is best understood as a series of attempts to disentangle these. By drawing the distinction more precisely than my predecessors, I offer a reinterpretation of classical mechanics that preserves its internal coherence.

Chapter 4 analyses “Mach’s principle” with close attention to Mach’s own writings. I discuss Mach’s *holistic* revision of causation, according to which there is no *default* behaviour in the absence of causes; seemingly balanced behaviour reflects rather a balance or homogeneity in its cause. I develop this view through Mach’s analysis of Archimedes’s balanced scale, and show how Mach’s holism leads both to his critique of the law of inertia and to his rejection of the dual status of absolute space identified in the preceding chapter. The central interpretive contribution of the chapter lies in a distinction between the *formal* part of Mach’s principle, which is an affirmation of relationalism, and the *hypothetical* part, which I call *Mach’s hypothesis*: the proposal that the physically observable content of the inertial law—the class of *preferred reference frames* in which inertial forces vanish—has its causal origin in the relation of a system to the sum of the masses of the universe.

Chapter 5 re-examines the conceptual foundations of special relativity in light of the preceding analysis. The central issue is that the received *kinematical* interpretation of special relativistic effects stands in direct tension with Mach’s principle, which rejects the possibility of purely kinematical behaviours. The problem can be framed in terms of a tension between two senses of the relativity principle, already encountered in earlier chapters. Both in Einstein’s writings and in those of Poincaré there is a tendency to conflate the empirically grounded relativity principle of special relativity—the local unobservability of the ether frame—with a stronger formal requirement that motion is relative in general, a claim that can only be justified a priori. I argue that this conflation is what underlies the initial view of Einstein that the unobservability of the ether frame implies that the ether does not exist. The tension can be resolved if special relativity is understood as a theory that properly applies to subsystems of the universe rather than to the universe as a whole, and from this perspective it is inappropriate to apply what Wallace (2022a,b) calls the “cosmological assumption” to the theory. I then address a question raised by Michel Janssen: if the substantivalist appeal to “spacetime” is unsatisfactory, what then is to explain the Lorentz invariance of physical interactions? I explore the proposal of Kroupa and Schmidt, according to which Lorentz invariance reflects the wave-like constitution of matter, and is therefore closely aligned with quantum mechanics.

Chapter 6 explores the complex relationship between Mach’s ideas and general relativity. Einstein’s engagement with Mach shaped the early development of the theory, and the programmes of Wheeler and Sciama, which conditioned the intellectual environment of the

renaissance of general relativity, likewise treated Mach’s principle as a cornerstone. I argue that Einstein’s argument for a closed universe on Machian grounds rested on assumptions tied to a non-expanding cosmology, and trace its survival in Wheeler’s early *relativity notebooks*. I conclude that the Einstein–Wheeler argument would have been better construed as one for the universe’s temporal horizon than its spatial closure. I then turn to Sciama’s lesser known Machian programme. During his doctoral studies, Sciama derived a field model of gravity and inertia from a vector potential, obtaining the striking result that Mach’s principle implies a constraint on the total gravitational potential, $U = c^2$, which entails that each particle’s gravitational potential energy exactly cancels its rest-mass energy. After the decline of the Steady-State model, this constraint was taken to favour the Einstein–de Sitter cosmos, but Sciama’s failure to reproduce the result rigorously in general relativity led to the decline of his programme in the 1970s. The chapter closes by addressing Weyl’s epistemological challenge to unmediated relative motion at a distance, for which I offer a response grounded in the methodological framework of the first half of the thesis.

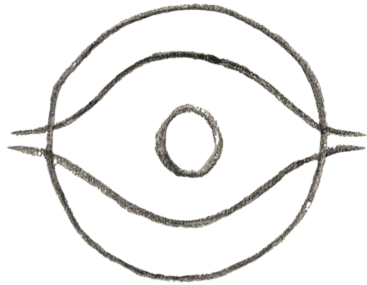
Chapter 7 introduces what I call *Reissner’s hypothesis*, traces its historical origins, and discusses its philosophical significance. Reissner’s hypothesis, according to which gravity exists as a necessary side-effect of the relativity of inertia, is framed as the most natural interpretation of the convergence of the two hypotheses that motivated Einstein’s initial investigations into gravity: Einstein’s *equivalence hypothesis* concerning the mechanical and optical identity of an acceleration field with a field of constant gravity, and *Mach’s hypothesis* concerning the material origin of inertia in mass-mass interactions. I connect Reissner’s hypothesis to the broader framework developed in Part I, and especially to chapter 1, emphasising the unificatory ambition of his idea and the way such unifications are emblematic of enduring scientific progress. The historical reconstruction draws attention to lesser known figures, including Benedict and Immanuel Friedlaender, Wenzel Hofmann and August Föppl. This chapter serves as a conceptual pivot in the second half of the thesis, redirecting attention from the relativity theories of the previous two chapters towards concrete theoretical models, where the question is whether these successfully embody Reissner’s hypothesis.

Chapter 8 surveys twentieth-century Machian gravinertial models, both electromagnetic analogues and internal-motion approaches, with the aim of clarifying how inertia might be rendered fully relational and gravity thereby *explained* rather than merely postulated. I begin with the electromagnetic analogue models of Schrödinger, Assis, Treder and Sciama, each of which replaces absolute kinetic energy terms with velocity-dependent relational potentials and consistently yields Machian relations of the form $U \sim c^2$, with the effective gravitational coupling determined by the total mass distribution of the universe. I discuss the persistent

challenges these models face, in particular the contested issue of the anisotropy of inertial mass, and examine Sciama's Maxwellian analogy as a methodologically distinct approach that hints at a deeper connection between the inertial-gravitational coupling ratio and the constraints of special relativity. I then turn to what I argue are the purest instantiations of Reissner's hypothesis: the internal-motion models of Reissner, Barbour and Cook, in which no independent scalar gravitational interaction is postulated and an effective gravitational attraction emerges from velocity-dependent inertial couplings when matter is assumed to undergo rapid internal oscillation. Barbour's suggestion that this motion be identified with the *zitterbewegung* of Dirac fermions opens a conceptual bridge to quantum mechanics and special relativity, made explicit in Cook's independent derivation, where properties of the Dirac velocity operator supply precisely the numerical factors needed to recover Newtonian gravity. I close with the speculative but ambitious research programme that emerges from these insights, one that would bring together gravity, inertia, special relativity, quantum mechanics and cosmology in an intimate and inseparable manner.

Part I

METHOD



UNIVERSAL SCIENCE

But life is short and truth works far and lives
long: let us speak the truth.

Arthur Schopenhauer, 1818

Why does nature conform to laws?

Introduction

In this first chapter, I will be sketching out the philosophical stance that will be adopted throughout the thesis.¹ I will most likely not be able to justify this stance to the full satisfaction of philosophers, and it will not be developed dialectically by contrasting it to all the relevant competing views in present day literature (apart from some brief remarks contrasting my view to Michael Friedman's later in this introduction). The reason for this is that the stance I adopt here was not developed due to explicit disagreement with the contemporary philosophical literature, rather, it is a reconstruction of the views that I found are implicitly presupposed if one is to take the ideas developed in later chapters as philosophically well motivated. This chapter should therefore be read as an exercise in unpacking and making

¹I use the term *stance* in a sense compatible with that of [Van Fraassen \(2002\)](#), according to whom a stance involves not merely a set of propositional commitments but a constellation of epistemic values, methodological commitments, and characteristic attitudes toward explanation and metaphysics. The stance I attempt to develop here may provisionally be called *the cosmological stance*, in contrast to Van Fraassen's "empirical stance."

explicit certain intuitions that many scientists may hold, but that have not yet been adequately articulated in philosophical terms. In particular, I found that in order to view what we call “Mach’s principle” (see chapter 4) as a worthwhile idea, it follows that one should be philosophically committed to a strict distinction between *a priori* and *a posteriori* knowledge, from which it will be argued that propositions that are *universal* in scope cannot be justified empirically and therefore must seek *a priori* justification if they are not to be abandoned. We will clarify the reasons for this in section 1.1. This stance motivates the exclusion of certain arbitrary elements in the laws of nature which we typically take to be universal, such as: (1) the preferred class of inertial reference frames, related to this (2) the applicability of and limits to relativity principles (including the Galilean relativity principle or the Einstein-Lorentz-Poincaré relativity principle), (3) the existence of certain natural constants, assumed to be universally valid, which could clearly have taken different values. A fuller justification for the views developed in this first chapter will be found spread out throughout the thesis and will involve dialectical engagement with mostly historical sources. For now however, I have tried to express the views adopted in the clearest and most logically consistent manner to make up for their incomplete discussion.

The strict distinction between *a posteriori* and *a priori* knowledge that is assumed throughout this thesis can be thought of as aligning with the distinction between the *content* and the *method* of science. The first has to do exclusively with contingent matters of fact, that is, causes and effects which are necessarily spatio-temporally specific, i.e. apply inhomogeneously over space and time. The second concerns the methodological presuppositions that are constitutive of our accounts of empirical matters of fact, but themselves lack empirical content. This is the definitional framework that scientific theories presuppose, and since it is concerned with definitions and their consequences, it is entirely analytic and *a priori*. This framework is not entirely arbitrary however since our definitions are guided by extra-theoretical considerations such as a commitment to the *unity* of nature, or the presupposition of *causality*.

The account we will give is *similar*, in certain respects, to the neo-Kantian view of Michael Friedman, who draws especially from many of the influential thinkers in the foundations of physics around the turn of the 20th century such as Poincaré, Helmholtz, Reichenbach, Schlick and others, to update the Kantian doctrine such that the *a priori* framework is *relativized* to the historical development of scientific theories.² We will reject this *relativization* however by pointing out that even though the principles of a given theory may be specific to it, it is possible to distinguish those parts which derive from facts from those which are

²See Friedman’s views expressed in [Friedman \(2001, 1992\)](#).

rooted in universal methodological considerations. Since most theories are not constructed (or shouldn't be taken) as theories of the universe as a whole, it is perfectly legitimate that they harbour content which is taken as constitutive but discretely smuggles in certain contingent facts, such is the case as we will see with the law of inertia and relativity principle of classical mechanics. By far the main motivator for the relativization of the *a priori* that Friedman reads into the intellectual developments of the late 19th and early 20th centuries is the rejections of Euclidean apriorism due initially to the discovery of non-Euclidean geometry by Lobachevsky and Bolyai in the early 19th century, and later to the application of this branch of mathematics to physics in Einstein's theories in the early 20th century. In this thesis we will argue that this rejection of Euclidean apriorism is only relevant to the geometry of the *physical* space assumed in the modelling of subsystems of the cosmos, however it should not apply to a science of the cosmos as a whole.

In the present chapter, we introduce our stance by examining three issues which have to do with the boundary between universal methodological commitments and the empirical content of natural science. We start in section 1.1 with the assumption of nature's conformity to law. In this context we discuss a fundamental problem with the widespread assumption that there can be universal laws harbouring empirical content, and motivate a research program which seeks to exclude such universal laws and explain these in terms of more fundamental more unified theories in which contingent facts are not smuggled into the definitional framework. This will lead us in section 1.2 to discuss more specifically the law of causality, its status as a universal methodological principle, and how the content of physics, namely *matter* is fully characterised by its participation in causal action. That is to say, there is nothing *essential* to the concept of matter apart from its causal action. We will also briefly discuss how in physics the law of causality has historically found expression in the principle of conservation of energy, which could therefore be treated as similarly universal and justified on methodological grounds alone, but that this was abandoned in the move to non-Euclidean geometry, i.e. in the rendering of the background with respect to which physical quantities are defined as something which participates in the causal dynamics of physical systems. We characterise this theoretical development as a historically contingent choice and criticise it as methodologically shallow. Finally, in section 1.3 we will discuss the issue of the relativity of space, distinguishing the general sense in which this relativity is true *a priori* from certain empirically motivated relativity principles which apply to dynamically isolated subsystems. We discuss the fallacy which Wallace (2022a,b) identifies as the "cosmological assumption," and focus on the contrast between the relativity of space in the context of Leibniz-shifts on one hand and Galilean relativity on the other.

1.1 The lawless universe

The lawfulness of nature is a presupposition of all of our enquiries, for how could we possibly get started with the contrary assumption. Nature does not supply us with facts neatly packaged from which we may ask whether these evidence a conformity to or violation of lawfulness, instead we are given raw information which can only be sorted into patterns provided that we are inclined to seek these. It is often said that visually we are given, as a starting point, a two-dimensional screen of information from which we can subsequently deduce our distance from objects by observing how the image changes as we move or by comparing the images supplied by each eye. However, an original two dimensional screen already presupposes knowledge of angles, that is of which receptors are located next to which others, and this can only be taught by the observation of the motion of bodies in our visual field, and these objects were themselves identified as instantiating persistent patterns in our sensations. Therefore even our most basic perceptions presuppose a mind inclined towards pattern recognition. The vast majority of the work has been done by the species prior to the emergence of our rational faculties and the development of science. For this reason, science takes as its precondition a thoroughly navigable world populated by familiar objects whose boundaries are relatively well defined. But in seeking a more profound and more unified account of nature, and in gathering together, not just two eyes, but the resources of countless individuals bound into communities by social instincts and a capacity to communicate with language, science tends to leave aside these familiar objects and reconceive the world as composed of other objects, quite alien to our usual perceptions, which allow us to engage with nature in a more technologically sophisticated manner and drastically amplify our power in the world.

Historically, the effort to focus the attention of human beings upon a more fundamental law-governed world was bound up with (often monotheistic) religion. This brought with it a common prejudice according to which transcendental principles are personified as real things, or as agents that may undertake mythical adventures in this world. As such, for Newton absolute space is methodologically necessary but also something physically real. More generally, laws of nature are treated as universal governing principles with a kind of transcendental status with respect to matter, and yet they, like matters of fact, harbour empirical content and therefore could conceivably have been different.³ An important shift in perspec-

³For further discussion of the universal laws of nature and their status, including some discussion of the distinction between the physical content of a theory and by-products of its mathematical representation see the many excellent entries in the collection [Ben-Menahem \(2022b\)](#). Although it should be noted that the view Ben-Menahem develops concerning “lawlessness” ([Ben-Menahem, 2022a](#)) differs substantially from what we call “the lawless universe” in this section. While the ideas we present here also have significant crossover with

tive was brought about by Kant, who argued that the universal part of science should stem from the “conditions for the possibility of experience,” which place an unbridgeable gap in between our *experiences of things* and *things-in-themselves*, the latter being made essentially unintelligible. The universal laws of nature for Kant must then be derived exclusively from the a priori conditions for the possibility of experience (Hatfield, 2004, p.50). As we have said, today Kant is usually rejected by appeal to his allegedly mistaken assumption that the geometry of space would not become the subject of empirical inquiry, however we will be drawing from the lesser known 19th century Belgian philosopher-mathematician Joseph Delboeuf in the next chapter to defend the apriorist view on this matter (see chapter 2). Delboeuf however did have some criticisms of Kant and other apriorists which we agree with. In particular he complained that in order to explain the “apodicticity of the geometric laws” these thinkers make them “the laws of our nature” or “the forms of our sensitivity” but do not explain why exactly these laws apply and not some others (Delboeuf, 1860, p.8). To explain why Euclid’s axioms must be assumed, Delboeuf would appeal to the *method* of science. But while we will not address his argument concerning geometry at present, some of Delboeuf’s more general methodological remarks concerning the central role of *abstraction* in scientific reasoning will be valuable to us since they will help to motivate our discussion of universal laws of nature.

In a section titled “Of the universal science” (Delboeuf, 1860, chp.2, §1), Delboeuf lays out his ideas concerning the process of scientific abstraction. For Delboeuf, science proceeds via a two-stage method: (1) on one hand “The human mind, faced with nature and the facts it sees there, generalizes them more and more, that is to say, reduces them to more or less general laws, always concrete and phenomenal [...] these laws [...] are, basically, only other facts less numerous but just as incomprehensible as the first,”⁴ on the other (2) “when, starting from these general facts, and granting them an ideal and subjective character, the scientist applies his own laws to them, he constructs an entirely subjective universe, where the facts are

Cartwright (1983), including similar treatment of the issue of idealisations, the main argument we will make concerning the impossibility of universal laws with empirical content has not specifically been made in recent literature that I am aware of, although I should thank Adam Weiler Gur Arye for alerting me to the similarity of my views with some of Berkeley’s remarks, especially in Berkeley (1710, §105-107). Additionally, it is worth noting that my the views I will express here intersect substantially with those of the British cosmologist E. A. Milne, especially in his essay “on the origin of the laws of nature” (Milne, 1937). Milne’s contribution was unfortunately brought to my attention only after this chapter had been completed, and I was unable to integrate his discussion properly into the body of the text; a fuller engagement with his views will have to be left to future work.

⁴The full passage in French reads: “L’esprit humain, en face de la nature et des faits qu’il y voit, les généralise de plus en plus, c’est-à-dire les ramène à des lois plus ou moins générales, toujours concrètes et phénoménales, qui n’ont jamais, en tant qu’objectives, un caractère de généralité absolue, d’évidence, de nécessité ; ces lois, auxquelles il soumet les faits, ne sont, au fond, que d’autres faits moins nombreux et tout aussi incompréhensibles que les premiers” (Delboeuf, 1860, p.39).

placed and classified in such a way as to explain themselves.”⁵ The *universality* of the laws of science comes only in this second stage, since the first only gives us facts grounded in experience which therefore remain essentially contingent observations, just with a greater level of generality than the original datum of observation. However, through a process of *abstraction* in this second stage, propositions about nature gain an *analytic* or “apodictic” status, becoming apparently universal and certain. How is this possible?

This is not achieved due to any sort of empirical certainty, but instead due to the definitional framework within which our scientific terms become embedded. As an example, Delboeuf discusses the chemical composition of water. He points out that we often say it is “necessary” that one volume of oxygen combines with two volumes of hydrogen to form water. But this necessity depends entirely on how we define “oxygen.” If “oxygen” denotes an unknown substance identified through its effects—a kind of placeholder—then the law is empirical and contingent. But if we define oxygen “*genetically*”, as “the body which goes to the positive pole, when we decompose the distilled water by a battery”, then the law becomes true by definition. The distinction here is between definition *de re*, naming a real but perhaps unknown substance, and definition *de dicto*, a nominal or conceptual specification. Once we define water in terms of a precise ratio of hydrogen and oxygen, the necessity of that composition becomes analytic: a substance with a different ratio, no matter its empirical similarity to water in other respects, would simply cease to be “water” (Delboeuf, 1860, p.47).

This is not only true of the definitions of terms, but also of the *laws* of nature, as soon as these laws are granted a universal status. As such if I decide to grant Newton’s law of gravitation a universal status, I am perfectly permitted to do so, however I may have to change some other definitions to accommodate for unexpected phenomena. For instance, I may need to change the way that I define mass, or else I may have to introduce additional forces between bodies to account for any deviations from expected purely gravitational behaviour. Which laws we choose to make universal and true by definition, and which we choose to subject to empirical scrutiny is ultimately conventional, however the choice is very much guided by considerations of parsimony, simplicity and unity. Once we look back at the image of the world that science has produced, we ask ourselves, *why does the world conform to universal laws?* But in this regard we are only seeing reflected back at us the image of the presuppositions of our own investigations, after having tirelessly endeavoured to *sort* the world in accordance with our assumptions.⁶

⁵Translation of: “D’un autre côté, quand, partant de ces faits généraux, et leur accordant un caractère idéal et subjectif, il leur applique ses propres lois, il construit un univers tout subjectif aussi, où les faits sont placés et classés de manière à s’expliquer d’eux-mêmes” (Delboeuf, 1860, p.39).

⁶Note that the view we have presented here conflicts with the distinction that is made in contemporary

Moving beyond Delboeuf's remarks now, I contend that insofar as the laws which we have chosen to set down appear as *arbitrary*, i.e. such that we can plausibly say that they *could have been different*, we are admitting that these laws are not really part of our definitional framework, but instead represent very general empirical facts. If this is the case however, it is not legitimate to regard these laws as *universal*, since this kind of absolute induction cannot be made regarding empirical things. Thus, as long as we understand laws as states of affairs about the world, there cannot be any universal laws of nature, and the prejudice in favour of these comes from a mistaken application of mathematical reasoning to empirical reality. To be specific, the existence of universal laws with empirical content would imply a violation of the *principle of sufficient reason*: such laws would not be derivable on *a priori* grounds, but similarly it would not be possible to justify them *empirically* since empirical facts only concern contingent states of affairs that may be true here or there and now or then but never everywhere and always. We would thus be faced with an explanatory demand that cannot (even in principle) be met, and it follows that such propositions should be rejected as a matter of principle.

This insight need not be taken pessimistically however, since it motivates a specific research programme. In particular, if science seeks to be *universal*, it must seek to distinguish those parts of the general laws of nature which are really consequences of definitions that we are obliged to lay down from those parts which harbour actual empirical facts. Once such facts are identified, they should then be recognised not as necessary and universal but as contingent, and therefore subject to potential spatio-temporal variability. Such is the case in particular with many so called “natural constants,” or rather with dimensionless ratios of these that are immune to arbitrary changes in our unit conventions (we will come back to this in chapter 7). If science is to be truly universal, it should seek to eliminate these quantities and give some account of why they presently take the values that they do. So long as physics is concerned with subsystems of the universe, it is perfectly legitimate to treat

metaphysics between *nomological* necessity (having to do with laws) and *metaphysical* necessity (having to do with the definitions of terms). We will not go into any detail here defending our stance against the contemporary philosophy literature, but simply assume that this distinction is artificial. We also reject the more outlandish claims of metaphysicians in the last century such as Kripke et al. (1980) who argued in favour of the existence of *necessary a posteriori* as well as *contingent a priori* propositions, giving the case of the sentence “I am here” as an example of the latter. Briefly, what we actually have (I argue) is a pair of distinct propositions disguised as a single one. The statement “I am here” can be decomposed into (i) “I am where I am,” an analytic and *a priori* truth, and (ii) “I am in this particular place,” a contingent and *a posteriori* proposition. Kripke exploits the indexical ambiguity of “here” to fashion a sentence that seems to cross boundaries; other examples can be subjected to a similar decomposition. A thorough treatment of this issue would distract from the central aims of this chapter so it will not be attempted here. To clarify, in this thesis, we assume that all proposition that are *analytic* are also *necessary*, *universal* and *a priori*, whereas all proposition that are *synthetic* are also *contingent*, subject to spatio-temporal variability and *a posteriori*.

apparently arbitrary laws as effectively universal by idealisation for practical purposes. But once we consider the universe as a whole, these laws should disappear.

One question remains: is such a *science of the universe*, in which contingent laws will be eventually eliminated, actually attainable? This ultimately will depend upon the validity of certain hypotheses that would make the universe something that is in reach of our scientific investigations. The remarks we have made so far concerning a science of the universe simply follow from the meaning of the term “universe” which to us means the totality of matter, i.e. of that which is, however indirectly, in causal connection with us. The applicability of these remarks however depends upon the coincidence of this universe with what cosmologists call the “observable universe.” If on the contrary we accept the existence of what has often been called a “multiverse,” i.e. the existence of states of affairs beyond our observable universe that are in a broader sense connected to us (perhaps due to some common origination), but that are at in all practical respects inaccessible, then the reasoning we have laid out above becomes irrelevant. This is because in such a multiverse, statements that may be true of the totality of the observable universe are not in fact true *universally*, therefore facts that appear arbitrary are perfectly permitted to apply throughout the observable universe and stand beyond the reach of any explanation. Throughout this thesis, we will be assuming that considerations of the universe as a whole are in fact pertinent questions to science, therefore if the hypothesis of the “multiverse” or some similar hypothesis is correct, this thesis will severely be diminished in relevance. Either way however, it will have been worthwhile to pursue the idea that a science of the universe *is* possible, in order to see what are the consequences that this should have.⁷

1.2 Cause and effect

Although, as we have said, the definitions that we give to words are ultimately conventional and tend to change over time due to the changing concerns of people and the disciplines they take part in, it will be useful for us to have a concept which captures all things insofar

⁷With this in mind we could also briefly consider certain principles that we might expect nature to conform to if the kind of reasoning we hope to apply to it is legitimate: (1) It would seem strange to allow some material substance which has at some point been in causal connection with us to leave this connection entirely. (2) We might want to insist that the universe should contain an infinite amount of matter and be infinite in extent either spatially or temporally, since otherwise its global state at any particular time would be wholly specifiable and thus constitute a contingent and yet universal truth which would thereby be inexplicable. (3) On the other hand we might expect that certain sums over the influences of all matter upon us *converge*, which seems to contradict (2), however all three of these criteria might plausibly be satisfied by a model of the cosmos for which the point where the scale factor goes to zero coincides exactly with the horizon of our observable universe, such as the Einstein–de Sitter model for instance.

as they participate in causal action. To this concept we assign the term “matter,” as per the convention of pre-relativistic physics. *Matter* therefore comprehends anything that participates in causation and is agnostic as to whether the thing is conceived as being composed of infinitesimal particles, spatially extended but limited atoms that fill small regions of space, or fields that permeate all of space but communicate causal influences through their propagating fluctuations.

The world insofar as it is thought as material therefore, is strictly subject to the law of causality; moreover, it is due to our subjecting this world to the law of causality that we think of its contents spatio-temporally. The reason for this has been explained by [Schopenhauer \(1819, Bk.1, §4\)](#) who points out that a world in space alone would be “rigid and immovable, without succession, without change, without action,” whereas a world in time alone would be “fleeting, without persistence, without contiguity, hence without co-existence, and consequently without permanence.” In neither case do we have the conditions necessary for the representation of causal action, therefore “[o]nly through the union of space and time do we reach matter.” It is therefore because we think of the world in causal terms that we must represent it spatio-temporally. The world thought of as *material* exists *spatio-temporally*, and conversely, the world insofar as it is *spatio-temporal* is composed of *matter*. The existence of matter is ascertained *empirically* that is to say, due to its causal connection to other material things that are ultimately in causal connection with our own bodies. It is therefore only insofar as material things act causally that they can be causally connected to us, and it is only in the sense of their causal action that we consider them real.

Scientific inquiry in general is concerned with the question of what causes produce a given effect but does not inquire into the more general question of why effects must have causes. Historically, the principle of conservation of energy has often fulfilled the role of a regulative presupposition representing the law of causality in physics. A particularly clean example of this is Pauli’s postulation of the neutrino—later confirmed experimentally by Cowan and Reines—which was motivated by apparent violations of energy conservation in β -decay.⁸ The methodological role of energy conservation can also be illustrated more vividly by a thought experiment: suppose that tomorrow morning Mt. Everest were to explode into pieces, leaving behind a great cloud of dust and a vast crater. Clearly, according to our knowledge of the situation, energy conservation appears to have been violated. At this point we would be left with a choice of strategies for interpreting this event: either (1) we admit our powerlessness in the face of this problem and attribute the event to the *supernatural*, i.e. to

⁸The neutrino hypothesis was proposed by [Pauli \(1930\)](#) and subsequently confirmed experimentally by [Reines and Cowan \(1953\)](#); [Cowan et al. \(1956\)](#).

magic or an act of some God, or (2) we suppose that the explosion was due to some *cause* which can in principle be assimilated into our picture of physics. In the first case we would say that energy was *created*, whereas in the second we say that it was *released*, which would thereby imply that there must have been some vast unknown reservoir of energy, perhaps contained within the mountain, which was responsible for the explosion. The scientific attitude seeks *unity* in its account of nature, and therefore will not arbitrarily accept divisions between *natural* and *supernatural* causes; all causes will be thought of as *material* and essentially on equal footing, and therefore it is appropriate to bring them all under a common concept of *energy*.

If the concept of energy is to fulfil this role in general, we must make it *essential* to the definition of energy that it be conserved once all of the relevant causes have been accounted for. In this case we should not prescribe *a priori* how energy ought to be measured but rather, fit our means of measuring it to the requirement that it should be conserved. Almost all physical theories implicitly follow this methodology, with the exception being the theory of general relativity, in which “global energy-momentum conservation becomes a contingent fact, dependent on the contingent symmetries of the spacetime” (Duerr, 2019). A rigorous treatment of energy conservation in general relativity quickly gets quite technical, and we will avoid getting deep into the weeds here since in this chapter we are primarily concerned with more general universal remarks on scientific methodology.⁹ We will discuss the issue briefly however, with the aim of elucidating the core reason why energy conservation no longer plays this regulative methodological role in this theory.

In field theories, conserved quantities arise when the dynamical laws are invariant under continuous transformations; in the case of energy, the relevant symmetry is invariance under time translation. In general relativity, such symmetries are represented by *Killing vector fields* X^a , which satisfy the condition

$$\mathcal{L}_X g_{ab} = \nabla_a X_b + \nabla_b X_a = 0.$$

Here g_{ab} denotes the spacetime metric, $\mathcal{L}_X$ the Lie derivative along the vector field X^a , and ∇_a the covariant derivative associated with the metric. The equation states that the metric is invariant under the infinitesimal diffeomorphisms generated by X^a : the Lie derivative measures how a tensor changes when “dragged along” a vector field, and its vanishing implies that the flow preserves lengths and angles, i.e. that it is an isometry. When a timelike Killing field exists globally, one can define a conserved energy associated with it. However, generic

⁹For a more detailed and historically informed philosophical treatment of energy conservation in general relativity see especially Hoefer (2000), as well as the more recent treatments by Pitts (2016) and Duerr (2019). See also Tasdan and Thébault (2024, §3) for some technical discussion of the issue.

curved spacetimes admit no such global symmetry. To say that $\mathcal{L}_X g_{ab} \neq 0$ is precisely to say that the geometry itself changes along the flow generated by X^a ; the transformation fails to be an isometry. Physically, this means there is no global time-translation invariance, and hence no basis for defining a conserved total energy in the usual sense. The failure of energy conservation in general relativity is therefore a structural consequence of treating spacetime geometry and inertial structure as dynamical rather than as a fixed background.

As we will see in later chapters (especially chapters 3 and 4), there was already a tension in classical mechanics between two distinct roles of Newton’s *absolute* space and time: on one hand (1) they are posited as homogeneous and isotropic for essentially *methodological* reasons, on the other (2) they are treated as *empirically real* given that they are what establishes the inertial structure of classical mechanics which represents an observable state of affairs in this theory (see chapter 3). General relativity inherits the second role (2) of space-time structure but denies the global homogeneity and isotropy that would be needed if we wanted the theory to carry over the *methodological* conceptions of energy and momentum conservation of classical mechanics.

What we will argue in this thesis is that it is possible to separate the *empirical* concept of inertia from the purely *methodological* concept (chapter 3), and likewise that we can separate the empirical concept of space-time geometry from the purely methodological conception of these (chapter 2). This will enable the justification of the recovery of a homogeneous Euclidean background structure in physics, introduced for methodological purposes, which we will claim does not in fact represent any empirical state of affairs. In turn this will trivially allow us to recover the methodological conception of energy conservation which becomes the image in physics of the law of cause and effect. Before we get to our arguments regarding spatial geometry in chapters 2 and inertia in chapter 3, we will briefly discuss how the same tension appears regarding the relativity principle of classical mechanics in the next section.

1.3 Relativity of space

In a pair of recent papers (Wallace, 2022a,b), David Wallace has brought attention to the important distinction between symmetries of the universe as a whole and symmetries of subsystems of the cosmos. Wallace argues against what he calls the “cosmological assumption,” which claims that “the primary way to understand a physical theory is to suppose that it models the entire universe” (Wallace, 2022a). Several problems with this assumption are identified, including that: (1) It misrepresents scientific practice, because physical theories are overwhelmingly used to model dynamically isolated subsystems with idealised bound-

ary conditions, not the universe taken as a single closed system. (2) It collapses physically meaningful symmetries into mere gauge, since interpreting a theory as describing the whole universe removes any external reference relative to which global or asymptotic symmetries could represent genuine physical differences. (3) It forces symmetry interpretation into unnecessary metaphysics, replacing concrete questions about empirical discernibility by distant systems or boundaries with abstract debates about identity, haecceitism, or counterpart relations between entire possible worlds.

What we will argue in this section is not substantially different from what Wallace has already covered in his paper, but we have a few things to add in order to relate this idea to the present thesis. The distinction between symmetries of the universe and its subsystems will be a key recurring theme throughout all of the chapters in this thesis; in the present section we will discuss this issue in the context of the distinction between the empirically motivated Galilean relativity and the relativity of motion under Leibniz shifts which is rooted *a priori*.¹⁰ Given the considerations already laid out in §1.1, we take the universe to be a *closed* system, indeed in an important sense it is the *only* closed system. Potential challenges to this view have recently been raised by Cuffaro and Hartmann (2021) as well as Ladyman and Thebault (2024), however we will not address these here.

1.3.1 Necessary relativity

The canonical example of the analytic sense of the relativity of motion is due to Leibniz. According to Newton, although absolute space and time are homogeneous, each of their points and moments are unique and exist distinctly from one another. In his fourth letter to Samuel Clarke, Leibniz famously rejects this on the basis of the identity of indiscernibles:¹¹

To say that God may cause the whole Universe to move along some arbitrary line, without otherwise changing anything in it, is again a chimerical supposition. For two indiscernible states are the same state, and consequently, it is a change that changes nothing. Furthermore, it has neither rhyme nor reason. But God does nothing without reason; and it is impossible that there should be one here.

¹⁰Note that in chapter 5 we will apply a very similar analysis to *special relativity*—which has not yet been examined with regards to the cosmological assumption in Wallace’s work—and come to similar conclusions.

¹¹See Rodriguez-Pereyra (2014) for more on Leibniz’s principle of identity of indiscernibles. Translation of: “De dire que Dieu fasse avancer tout l’Univers, en ligne droite ou autre, sans y rien changer autrement, c’est encore une supposition chimérique. Car deux états indiscernables sont le même état, & par conséquent c’est un changement qui ne change rien. De plus, il n’y a ni rime ni raison. Or Dieu ne fait rien sans raison; & il est impossible qu’il y en ait ici. Outre que ce seroit *agendo nihil agere*, comme je viens de dire à cause de l’indiscernabilité” (Leibniz, 1740, p.52-53).

Moreover, it would be *agendo nihil agere* [to accomplish nothing by acting], as I have just said, because of the indiscernibility.

Leibniz's argument does not appeal to any empirical state of affairs, however its *a priori* status is often objected to on the basis that it would not hold in an Aristotelian cosmos for instance since in that case the universe would have a preferred centre. To this we can respond that *analyticity* simply has to do with the meanings of words; Leibniz's argument is analytic in the sense that it is true provided that the "whole Universe" is taken, by definition, to refer to the sum of all things that are *physically real*. In the case of the Aristotelian cosmos this means we should include the centre point of the universe—with respect to which the natural motion of bodies is defined—as part of this totality, such that a rigid motion of the universe as a whole would also move this reference point. The statement is then true *a priori* in the same sense that all analytic statements are true *a priori*. Since these arbitrary motions of the universe as a whole do not produce any real difference they can never be observed to take place in experience. We are only able to picture these motions occurring in our minds insofar as we can *abstract* and take the perspective of an entirely passive observer with respect to whom this motion happens. In reality however, since all real observers are also part of this universe, the transformation does not take place.

This sense of the relativity of space does not therefore depend upon the particular content of our theories, and likewise it is itself essentially without content. It is not *useless* however, since as we have just seen, in the case of the Aristotelian cosmos it played a role in motivating that we recognise the centre point with respect to which natural motion is defined as something physically real. Thus the analytical sense of *relativity* may have methodological significance although it clearly lacks empirical content.¹²

In its most basic and general sense, the relativity of spatio-temporal quantities, and indeed of any quantities whatsoever in physics is due to the fact that measurement always consists in a relation between at least two given things. The position of an object for instance, can only be specified in relation to another or system of objects, such as our own bodies, or the earth. Similarly with motion, be it velocity or acceleration, and likewise lengths can only be measured as a ratio of two given things. Even quantities that may seem to have absolute values, such as temperature, are really only measured as relations. The temperature in a room is typically measured by its effect on the expansion of mercury in a tube. In this case the increase in the volume of the mercury produces a measure of length, which is compared

¹²Note that a very similar situation is true of the diffeomorphism invariance of metrical theories like general relativity. This invariance is analytic, but served to identify the metric as something that is physically real. Apart from the Leibniz shifts and diffeomorphisms, most symmetries of physical theories are not like this and are actually empirically motivated results that express physical content.

with the graduation marks on the thermometer. The graduation marks represent our conventional choice of units, and in general when measurements are made, we translate the various *real ratios* that are recorded from actual experiment or observation into ratios of the thing measured to the conventional unit. This allows us to standardise our measured quantities and thereby compare quantities measured at entirely different times, in different places and using different apparatus.

Our comparison of quantities to standard units thereby plays the role of coordination between different measured ratios, thus it is fitting that the scale of units to which measured quantities are referred is usually called a system of *coordinates*. By introducing coordinates into the world we may get a misleading impression that the numbers which we assign to certain objects, which represent perhaps their temperature, length, or volume... are properties of those objects in-themselves. Because we are accustomed to comparing directions with the Earth's axis of rotation, elevation with the ground, or size with respect any of the many examples of solid bodies whose dimensions remain faithfully in (approximately) the same proportions to one another, we forget that all of these measurements were in the first place, and can only be *ratios* and do not belong intrinsically to the bodies considered in isolation. The realisation of this reappears to us then as the discovery of the *relativity of space*, and of other systems of units.¹³

1.3.2 Empirical relativity and its elevation

Now that this has been said we need to discuss the application of relativity principles to physics, this issue in particular has caused a great amount of confusion (as [Wallace \(2022a\)](#) points out) so it is important to treat it carefully. Unlike the principle of invariance under Leibniz shifts, the principle of Galilean relativity in classical mechanics appears to be of empirical origin. A physical system, be it the inside of a ship, or a car for instance behaves the same whether it is stationary with respect to its environment (the sea or the road) or moving rectilinearly at a constant speed. This is a fact known from experience. Nonetheless, it is often said that it is only true exactly once we consider the universe as a whole. As an example, Poincaré, who had a particularly keen understanding of the relativity principle for his time, writes concerning the principle of Galilean relativity:¹⁴ “to apply the law of relativity in all rigour, it must be applied to the entire universe.” But again, if a Galilean boost were

¹³See [Weyl \(1949, p.116\)](#) for a very similar discussion of the necessity to introduce coordinates into the world and the consequences this has concerning the relativity of quantities.

¹⁴Translation of “pour qu'on puisse appliquer la loi de relativité en toute rigueur, il faut l'appliquer à l'univers entier” ([Poincaré, 1902a, p.97](#)).

to be applied to the universe as a whole this would reduce to a Leibniz shift, the invariance would be trivial and no longer susceptible or in need of empirical investigation. What is the status of the principle of Galilean relativity then? Is it just an approximate case of Leibniz shift symmetry which applies also to certain subsystems? To what degree is the principle of Galilean relativity empirical?

The original statement of the Galilean relativity principle rests on demonstrable empirical facts. In Galileo's *Dialogo* (Galilei, 1632), Salviati is responding to a common Aristotelian objection to the rotation of the earth. If the earth rotates, they say, a stone falling from the top of a tower would not fall to its base, since, during the fall, the ground will have been carried eastwards by the earth's motion. Salviati rejects this, remarking that this would also imply that a stone dropped from the mast of a moving ship does not fall at the base of the mast, which can be disproven experimentally. The ship's motion, Salviati argues, has no effect on the stone's fall since the stone accompanies the ship as it moves horizontally. He generalises this to claim the lack of any observable difference whatsoever in the cabin of a uniformly moving ship. He describes how the dripping of water drops into a vessel, the flight of insects, the swimming of fish in a bowl and the throwing of a ball between two people, are all not in the least affected by the uniform motion of the ship.

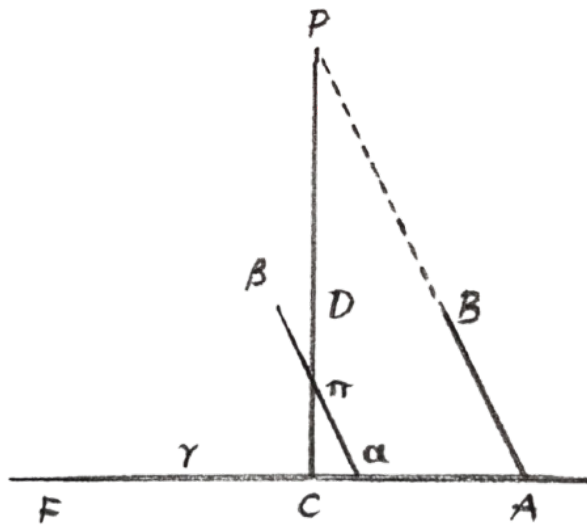
However, empirical facts could never prove the principle rigorously, besides, we know that it does not in fact hold exactly, since no system can truly be causally isolated from its environment. What is it then that permits us to say that this principle holds exactly? This is of course due to an *abstraction*, we abstract away from the small variability of the environment and thus assume it to be homogeneous in time and space. There is then by assumption, no energy exchange between the system and its environment, and we call the system *closed* provided that it meets this criterion. Now, the principle of Galilean relativity follows *analytically* and *a priori* as part of the definition of a closed system. In this way, the empirical question is no longer whether the Galilean relativity principle holds, rather, the question shifts to whether the given system under consideration sufficiently approximates a *closed system*, such that it will be useful to apply these ideas.

Therefore, although Galileo's relativity principle was originally grounded in an empirical result—namely, the observed invariance of certain systems under *active* transformations of their state of motion relative to their environment—this empirical grounding is obscured once the environment is idealised as homogeneous. By homogenising the environment, its concrete physical features are effectively removed and it is treated as what Newton later called *absolute space*. In this move, the invariance of a system under Galilean boosts is no longer understood as a *dynamical* consequence of its interaction with a roughly homogeneous en-

vironment, but instead as a *kinematical* feature of the spacetime structure presupposed by classical mechanics. Galilean boosts can now be treated as merely *passive* transformations between coordinate descriptions. Newton's introduction of absolute space and absolute time enables precisely this shift: from empirically observed invariance under changes of state of motion to an analytical principle of relativity that follows from the very definition of a closed system. The relativity principle thus ceases to be grounded in experience and instead becomes a consequence of the assumed background structure within which dynamics is formulated.

Whereas the principle of invariance under Leibniz shifts, which follows analytically from the definition of the universe as a whole and was not motivated by empirical considerations at all, the principle of Galilean relativity was motivated by its approximate applicability to many real physical systems. Experience teaches us that while such (approximately) "closed" systems are dynamically invariant under uniform velocity boosts relative to their environment, they are not invariant under transformations involving *accelerations* relative to that environment. Crucially, then, even after homogenising the environment, the principle of Galilean relativity continues to rely on the *existence* of an environment in order to establish the limits of its applicability. Classical mechanics therefore presupposes a distinction between subsystem and environment that cannot be eliminated without undermining the empirical content of the theory. For this reason, classical mechanics—inasmuch as it relies on these concepts—is not suitable for application to the universe as a whole. And if the laws of classical mechanics apply only to limited subsystems under specific idealising abstractions, rather than to the universe in its entirety, then they cannot be regarded as genuinely *universal* laws.

In the next chapter, we consider the consequences that this distinction between symmetries of subsystems and symmetries of the universe as a whole has with regards to debates concerning the foundations of geometry. We will travel back in time to the late 19th century, when these debates were live, and defend a somewhat contrarian view according to which Euclidean geometry will be given a more central role than is normally acknowledged today.



HOMOGENEOUS SPACE

Shall we then posit, if you like, two kinds of beings, the one visible, the other invisible?

Plato, *Phaedo*

What is Euclidean geometry grounded on?

Introduction

Opening

There is a tension between the everyday philosophical sense of terms such as “space” and “time” and the notion of “spacetime” as used in modern physics. In the latter, spacetime is taken to be *dynamical*—that is, it participates in causal interactions—which blurs the traditional distinction between *space and time* on the one hand and *matter* on the other. In the classical picture, space and time formed a passive backdrop against which material events unfolded, whereas matter was characterised precisely by its participation in causation; dynamical spacetime collapses this distinction. Consider the notion of *expansion*, this is a *spatial concept* which means that a thing increases in size. Strictly speaking, things may be said to expand with respect to one another, but nothing can be said to expand *in-itself*, that is, without an external reference. Nonetheless, when a thing expands or otherwise moves in any way, it is useful to be able to isolate it in our thoughts from its surroundings. In the interest of this abstraction, we introduce the concept of *space*, with respect to which we imagine a

thing moved, expanded or shrunk. Space thereby allows the isolation of spatial phenomena so that a single system can be analysed without having to consider the endless network of connections in which it is actually embedded. Space is therefore not a real thing at all; and similar things can be said concerning time.

In modern cosmology, it is said that space is “expanding” over the course of the epoch. What are we to make of this? Surely, insofar as *expansion* is a spatial concept, we are implicitly imagining a further abstract space concept with respect to which our physical space expands. And again, if we were to say that this second space expands or otherwise moves, we would represent this motion with respect to a further space concept, and so on *ad infinitum*. Therefore our spatial concepts, taken in the abstract, always ultimately rest on a notion of an entirely *passive* space, and it might be more appropriate to say that planets, stars and our bodies are *shrinking* in space, rather than that space expands. This passive conception of space has been abandoned by modern physics on the grounds that it introduces *background structure* that places unwarranted constraints on physical possibilities. One of the central aim of this thesis is to show that this was mistaken, and indeed that on the contrary, it is the *abandonment* of this passive concept of space (and time) that has constrained fundamental physics and philosophy in the last century by introducing unnecessary conceptual confusion and giving license to various kinds of ungrounded speculation.

The present chapter traces the forgotten history of this alternative, *passive* doctrine of space, and defends it against certain fallacious objections that have not been properly addressed before. Although it was not initially the intention of this chapter, what we find is that by recovering this concept we at the same time provide principled philosophical foundations for the mathematical axioms of Euclidean geometry, and bring some clarity also to the relationship between mathematics and natural science.

Context and roadmap

Following the discovery of the mathematical possibility of non-Euclidean geometries by [Lobachevsky \(1829a,b\)](#), the “problem of space,” that is, the problem of determining which among the available geometries should be chosen as that which represents the space of our physical world, drew the attention of pre-relativistic physicists, philosophers and mathematicians throughout the 19th century. The well known contributions of [Helmholtz \(1870, 1876\)](#), [Riemann \(1854\)](#), [Poincaré \(1898\)](#) and [Lie \(1893\)](#) brought together an assemblage of empiricist and neo-Kantian ideas which would give birth to new perspectives, such as geometrical conventionalism. On one hand, neo-Kantian strands of thought suggested that the geometry of space should be something regarded as distinct from the material contents therein; on

the other hand, empiricists argued that geometry is only an abstraction from the observed behaviours of material bodies. By considering space as a *condition for the possibility of measurement* rather than a condition for the possibility of experience, Helmholtz developed a form of *empiricist neo-Kantianism*, which had a profound and enduring influence on later neo-Kantians and logical positivists (Ryckman, 2003; Friedman, 2001, 2009).

There are, however, some problems with this standard 19th century approach to the problem of space: Helmholtz’s “conditions for the possibility of measurement” rely on an approximately physically instantiated notion of rigid bodies, which was undermined by later developments in special and general relativity. Whereas Poincaré’s lesser emphasis on empiricism arguably avoids this issue, the Helmholtzian views which he adopts appear to be inconsistent with certain other aspects of his philosophy of space (see §2.3.3).

The aim of this chapter is to introduce English-speaking audiences to Joseph Delboeuf’s alternative approach to the space problem, which has been overlooked by recent accounts such as Dewar and Eisenthal (2020); Heinzmann (2001); Biagioli et al. (2016). Although Delboeuf’s work engages deeply with the Kantian tradition, he is not even mentioned in recent neo-Kantian texts in the philosophy of physics such as Friedman (2001, 1983); Ryckman (2005); Bitbol et al. (2009); Biagioli et al. (2016). The most significant acknowledgement of Delboeuf’s contribution since the mid 20th century is by Torretti (1978). But while Torretti recognises that Delboeuf was “probably the earliest philosophical writer who had first-hand acquaintance with the works of Lobachevsky” (p.153), and acknowledges Delboeuf’s ideas as “interesting” (p.298), he ultimately gives them a disfavourable verdict, and even revives a fallacious objection to Delboeuf’s philosophy of geometry which had formerly been touted by Russell and Poincaré (see §2.3.2).

Delboeuf defended the apriority of Euclidean space on the basis of the relativity of magnitudes. Although he developed his ideas independently, the central argument dates all the way back to Wallis (1696) (see §2.2.2.1), and has recently been revived by Čulina (2020, 2018, 2023). Delboeuf is unique in that he gives this notion of the relativity of magnitude the status of “first postulate,” thereby attempting to erect geometry upon new foundations. Delboeuf rejected the prior systems of geometry of Euclid, Lobachevsky, Mill and Kant on the basis that none of these had rationally deduced the postulates or axioms taken as primary. If geometry is the study of determinations in space, Delboeuf claims that our concept of space is characterised by the notion of “homogeneity” by which he means not only that the properties of space are the same in all its parts, but also that these are independent of its size (Delboeuf, 1860, p.126). In other words, in geometry we must be able to consider the shape and size of figures independently. For Delboeuf, this notion of homogeneity is not an empirical fact

about some “real space” (Delboeuf, 1893), rather, it is the result of the process of abstraction which the intellect undertakes in its effort to describe nature in terms of universal laws, i.e. to step outside of the contingency of given (real) material objects.

While Delboeuf’s philosophy of science is a fascinating topic in its own right, we will not dive too deeply into it here (we have discussed some aspects already in chapter 1). Our discussion in this chapter will be mostly limited to situating this author’s ideas concerning geometry within their historical context in the 19th century and drawing parallels with ideas of other philosophers and mathematicians. Among the novel arguments and findings made in this essay: (1) We uncover a convergence of thought between Delboeuf’s approach and an essay by Leibniz titled *Uniformis locus* which has only been made available relatively recently by De Risi (2005, 2007) (see section 2.2.3). (2) we elucidate the fundamental difference between Helmholtz’s empirically realisable motions, and Delboeuf’s symmetry conditions, and argue that only the latter are relevant to the Kantian conception of space (see section 2.3.1). (3) We refute Bertrand Russell’s enduring ‘*relative angles*’ objection to Delboeuf’s notion of the relativity of magnitudes (see section 2.3.2). (4) We propose that Poincaré’s “principle of relative motion” and his more general conception of the mathematical infinite can be grounded in Delboeuf’s philosophy (see section 2.3.3). We conclude in section 2.3.3 with some suggestions concerning the relevance of these findings to the methods of modern physics. Since our findings here demonstrate the incompatibility of the non-Euclidean geometries with the relativity of magnitude, this work may be seen as complementary to the recent work of Eisenthal (2026) which brings attention to the incompatibility of non-Euclidean geometries of constant curvature with the relativity of velocity.

Given that Poincaré’s philosophy of space has some significant similarities to Delboeuf’s, is better-known than Delboeuf’s, and represents in some sense a lucid synthesis of many of the findings that were made by investigators in the late 19th century, we have chosen in section 2.1 to set the scene by discussing certain key aspects of Poincaré’s philosophy of space that do not directly involve the question of geometry, but which are nonetheless deeply relevant to later discussions.

2.1 Poincaré on space

While Poincaré is quite famous today for his geometrical conventionalism (which will be discussed in section 2.2.1), other aspects of his philosophy of space and mathematics are equally significant and will help provide context for our subsequent discussion of geometry. In this section, we will briefly cover some of these aspects: (1) Poincaré’s revision of the Kantian

notion of synthetic *a priori* reasoning, (2) Poincaré’s empiricist account of the distinction between changes of state and changes of place, (3) the distinction between empirical objects and their mathematical idealisations.

2.1.1 Mathematical reasoning

By the late 19th century, Kant’s claim that mathematics contains synthetic *a priori* propositions was being subjected to severe criticism by the logicians. Frege and Russell attempted to show that all true mathematical statements could be derived from a basic set of concepts defined terminologically. Thus all mathematical truths would be reduced to logic, without need of intuitions.¹ Poincaré, on the other hand, did not abandon the notion of synthetic *a priori* reasoning, but rather, attempted to revise it.²

Poincaré develops his conception of synthetic *a priori* knowledge in the first chapter of *Science and Hypothesis*, titled *On the Nature of Mathematical Reasoning*. Kant had claimed that the basic propositions

of arithmetic, such as $5 + 7 = 12$, are *synthetic*, since nowhere in the concept of the sum of 5 and 7 is contained the concept of 12. Something additional is needed for Kant, that is, an intuition of space in which the two quantities can be placed side-by-side with one another, and the operation of summation can be accomplished (Hatfield, 2004, p.18). This claim is controversial, in part because it depends greatly upon how we define things. It is not too difficult to define our numbers in such a way that basic propositions of arithmetic, such as $5 + 7 = 12$ or $2 + 2 = 4$ appear as analytic truths. Unlike Kant, Poincaré does not argue that there is anything synthetic in these basic propositions; instead, he claims that the so-called “demonstrations” of these sums are really only analytic “verifications.” However, these trivial verifications are not the true subject matter of mathematics, on the contrary, Poincaré (1913, p.33) insists:

It may even be said the very object of the exact sciences is to spare us these direct verifications.



Figure 2.2: Henri Poincaré.

¹Note that for Frege, this meant that mathematics would be purely analytic, whereas Russell viewed logic as synthetic.

²See (Folina, 1992) for an in depth discussion of Poincaré’s neo-Kantianism.

The essence of mathematics, for Poincaré, lies in the ability to generalise across an infinity of cases, using what is called “reasoning by recurrence,” or “mathematical induction.” The basic structure of a proof by induction proceeds as follows:

1. The theorem is proven for $n = 1$.
2. It is shown that if it is true for $n = a$, it must be true for $n = a + 1$.
3. Therefore we know that it is true for $n = 2$, and likewise $n = 3, 4, 5, \dots$. By induction, we have shown that it must be true for all $n \in \mathbb{Z}$.

This enables one to make generalisations about some theorem over an infinity of cases.³ It is in this possibility of reasoning by recurrence—which Poincaré calls “the mathematical reasoning *par excellence*”—that he locates the true *synthetic a priori* judgement (Poincaré, 1913, p.39):

This rule, inaccessible to analytic demonstration and to experience, is the veritable type of the synthetic *a priori* judgment. [...] Mathematical induction, that is, demonstration by recurrence, [...] imposes itself necessarily because it is only the affirmation of a property of the mind itself.

2.1.2 Empirical ground of space

In chapter IV of *Science and Hypothesis*, titled *Space and Geometry*, Poincaré takes up the perspective of a naive investigator attempting to make sense of the world present to his senses while lacking any pre-conceived notions about how these ought to be organised and interpreted. How do we come to the idea of space, and in particular, how do we distinguish between changes of position and changes of state (such as changes in colour)? Poincaré presents this problem as follows (Poincaré, 1913, p.70):

Whether an object changes its state or merely its position, this is always translated for us in the same manner: by a modification in an aggregate of impressions. How then could we have been led to distinguish between the two?

His solution is rather straight forward:

It is easy to account for. If there has only been a change of position, we can restore the primitive aggregate of impressions by making movements which replace us

³In section 2.3.3, we will argue that it is not the notion of infinity, but the *a priori* concept of symmetry or mathematical equality that makes reasoning by recurrence possible.

opposite the mobile object in the same relative situation. We thus correct the modification that happened and we reestablish the initial state by an inverse modification.

A change in spatial position is distinguished from a change in state by the possibility of performing the reverse operation by means of the correlative movement of our own bodies. In the case of sight, this movement may also be performed by the “appropriate movement of the eyeball.”

Now the possibility that certain “aggregates of impressions” may be restored through our correlative movements depends upon the existence of “solid bodies,” i.e. bodies which retain the relations among their parts while changing position with respect to us. Indeed, it is the observation of solid bodies, Poincaré argues, that has taught us to distinguish between changes of state and changes of position, such that, he concludes: “*if there were no solid bodies in nature, there would be no geometry.*”

2.1.3 Mathematical idealisation

While this empiricist account of the origin of geometry is persuasive, it does not lead directly to the complete *mathematical* notion of geometric space. While I may observe that as a body recedes from me, I can restore its original size by approaching it once more, I can never infer from experience that this will continue to be true if the body recedes to an arbitrarily large distance. Should my concept of space therefore be limited to distances for which the compensation is practically realisable? Clearly, when we imagine objects in geometrical space, we do not limit ourselves to distances which our bodies are capable of traversing. Rather, we consider space as potentially infinite in extent, and thereby we implicitly imagine an *idealised* observer capable of visiting all parts of this space at will to perform the necessary compensatory motions. The space of geometry differs in this respect from the empirical condition that motivated its invention.

It is even more clear that an *idealisation* is involved when we place our representations in space-time.⁴ In space-time we imagine bodies, extended in time as well as in space, and thereby we implicitly invoke the possibility of an idealised observer that may travel to the different parts of this space-time and measure it with ideal rulers and clocks. But these motions are by no means physically realisable. It is not even possible to visit distant points which seem to lie in our plane of simultaneity, let alone to travel into the past. If geometry can, in

⁴The present comments concerning space-time are not drawn directly from Poincaré’s works, however, they are inspired by his discussions of the issues.

any sense, be said to have an empirical origin, it must have departed from its empirical roots in order to encompass the notion of time.

A second issue concerning the difference between the empirical and mathematical concepts of space is that no empirically given “natural solid” is absolutely rigid. When we look at any given body close enough we find motion and change in its structure, modifications due to heat, the vibrations of the constituent particles, and so on. These contingencies make it impossible for us to use empirical objects as standards for the definition of a mathematical space. As [Poincaré \(1913, p.79\)](#) puts it:

Geometry would be only the study of the movements of solids; but in reality it is not occupied with natural solids, it has for object certain ideal solids, absolutely rigid, which are only a simplified and very remote image of natural solids.

To reach the mathematical concept of space, we must substitute our empirical notions of solid bodies with their *ideal* counterparts; and in so doing we substitute the empirically grounded, physical concept of space—which we have no definite knowledge of—with the pure, mathematical concept of space, of which we have absolute knowledge *a priori*. Once we have performed this substitution of the empirical objects with their ideal counterparts we make it possible to apply the mathematical reasoning which Poincaré characterises as *synthetic a priori*: While we may attempt to infer by *physical* induction that a rock remains the same wherever it is placed in relation to the other bodies of the universe, this knowledge will only ever be approximate, contingent and subject to the possibility of being refuted by experience. However, if my object is not a rock, but an ideal rigid body in Euclidean space, I can say with absolute certainty that it will retain the relations amongst its parts no matter where it is placed in this space.

2.2 Geometry of space

In the first part of this section, we briefly recount the well-known history of Helmholtz and Riemann’s canonical 19th century approach to the problem of spatial geometry which was based on the notion of the free mobility of bodies ([2.2.1](#)). In the latter parts ([2.2.2](#) and [2.2.3](#)), we discuss the alternative approach of Delboeuf and some others which is based on the idea of the relativity of magnitudes.

2.2.1 The axiom of free mobility

The repeated failures to prove the necessity of Euclid’s fifth postulate on the basis of the first four culminated in Lobachevsky’s construction of a self-consistent geometry based on the denial of the parallel postulate ([Lobachevsky, 1829a](#)). Just as theorems concerning shapes in Euclidean geometry can be studied and proven, a corresponding set of theorems pertaining to Lobachevsky’s hyperbolic geometry can be proven mathematically. Which set of theorems is, then, true of our space? This glaring ambiguity at the level of mathematics prompted various thinkers, including Riemann and Helmholtz, to seek an empirical ground for the validity of Euclidean geometry, or lack thereof.

2.2.1.1 Riemann

Following from Gauss’ work on the geometry of curved surfaces, Riemann developed the general concept of a “multiply extended manifold” whose curvature may vary from point to point. Since the metrical properties of this manifold should be grounded in empirical facts, this manifold needed to be susceptible of measurement, which implied the mobility of certain quantities in space ([Riemann, 1854](#)):

Measuring involves the superposition of the quantities to be compared; it therefore requires a means of transporting one quantity to be used as a standard for the others.

The first hypothesis that Riemann explores is that “the length of lines is independent of their configuration, so that every line can be measured by every other.” This allows for a broad class of possible geometries that we now know as *Riemannian geometries*. Riemann also remarked that if we assume—not only that lines are independent of configuration—but also that the bodies are so, then:

it follows that the curvature is everywhere constant, and the angle sum in all triangles is determined if it is known in one.

2.2.1.2 Helmholtz

Helmholtz placed great emphasis on this latter idea, arguing that the mobility of rigid bodies was a necessity for the possibility of measurement, and concluded that only the spaces of constant curvature could properly be considered as geometry ([Helmholtz, 1866, 1870](#)).⁵

⁵For some more in depth discussion of Helmholtz, as well as Helmholtz’s relationship with Weyl, see [Stemeroff \(2024\)](#).

Helmholtz acknowledges, however, that the natural bodies apparent to observation are never identical to our idealisations of these. In his latter paper ([Helmholtz, 1870](#)), Helmholtz approaches something like a Kantian view, according to which the notion of a geometric figure would be “formed independently of actual experience.” However, Helmholtz insists that:

we should have to maintain that the axioms of geometry are not synthetic propositions, as Kant held them: they would merely define what qualities and deportment a body must have to be recognised as rigid.

Rather than being a *condition for the possibility of representation*, like Kant thought, space and its geometry become *conditions for the possibility of measurement*, as it were.⁶ Helmholtz finishes his essay by settling on a conventionalist stance according to which: if taken apart from mechanical propositions, the axioms of geometry “constitute a form into which any empirical content whatever will fit.” However, this is not only true of Euclid’s axioms, but also of the axioms of spherical and pseudospherical geometry ([Helmholtz, 1870](#)).⁷

2.2.1.3 Poincaré

Following Helmholtz, Poincaré further developed this conventionalist standpoint ([Poincaré, 1898, 1902b](#)). According to Poincaré any empirical assertion of some given geometry over another is founded on a “disguised definition”; It is a *convention*, and only an appeal to some extra-empirical theory virtue such as “*simplicity*” may allow us to decide between conventions. Poincaré’s preference for conventionalism, rather than a purer form of Kantianism was grounded in his *group-theoretic* approach to the problem. For Poincaré space is not a form of the sensibility, since “sensations by themselves have no spatial character,” rather the “sensible space” must be a *form of our understanding*: “it is an instrument which serves us not to represent things to ourselves, but to reason upon things” ([Poincaré, 1898](#)).

However, the geometry of this form cannot be determined *a priori* for Poincaré, since there are a multiplicity of conceivable forms that we may use to reason on things. These are the *group structures*, which are the objects of study of mathematics. The various transformations on a Euclidean space are only one among a multitude of possible group structures that may be employed if experience warrants it. From this point of view, nothing truly distinguishes Euclidean geometry from the alternatives apart from its simplicity. The empirical observation that it is at least approximately instantiated in the observable behaviours of

⁶This view is elaborated by [Russell \(1898\)](#).

⁷The “pseudospherical” geometry is Helmholtz’s term for the hyperbolic space of Lobachevsky and Bolyai.

natural solids and rays of light does not even distinguish it from geometries of very low curvature.

Despite claiming the conventionality of geometry, Poincaré devotes very little attention in his work to geometries of changing curvature. The bulk of Poincaré's discussion of the conventionality of geometry in part II on *Space* (chapters III, IV and V) of *Science and Hypothesis* concerns the geometries of constant curvature. The reason for his neglect of the former is given in the one passage in which they are briefly discussed (Poincaré, 1913, p.63):

most of these definitions are incompatible with the motion of a rigid figure, [...] These geometries of Riemann, in many ways so interesting, could never therefore be other than purely analytic and would not lend themselves to demonstrations analogous to those of Euclid.

Like Helmholtz, Poincaré rejects Riemann's geometries of changing curvature on the basis that they are incompatible with the motion of rigid figures. However, for Poincaré the crucial point here is that this incompatibility would undermine the very aim of mathematics. Since the geometrical properties of figures in a space of changing curvature would depend upon the value of the curvature from place to place, it would become hopeless to make those *inductive generalisations* that Poincaré views as so central to mathematical reasoning. Particular propositions about these geometries would not be synthetic, but *analytic*, since they would depend upon how the curvature is defined to change from point to point.

2.2.2 The relativity of magnitudes I: before Delboeuf

Helmholtz and Poincaré's refutations of Kant's Euclidean *a priori* rest essentially on a single claim: that the geometries of constant positive or negative curvature of Riemann and Lobachevsky respectively may just as well serve as *forms* into which the empirical content of our sensations may be placed. These geometries, they say, have just the same right to be viewed as "transcendentally given" as that of Euclid (Helmholtz, 1870).

But is there not some characteristic of Euclidean space, beyond its mere "simplicity," that sets it apart from those of constant non-zero curvature? Indeed there is. It is that *Euclidean space remains similar to itself at different scales*. In other words, we may *zoom into* some part of this space without changing anything about it. Thinking in terms of figures, rather than space itself, Euclidean space is the only space which allows for the possibility of incongruent *similar* figures (i.e. figures which differ in size but possess the same shape). All other geometries necessarily fail this test since curvature is a scale-dependent property of space. For instance, the sum of the angles of a triangle placed in Lobachevsky's hyperbolic space will shrink

as the triangle is enlarged with respect to this space; therefore, two equilateral triangles of different sizes will not be similar.⁸

This criterion, by which Euclidean space can be uniquely determined, seems to have only been considered a handful of times in the history of geometry. For us, it is easy to become conscious of it, since we have knowledge of non-Euclidean geometry, and we can thereby easily identify what characteristics distinguish Euclidean geometry by contrast. Prior to the development of the theory of non-Euclidean geometries however, it would have been more difficult to deduce the relationship between the absence of an absolute scale and the parallel postulate.

2.2.2.1 Wallis (1663)

The relationship between the possibility of similarities and the parallel postulate was first recognised by the English mathematician John Wallis, Savilian Chair at Oxford, in 1663 (although his proof was published in 1696 ([Wallis, 1696](#))), over a century prior to the development of the theory of non-Euclidean geometries. Wallis attempted to show that Euclid’s fifth postulate can be deduced from ideas which are self-evident. Wallis’s proof is in two parts:⁹

(1) Firstly, Wallis demonstrates that Euclid’s fifth postulate is identical to the possibility of constructing *similar* triangles, that is, triangles which have the same shape though they differ in size.

(2) Secondly, Wallis provides a metaphysical argument for the possibility of transformations by similarity. This is due to the distinction between quality and quantity. Whereas, for Wallis, the size of a figure is a *quantity*, the shape of a figure belongs to the category of *quality*. Being different categories, each of these two must be able to vary independently of the other.



Figure 2.3: John Wallis.

⁸Throughout this chapter, we use the term “similar” in the geometrical sense to denote figures of different sizes which have the same shape; we also sometimes refer to shape-preserving scale-transformations as “transformations by similarity” or simply “similarities.”

⁹The original text is written in Latin by Wallis. We will not go through the details of Wallis’s demonstration here; the reader can consult this in [Hill \(1925\)](#) for a reconstruction of the proof in English. See also [Therrien \(2020\)](#) for a more detailed discussion of Wallis’s proof.

Wallis's proof would be circulated around Europe in the 18th century. Notably, it is discussed in the doctoral dissertation of Georg Simon Klügel (1763), which surveyed roughly thirty attempts to prove Euclid's parallel postulate.¹⁰ Klügel argues that Wallis's proof is circular since his postulate is equivalent to Euclid's (Klügel, 1763, §X), and since then it has generally been regarded as yet another failed attempt to prove the parallel postulate.¹¹

2.2.2.2 Carnot and Laplace

The next mention of this relationship between the possibility of similar figures and the parallel postulate appears in a note in Carnot's *Géométrie de Position* (Carnot, 1803, p.481):¹²

The theory of parallels depends on a more primary notion which appears to me to be of the same order of clarity as that of the perfect equality or superposition of figures; this is the notion of *similarity*. It seems to me that it can be regarded as a self-evident principle, that that which exists as large, such as a ball, a house, a drawing, can be made in small, and vice-versa; by consequence, whatever figure we may imagine, it is possible to imagine others of all sizes and similar to the first, that-is-to-say of which all the dimensions have amongst themselves the same proportions as that of the first. This notion once admitted, it is easy to establish the theory of parallels, without recourse to the notion of infinity.

Though Carnot asserts that the proof is easy, he does not derive it. Moreover, he does not cite Wallis's proof, so it is not clear whether or not he learned of it from there.

This idea is also mentioned by Laplace in passing amid a discussion of the scale-invariance of the inverse-square law of gravitational attraction (Laplace, 1835, p.471-472). Likewise in a footnote we find:¹³

¹⁰I thank José Ferreirós for recently bringing Klügel's dissertation to my attention.

¹¹See for instance Jammer (2013, p.145) for a characterisation of Wallis's proof as a failed attempt to prove the parallel postulate. Wallis's proof is also mentioned by Mach (1906, p.119) who then adds that "a modern geometer, Delboeuf, deduced from the assumption of similitude the entire Euclidean geometry."

¹²Translation of: "La théorie des parallèles tient à une notion première qui me paroît être à-peu-près du même ordre de clarté que celle de l'égalité parfaite ou de la superposition; c'est la notion de *similitude*. Il me semble qu'on peut regarder comme un principe de première évidence, que ce qui existe en grand, comme une boule, une maison, un dessin, peut être fait en petit et réciproquement; que part conséquent, quelque figure qu'on veuille imaginer, il est possible d'en imaginer d'autres de toutes grandeurs et semblables à la première, c'est-à-dire dont toutes les dimensions aient entre elles les mêmes proportions que celles de la première. Cette notion une fois admise, il est facile detablir la théorie des parallèles, sans recourir à la notion de l'infini." (Emphasis in original).

¹³Translation of: "La perception de l'étendue renferme donc une propriété spéciale, évidente par elle-meme et sans laquelle on ne peut rigoureusement établir les propriétés des parallèles. L'idée d'une étendue limitée, par exemple du cercle, ne contient rien qui dépende de sa grandeur absolue. Mais, si nous diminuons, par la pensée, son rayon, nous sommes portés invinciblement à diminuer dans le meme rapport sa circonférence et

The perception of extension contains a special property, self-evident and without which we cannot rigorously establish the properties of parallels. The idea of a limited extension, for example of the circle, contains nothing which depends on its absolute size. But, if we diminish, by thought, its radius, we are inevitably inclined to diminish in the same ratio its circumference and the sides of all the figures inscribed. This proportionality appears to be a much more natural postulate than that of Euclid; it is curious to find it again in the results of universal gravity.

Once again, the work of Wallis is not mentioned.

2.2.3 The relativity of magnitudes II: Delboeuf

Joseph Delboeuf was a Belgian psychologist, mathematician, and philosopher. Although he spent the bulk of his career as a psychologist, he obtained a doctorate in philosophy at the University of Liège and was deeply concerned with the foundations of geometry in his youth. While he was studying philosophy at the University of Liège, his friend and colleague François Folie had attempted to prove the necessity of Euclid's parallel postulate. Folie's professor had pointed out the questionable proposition involved, and this disappointment led Folie to abandon the endeavour (Delboeuf, 1895, p.346). Delboeuf, on the other hand, did not abandon his youthful ambitions, and some years later, while on a fellowship at the University of Bonn, published a radical reconception of geometry that would place Euclidean intuitions surely at its foundation (Delboeuf, 1860).¹⁴



Figure 2.4: Joseph Delboeuf in 1891.

The difficulty with Euclid's fifth postulate draws investigators into a labyrinth from which they can only escape by a total revolution in thinking about geometry. In this respect, two pathways are available; we may either (1) seek new foundations for Euclidean geometry, or

les côtes de toutes les figures inscrites. Cette proportionnalité me paraît être un *postulat* bien plus naturel que celui d'Euclide; il est curieux de la retrouver dans les résultats de la pesanteur universelle."

¹⁴François Folie would go on to become a distinguished astronomer and director of the Royal Observatory of Belgium in Uccle.

(2) we should absorb Euclid's geometry into a more general conception, of which Euclid's is only a special case. The second approach, that of the "neo-geometers," has been favoured by history. Delboeuf, on the other hand, embarks upon the first project (Delboeuf, 1894b, p.122). Lobachevsky's discovery, for Delboeuf, did not disprove the necessity of the parallel postulate in geometry; rather, it only served to help us better understand what our Euclidean intuitions are founded on.

2.2.3.1 The homogeneity of space

In his *Prolégomènes Philosophiques De La Geometrie Et Solution Des Postulats* (henceforth *Pro-légomènes*) of 1860, Delboeuf independently rediscovers the insight of Wallis, Carnot and Laplace that the "mutual independence of shape and size" implies the Euclidean nature of space (Delboeuf, 1860). He elevates this insight to the "first postulate" of geometry and argues, much like Wallis, that it is in philosophy and metaphysics, not geometry, that we must seek its justification. This justification is found, for Delboeuf, to be implicit in the concept we have of space, namely, that it is "homogeneous." Homogeneity, for Delboeuf, is a more restrictive criterion than what this word usually means today. Today, we typically define a manifold M as "homogeneous" if all points stand in the same relation to the whole. This can be specified mathematically in terms of the isometry of the metric g under the group action of the translation group T . Delboeuf calls this property "isogeneity" rather than homogeneity, and recognises that it holds for instance of the circumference of a circle, the surface of a sphere, as well as non-Euclidean geometries of constant curvature such as the hyperbolic geometry of Lobachevsky and the spherical geometry of Riemann. The word "homogeneity," for Delboeuf, is reserved for an extension which is *self-similar*, that is to say, invariant under scale transformations; in present day language we would call this *rescaling conformal isometry*: the group action ϕ transforms the metric g such that $\phi^*g = \Omega^2g$, where Ω is constant across space. It can be shown that a geometry (M, g) , which is isometric under translations as well as rescalings must be Euclidean (Wald, 2010).

For Delboeuf, the assertion of the homogeneity of space is not some habit of thought taught by the regularities of our experience, as an empiricist such as Mill might see it, on the contrary, Delboeuf sees the space and time of our experience as heterogeneous (Delboeuf, 1860, p.41)¹⁵:

Any being does not remain the same when we transport it from here to there;

¹⁵Translation of: "Un être quelconque, ne reste pas le même quand d'ici on le transporte là ; aujourd'hui il est différent de ce qu'il était hier et de ce qu'il sera demain."

today it is different from what it was yesterday and from what it will be tomorrow.

Instead, the notion of space's homogeneity follows from the *method* of science, it is the consequence of the effort of our intelligence to uncover the unity underlying the multiplicity given in phenomena¹⁶:

It is from this vast whole that intelligence must seek the laws; it is this infinite variety of facts that she must reduce to a few general facts. For this, she resorts to a first abstraction; she supposes time and space to be perfectly homogeneous, that is to say, as we will explain later, indefinitely and arbitrarily divisible into parts which differ only by their magnitude.

Through this process of abstraction, of “ideal *homogeneification*” of space and time, we make the world appear as “inert” and “infinitely divisible” (p.42). But it is only insofar as we carry out this abstraction that we are able to subject this world to universal laws. In bk. I, ch. 2 of *Prolégomènes*, Delboeuf describes how the object of science changes across multiple consecutive processes of abstraction. Zoology and botany study organisms insofar as they are instances of their species, the physical and chemical sciences consider bodies as inert, they are no longer unique individuals but assemblages of some basic substances such as elements which are each universal. Through a further abstraction we reach the mathematical sciences: mechanics considers bodies in terms of the actions they exert on one-another, which we call *force*. Now if we abstract further, away from the changes and motions which result from the inequality of forces, Delboeuf writes (p.67)¹⁷:

the universe is reduced to an ensemble of figures. The science of these figures is called geometry.

All figures are endowed with a shape and a size. Indeed geometry can be studied from either perspective. Delboeuf gives the name “*synthetic geometry*” to the study of the figure “in-itself,” that is, if we “bring back questions of size to questions of shape.” Conversely, he calls “*analytic geometry*” the study of shapes in terms of relations of magnitudes, for instance “the shape of a figure is given in terms of the length of the coordinates of each of its points” (p.69). The postulate of the mutual independence of shape and size is thus inherent to synthetic

¹⁶Translation of: “C’est ce vaste ensemble dont l’intelligence doit chercher les lois c’est cette infinie variété de faits qu’elle doit ramener à quelques faits généraux. Pour cela, elle a recours à une première abstraction; elle suppose le temps et l’espace parfaitement homogènes, c’est-à-dire, comme nous l’expliquerons plus tard, indéfiniment et arbitrairement divisibles en parties qui ne diffèrent que par leur grandeur.”

¹⁷Translation of: “l’univers se réduit à un ensemble de figures. La science des figures est la géométrie.”

geometry, since it is assumed in the notion that one can study shape in itself. As well as the homogeneity of space, Delboeuf identifies this *first postulate* with the recognition of the relativity of magnitude (p.129)¹⁸:

To say that space is homogeneous is at bottom nothing but the assertion that there is no absolute magnitude.

2.2.3.2 Leibniz and the definition of the straight

Interestingly, the concepts of homogeneity and isogeneity had previously been discussed by Leibniz under the names of “self-similarity” and “self-congruence” respectively. In an unpublished essay titled “*Uniformis locus*” which has so far been discussed solely in the work of Vincenzo de Risi (De Risi, 2005, 2007, 2015), Leibniz includes the definitions of the plane, the straight and space in terms of their self-similarity¹⁹:

A locus can be called uniform or self-congruent if its congruently bounded parts are congruent. On the other hand, a locus is self-similar if its similarly bounded parts are similar. The only self-similar loci are the straight line, the plane, and space itself. Uniform loci include all self-similar loci and, besides, others—that is to say, among the lines, the arc of a circle and the cylindrical helix and, among the surfaces, the spherical and the cylindrical ones.

Indeed Leibniz even recognises the same concept of “homogeneity” as Delboeuf, though he does not use the term “isogeneity,” preferring to use “equality” instead:

As I have discussed similarity and congruence, I have also distinguished between homogeneity and equality. In fact, the loci that can be transformed into similar ones are homogeneous; while the loci that can be transformed into congruent ones are equal.

The resemblance to Delboeuf’s writings here is quite striking.²⁰ In fact, Delboeuf himself

¹⁸Translation of: “Dire que l’espace est homogène, revient, au fond, à dire que rien n’a une grandeur absolue.”

¹⁹The term “locus,” which Leibniz uses, corresponds to the term “quantum” in Delboeuf’s writings. We have also used the term “extension” in this text to denote the same concept.

²⁰There is no evidence that Delboeuf was aware of the above mentioned essay by Leibniz, since it had not been published at the time. In fact, Delboeuf recounts that he was only alerted to the similarity between his work and some of Leibniz’s other writings (which had just been made available two years prior in Leibniz (1858)) by his mentor Ueberweg shortly after the publication of Delboeuf’s book (Delboeuf, 1895, p.346). While it is plausible that Delboeuf may have been influenced indirectly by Leibniz through his conversations with Ueberweg, the similarity between Leibniz and Delboeuf should be understood first and foremost as an instance of convergence of thought, suggesting an affinity between these two thinkers.

placed great emphasis on defining not only space, but the straight line and the plane in terms of their homogeneity or self-similarity (Delboeuf, 1860, p.180)²¹:

The plane is a homogeneous surface; the straight is a homogeneous line; that is to say that a portion of a plane, magnified, generates the same plane; that a portion of a straight, magnified, reproduces the straight. We can therefore regard homogeneity as being the genetic characteristic of space, of the plane, and of the straight line.

As Delboeuf notes, historically there have been a multitude of apparently different definitions of the straight: viewed from the standpoint of *distance*, it is the shortest path between two points; from that of *direction*, it is a line of constant direction, and so on (p.175). Given one of these definitions, the others would appear as *synthetic* truths, but none can be used to deduce the others analytically. However, these *synthetic* theorems, Delboeuf argues, are each in fact *analytic* decompositions of the original intuition that gave rise to them all (p.177). If we wish to escape the paradoxes, to overcome the impossible task of deriving one definition from another, we must seek to characterise the fundamental *essence* of the straight or of the plane. This leads Delboeuf to define them in terms of their *homogeneity*; like Euclidean space, the straight and the plane are distinguished by their invariance under dilations. Although the original intuition which underlies our notion of the straight is not itself conceptual, it can be captured by a concept—that of *homogeneity*—from which the theory can be developed.

Given this definition, the notion of straight lines in non-Euclidean geometries automatically becomes absurd; the geodesics of a curved space will not be invariant under dilations since the space itself is not invariant under these transformations: a great circle on a sphere surface will be pushed outside of the sphere it inhabits if it is dilated with respect to that surface. The notion of geodesics which is proper to Riemannian geometry assumes one of the secondary definitions of the straight: that it is *the shortest path between two points*, but if we follow Delboeuf and take the essence of the straight to be its *homogeneity*, we would no longer recognise the geodesics of non-euclidean spaces as straight lines, and the controversy concerning Euclid's postulate of parallels would be resolved. As De Risi (2015, p.61) puts it in his commentary on Leibniz's definition²²:

²¹Translation of: “*Le plan est une surface homogène; La droite est une ligne homogène ; c’est-à-dire qu’une portion de plan, majorée, engendre le même plan; qu’une portion de droite, majorée, reproduit la droite. Nous pouvons donc regarder l’homogénéité comme étant le caractère génétique de l’espace, du plan, de la droite.*”

²²For more information concerning Leibniz's development of this notion of the self-similarity or homogeneity in geometry, see De Risi (2015).

one could say that the truth of the Parallel Postulate is the condition for such a definition or rather that this last notion of a straight line, taken as a *real* definition, already implies the truth of the Parallel Postulate.

2.2.3.3 Neo-Kantianism?

While there are many aspects of Delboeuf's philosophy of geometry and science in general that could reasonably be characterised as neo-Kantian, Delboeuf explicitly made an effort to distinguish himself from Kant. The central argument of Delboeuf's book is framed in terms of a dialectic between Kant's apriorism and Mill's empiricism, which he seeks to reconcile by recognising the *process of abstraction* as the source of the apodicticity of the laws of nature (Delboeuf, 1860, p.50)²³:

It is therefore on an abstraction of our mind that the apodicticity of the laws of nature rests; and the famous axiom on the constancy and invariability of these same laws is itself only a consequence. [...] This solution reconciles empiricism and idealism, Mill and Kant.

Delboeuf's main objection to Kant's account of geometry is that it only "push[ed] back the difficulty instead of resolving it" since it makes the laws of geometry "the laws of our nature [...] without telling us why it is that precisely the ideas of geometry are innate to us" (p.8). As we have seen, Delboeuf seeks to answer this "why" by identifying homogeneity as the key concept that characterises this intuition, and from which the postulates of geometry would follow.

Like Kant, Delboeuf recognises the invariability and uniformity we discover in nature as the imprint of our own reasoning (Delboeuf, 1860, p.51)²⁴:

We are also getting closer to Kant; when we discover nature's agreement with the laws of our mind, we recover what we have placed in her: we have given her order and invariability; we discover order and invariability; we see what we

²³Translation of: "C'est donc sur une abstraction de notre esprit que repose l'apodicticité des lois de la nature ; et le fameux axiome sur la constance et l'invariabilité de ces mêmes lois, n'en est lui-même qu'une conséquence. [...] Cette solution concilie à la fois l'empirisme et l'idéalisme, Mill et Kant."

²⁴Translation of: "Nous nous rapprochons aussi de Kant. En effet, quand nous constatons dans la nature l'accord avec les lois de notre esprit, nous y retrouvons ce que nous y avons mis nous y avons mis l'ordre et l'invariabilité ; nous y retrouvons l'ordre et l'invariabilité ; nous y voyons ce que nous pouvons y voir ; nous la voyons comme nous pouvons la voir. L'abstraction est une opération de notre esprit, et la nature est pour nous devenue abstraite elle nous montre des lignes droites , des cercles parfaits ; nous y découvrons des corps simples, des corps purs, de l'eau distillée, du nitrate de potasse, du carbonate de chaux , tandis qu'elle nous fournit des mélanges, l'eau de la mer et des fleuves, du salpêtre, de la craie et du marbre."

are capable of seeing; we see nature as we are capable of seeing her. Abstraction is an operation of our mind, and nature has become abstract for us; it shows us straight lines, perfect circles; We discover simple and pure bodies, distilled water, potassium nitrate, carbonate of lime, whereas it provides us with mixtures, water from the sea and rivers, saltpeter, chalk and marble.

Concerning space, we have already seen that Delboeuf makes an effort to distinguish “real” or empirical space from “geometrical” space. In fact, in 1893, returning to the topic of geometry after a three decade hiatus, Delboeuf devotes an entire essay to emphasising this very distinction (Delboeuf, 1893). Concerning “real” space, Torretti (1978) has brought attention to the following intriguing passage in which Delboeuf argues that the non-Euclidean geometries of constant curvature would in no ways help us to represent real space better than the Euclidean (Delboeuf, 1894a, p.372)²⁵:

We can therefore say of Riemann and Lobachevsky’s spaces that they are artificial spaces, like Euclidean space; and in this respect they are just as geometrical as Euclidean space. But they have no special quality to represent real space better than the latter. This [real space] certainly has a curvature, but this curvature is different at each of its points and varies there at each instant. The real figures, that is to say, the bodies, change with time and place. The constant curvatures of meta-Euclidean spaces are therefore as far from reality as is the homogeneity of Euclidean space.

From the present-day perspective, we are compelled to respond: *why not then ditch Euclidean space, and the “meta-Euclidean” spaces of constant curvature? Why not embrace the varying curvature of real space and apply Riemann’s broader notion of differentiable manifolds?* Delboeuf seems on the point of anticipating the revolutions of the subsequent decades, but instead he passes by this and retreats to his aprioristic defence of Euclidean geometry. Torretti (1978, p.300) takes this as evidence that Delboeuf had not read Riemann, However, Delboeuf’s other references to Riemann in his works indicate that he had. It is more likely that Delboeuf avoided considering Riemann’s geometries of changing curvature for the very same reason that most commentators did at the time; that, as Poincaré put it, these geometries are purely

²⁵Translation of: “On peut donc dire des espaces de Riemann et Lobatschewsky, que ce sont des espaces artificiels, comme l’espace euclidien; et sous ce rapport, ils sont tout aussi géométriques que l’espace euclidien. Mais ils n’ont pas qualité spéciale pour représenter mieux que lui l’espace réel. Celui-ci, comme je l’ai dit dans ma première étude, a certainement une courbure, mais cette courbure est différente en chacun de ses points et y varie à chaque instant. Les figures réelles, c’est-à-dire les corps, y changent avec le temps et avec le lieu. Les courbures constantes des espaces méteuclidiens sont donc aussi éloignées de la réalité que l’est l’homogénéité de l’espace euclidien.”

analytic, they do not permit the kinds of mathematical generalisations that are the essence of all *a priori synthetic* reasoning.

Moreover, it is worth remarking that Riemann's own writings are not in contradiction with Delboeuf's position as Torretti seems to believe. Amid some of Riemann's most suggestive remarks, in which he proposes (in 1853) that the force of gravity be described along with inertia in terms of the dynamical geometry of a physical space, anticipating Einstein's equivalence hypothesis (Riemann, 1876b), we find a similar distinction between a *physical* (or real) and a *geometrical* space that Delboeuf makes:²⁶

I seek the cause [of gravity] in the state of motion of the continuous substance spread throughout the entire infinite space. [...] this substance may be thought of as a physical space whose points move in geometrical space.

This passage suggests that Riemann may have endorsed the recognition of two distinct notions of space; on one hand a real space as a kind of substance, a physical field, and on the other hand a geometrical, conceived space with respect to which we define and measure the motion or curvature of the physical space.

Although Kant himself had not considered the possibility of non-Euclidean spaces, it is worth noting that Kant does make a similar distinction between “empirical” and “absolute” space according to which the latter, which exists “for the sake of the possibility of experience,” is considered as “in itself nothing, and no object at all” (Kant, 1786a, p.16). We may here draw an analogy: whereas the motion of an empirical, relative space, for Kant, “presupposes in turn an enlarged space” in which it is moved, we could say that the curvature or inhomogeneity of some empirical space presupposes a homogeneous space with respect to which this curvature would be defined. In this way we would arrive by abstraction at Delboeuf's notion of a homogeneous space, lacking curvature, not as a physical thing, but as a reflection of a fundamental feature of the scientific method.

Overall, it seems fair to characterise Delboeuf's philosophy of geometry as broadly neo-Kantian; moreover, unlike the well established neo-Kantianism of today (Friedman, 2001; Bitbol et al., 2009) which was to some extent inaugurated by Helmholtz, Delboeuf's approach offers a path forwards which avoids parting ways with Euclidean apriorism.

²⁶The contracted passage given above is from Peter Pesic's English translation (Riemann, 1853). The full passage in German reads as follows: “Die nach Grosse und Richtung bestimmte Ursache (beschleunigende Schwerkraft); welche nach 3. in jedem Punkte des Raumes stattfindet, suche ich in der Bewegungsform eines durch den ganzen unendlichen Raum stetig verbreiteten Stoffes, und zwar nehme ich an, dass die Richtung der Bewegung der Richtung der aus ihr zu erldarenden Kraft gleich, und ihre Geschwindigkeit der Grosse der Kraft proportional sei. Dieser Stoff kann also vorgestellt werden als ein physischer Raum; dessen Punkte sich in dem geometrischen bewegen.”

2.3 Reflections and discussions

In this last part of the chapter we reflect on three separate issues which were raised by the mostly historical account of Delboeuf's contribution to the space problem which was given above: (1) We discuss the distinction we found between *physical* and *ideal* notions of space with respect to the types of *motions* that take place in them. This is particularly relevant to later parts of this thesis. (2) We will discuss an objection to Delboeuf's argument which has appeared without objection at least three times in history, and show why it is fallacious. (3) We will come back to Poincaré's understanding of the essence of mathematical reasoning and argue that it from the basic notion of *symmetry* or *sameness* that all mathematical reasoning follows, and that this also gives it its *a priori* character

2.3.1 Mobility or Leibniz shifts?

Readers may have noticed the contrast between Delboeuf's arguments for scale-invariance and Helmholtz's requirements for the possibility of congruence. Helmholtz's notion of congruence is empirically grounded, it depends upon the *physically realisable* motions of natural solids. Dilations of natural solids, on the other hand, are not physically realisable. On what grounds, then, do we assert the possibility of *similarities*?

In the physically realised motion of a natural solid, the given body is known to have been moved because it has changed its relation to other bodies. *Has it moved in geometrical space?* That is entirely a matter of convention since this space is a creation of our minds. The only space in which we know it to be moved is the *relative* space defined, and perhaps *conditioned*, by the surrounding bodies. By verifying the empirical possibility of congruence, we have only shown that this *physical* space—the relative space conditioned by surrounding bodies—is approximately isogenous. We have shown nothing of geometrical space.

The translations that are analogous to the dilations imagined by Wallis, Laplace, Delboeuf, and others, are not translations of single bodies with respect to others, they are *Leibniz shifts*: motions of all bodies in the universe with respect to geometrical space itself. These motions lack any physical meaning, and take place only in our minds. We are not concerned with motions of some bodies with respect to others, since such motions could only tell us of the properties of bodies. We want to know about the properties of space itself, and, since we conceive this space as *passive*, we may assert that the relations among bodies should be invariant under Leibniz shifts. This condition tells us with certainty that the curvature of geometrical space is constant. Further, we also assert the invariance of the relations among bodies under *universal dilations*, which tells us that geometrical space must be Euclidean.

The impossibility of dilating natural solids with respect to one another informs us that the real, physical space they mutually inhabit possesses a definite scale, but tells us nothing about the properties of an ideal space. The converse is also true: the possibility of dilating all bodies with respect to an imagined space ensures that it must be Euclidean, but says nothing of the physical properties bodies possess with respect to one another. One class of motions is proper to the one, the other to the other, but the two are not interchangeable. As we have seen above, in the *Metaphysical Foundations of Natural Science*, Kant explicitly distinguishes between absolute space, which is an ideal form, and empirical or relative space which are those spaces in which we perceive objects to be moved (Kant, 1786a, p.16-17). Therefore Helmholtz's claim that the possibility of congruence of natural solids implies anything about Kant's forms of intuition, is simply mistaken.

If we admit that Helmholtz's empiricist method can only tell us of the geometry of a real or physical space, then history has vindicated Delboeuf's objection to this. As we saw in section 2.2.3.3, Delboeuf insisted that real space has a curvature which is "different at each of its points and varies there at each instant," therefore the non-Euclidean spaces of constant curvature "have no special quality to represent real space better than the [Euclidean]" (Delboeuf, 1894a, p.372). On the other hand, If we wish to determine the properties of a space conceived as a pure form in the Kantian sense, then, once again, it is Delboeuf's method which is more appropriate.

2.3.2 Russell's relative angles objection

The tendency to assert the relativity of position to the neglect of the relativity of magnitude is epitomised by Russell (1897, 1898). Towards the end of the 19th century, like Helmholtz and Poincaré, Russell defended the notion that space should be of constant curvature, but that we had no criterion by means of which to favour Euclidean geometry. In his "*Essay on the foundations of geometry*," Russell emphasises above all the *passivity of space*, arguing that "Geometry depends throughout on the irrelevance of causation" (Russell, 1897, p.112). On this basis he endorses Helmholtz's axiom of *free mobility*, yet rejects Delboeuf's argument for space's "homogeneity." Russell's rejection is founded on a peculiar understanding of the "space-constant" of a non-Euclidean space of constant curvature, according to which it



Figure 2.5: Bertrand Russell at Trinity College, Cambridge, in 1891.

is not itself a magnitude but a “standard of comparison” between magnitudes (Russell, 1897, §79, §98). Accordingly, a dilation of all magnitudes with respect to space would leave no observable change since the value of the space constant would be transformed along with all the magnitudes.

Since Delboeuf himself had unfortunately died abruptly in 1896 at the age of 64, prior to the publication of Russell’s book, his view was defended instead by the French philosopher-mathematician Louis Couturat. In a response essay to Russell, Couturat remarks that Russell’s conception of the space-constant is at odds with other statements Russell makes such as: “although measurement and the judgment of quantity express the result of comparison, yet the terms compared must exist before the comparison” (Russell, 1897, §164, ft.2). Since a given magnitude can be measured with respect to the space constant, this implies that the space constant must be a real thing (Couturat, 1898)²⁷:

It is therefore not correct to say that the spatial constant is not a quantity, how could it not be one, since all the quantities of the corresponding space can be related to it? We can only measure a quantity by a quantity of the same kind; now, if we can measure all the magnitudes of a space in relation to the constant of this space, this constant must obviously be itself a magnitude, indeed a spatial magnitude.

In a follow-up article that same year, Russell appears to have acknowledged Couturat’s critique, however he raises a new argument against Delboeuf’s ideas (Russell, 1898):

I come now to the principal argument in favour of the *a priori* character of Euclidean space, namely the argument which derives from the impossibility of an absolute magnitude. For this discussion, it will suit me better to adopt Delboeuf’s terminology than to insist upon my own. [...] The question is: Can it be demonstrated that homogeneity is an *a priori* property of space?

On this point, a strong argument in my favour is derived, I believe, from the absolute magnitude of angles. Those who affirm it to be evident *a priori* that the sides of a triangle can be lengthened in a given ratio without altering the angles, ought to hold, it seems to me, that it is equally possible to alter all the angles in a given ratio without altering the sides. But that, we know, is impossible in all

²⁷Translation of: “Il n’est donc pas exact de dire que la constante spatiale n’est pas une grandeur comment n’en serait-elle pas une, puisque toutes les grandeurs de l’espace correspondant peuvent lui être rapportées? On ne peut mesurer une grandeur que par une grandeur de même espèce; or, si l’on peut mesurer toutes les grandeurs d’un espace par rapport à la constante de cet espace, il faut évidemment que cette constante soit elle-même une grandeur, et une grandeur spatiale.”

Geometries. If the logically relative nature of all magnitude is admitted, I cannot see why the argument would apply only to linear dimensions, and not to angles, which are equally magnitudes.

This rather strange argument by Russell might be dismissed as an idiosyncrasy were it not that others have claimed the same. It is independently repeated by [Torretti \(1978, p.297\)](#) for instance, and even [Poincaré \(1898\)](#) raises the same objection in his own essay of 1898:

It is absurd, they say, to suppose a length can be equal to an abstract number.
But why? Why is it absurd for a length and not absurd for an angle?

Since Couturat appears not to have addressed this objection, and Delboeuf did not live to see it, we will here show, on their behalf, that it is founded on a blatant misconception.²⁸

The rebuttal to this argument is suggested by Russell's formulation of it; *the relativity of angles is impossible in all geometries*. Why is this?

One does not need to compare two angles in order to measure them. The angle of intersection of two lines is already a relation, a relation between the directions of these two lines. An angle denotes an objective relation between two directions, not a relation of an object to space. While we may assert the relativity of directions (based on the isotropy of space), we cannot assert the relativity of angles. Similarly, while we may assert the relativity of magnitudes (based upon the homogeneity of space), we cannot assert the relativity of ratios between magnitudes. We cannot assert the relativity of angles therefore, for the very same reason that we cannot assert the relativity of ratios between magnitudes.

To Russell, the fact that angles seem "tied absolutely to their magnitude" is evidence for the existence of an *angular space-constant*:

We have an angular space-constant in every space, namely the four right angles. [...] angles are tied absolutely to their magnitude, and cannot be conceived as all expanded in a given ratio. We cannot therefore infer, from the fact that magnitude is relative, the impossibility of a space-constant.

But Russell makes a mistake when he imagines that 2π is an angular space constant; Russell is implicitly assuming here that angles denote relations of things to space. In fact, they do not, rather, as we have seen, angles denote relations between the directions of lines. Moreover, angles can also be understood as ratios of magnitudes if we remember that a *radian* is simply the quotient of the length of an arc by the radius of a circle.²⁹ The claim that 2π (which

²⁸Note, Russell and Couturat would go on to have an extended multi-year correspondence ([Schmid, 1983](#)).

²⁹Note also that this definition only hold in a Euclidean space, and that in a non-euclidean space the angle will need to be defined as a ratio of infinitesimals.

is the angle between two lines that have the same direction) represents an angle constant everywhere in space is as meaningless as the claim that at each of its points, space is endowed with a *ratio constant* of 1, i.e. the ratio between two lengths of the same magnitude which might form a standard with respect to which other ratios can be defined. But we naturally understand that a ratio is not a relation of an object to space, but rather, represents a relation between two magnitudes. Delboeuf's assertion of the homogeneity of space is, above all, an assertion of its *ideality*. Real things do not have real relations to space, but only to one another. If space possessed a scale constant, then bodies would be endowed with real relations to space, this property of space would become measurable, and space would no longer be *ideal*.

As relations, angles are measurable in and of themselves, whereas the magnitudes of bodies must be compared to one another if they are to be measured. This is the source of their relativity. Epistemically, all measurement is a relation between two given things. By means of this principle, it is easy to rule out the possibility of using non-Euclidean geometries as forms for phenomena; for if it is claimed that we live, or should represent ourselves to live, in a space of constant positive or negative curvature, we must ask upon what reason this claim is based. There are two options:

1. If this choice is grounded upon some empirical observations, suppose for instance that we live on the surface of a hypersphere; then according to the principle stated above, this hypersphere—whose curvature is measurable with respect to real objects—must be a real object itself.
2. If on the other hand it is not grounded empirically, rather, this geometry is being used purely in its capacity as a form; then there is no reason to choose it over the Euclidean. In this case, Poincaré's simplicity criterion rules it out, moreover, a compensatory field would need to be invented to abrogate its needless effects.

In both cases we find therefore that the geometries of constant, non-zero curvature, cannot—despite what was asserted by Helmholtz and others—be used as forms for phenomena on account of the relativity of all measurement of sizes. This argument naturally carries over to the more general geometries of changing curvature as well, which are even less competent to be conceived as forms. We are forced to the conclusion that space, as pure form, must be Euclidean.³⁰

³⁰As complementary to this argument for the Euclidean nature of space based on the relativity of magnitude, it is interesting to note that, as [Eisenthal \(2026\)](#) has very recently pointed out, only Euclidean geometry is consistent with the relativity of velocity due to geodesic deviation. Towards the end of his essay, Eisenthal writes:

2.3.3 Synthetic knowledge and the passivity of space

Given the weakness of the *relative angles* objection, it is surprising that Poincaré approves it in his essay of 1898 (Poincaré, 1898). A decade later, however, Poincaré's views concerning the relativity of magnitude seem to have changed. In book II of his volume *Science and Method* (Poincaré, 1908), amid comments concerning the relativity of space, Poincaré affirms the relativity of magnitude, citing Delboeuf as the principal proponent of this idea³¹:

there is another [sense of the relativity of space], upon which Delbeuf [*sic*] has particularly insisted. Suppose that in the night all the dimensions of the universe become a thousand times greater: the world will have remained similar to itself, giving to the word similitude the same meaning as in Euclid, Book VI. Only what was a meter long will measure thenceforth a kilometer, what was a millimeter long will become a meter. [...] When I awake to-morrow morning, what sensation shall I feel in presence of such an astounding transformation ? Well, I shall perceive nothing at all. [...]

Poincaré uses this idea to deny that we can have knowledge of absolute magnitudes (Poincaré, 1913, p.414), but does not go on to discuss Delboeuf's argument for the Euclidean nature of space.

Poincaré's deliberate avoidance of Delboeuf's thesis may be connected to his personal enthusiasm about non-Euclidean geometries as mathematical objects of study. In a well-known anecdote, he recounts how, out of the blue, upon stepping onto an omnibus in Coutances, it suddenly hit him with full clarity that "the transformations [he] had used to define the Fuchsian functions were identical with those of non-Euclidean geometry" (Poincaré, 1913, p.417). This realisation would have taken place at some time before 1880 (Gray, 1997). It is plausible that Poincaré's personal involvement with the development of non-Euclidean geometries drove him away from the defenders of Euclidean apriorism, who, at the time, were largely considered to be a reactionary force, opposed to those that were creatively driving the progress of knowledge. This may have led him to seek out conventionalism as a mid-way compromise between empiricism and apriorism.

Elsewhere in his writing, however, Poincaré has based his "*principle of relative motion*" on an affirmation of the "passivity of space" (Poincaré, 1913, p.83). Russell too, who, even more

"If someone were disposed to think that absolute motion is impossible in principle—if space were thought of as not the kind of thing that objects can move with respect to—then that idea could be leveraged as a metaphysical argument against the very possibility of space having a constant non-zero curvature in the first place."

³¹This is a famous passage which has recently drawn attention due to its suggestion of the possibility of scale-invariant cosmological models. See for instance Gryb and Sloan (2021).

than Poincaré, defended non-Euclidean geometries of constant curvature, affirmed that space is *passive*. But what is the root of this notion of space's passivity?

We have seen above that if we base some notion of the passivity of space on the invariance of bodies when they are moved with respect to one another, we will only have learned of the (approximate) passivity of a *physical* space (see section 2.3.1). This physical space does not permit the dilations of individual bodies, therefore it may be non-Euclidean. But *who are we to say that this physical space is passive?* Why should physical space allow for the possibility and mobility of rigid bodies? Even if we put aside the complications raised by modifications due to heat, surely the question of whether a natural solid retains the same relations among its parts when it is moved with respect to other physical bodies is one that should be answered by empirical science. Indeed this is what was done, through the recognition of the *equivalence principle*—which, in the division between force and inertia, places gravity on the side of inertia and (chrono-)metricity—Einstein fulfilled Riemann's ideas and showed once and for all that the physical space which governs those motions which have traditionally been called “inertial” is not passive, but *dynamical*.

But what then of our intuition of the passivity of space, and the corollary *relativity of motion*? This law, and the intuition underlying it, can only be based in a truly Kantian conception of space, a space abstracted from all contingent phenomena. Notions of space's passivity, the relativity of magnitudes, and the relativity of motion are pervasive in Poincaré's works. Poincaré at times justifies these ideas on the basis that contrary hypotheses would be “repugnant to the mind” (Poincaré, 1913, p.107-109), but he does not discuss why we feel this repugnance. I propose that these intuitions we have of space, of the relativity of motion and of magnitudes, are rooted in this same methodological concept of space's homogeneity which is responsible for providing foundations for Euclidean geometry.

Quite apart from space and its geometry, we saw in section 2.1.1 that for Poincaré, the inductive method, which allows a formula to generalise over an infinity of cases, is the “veritable type of the synthetic *a priori* judgment” since it is “inaccessible to analytic demonstration and to experience” (Poincaré, 1913, p.39). But what is it that makes this generalisation possible? Is it the mathematical concept of infinity? An infinite set which is not *ordered*, which is not in some respect homogeneous, does not permit generalisations. It is not the notion of infinity that allows for reasoning by recurrence, rather it is the concept of symmetry, of the absolute mathematical invariance of some property under transformations. It is through the “process of abstraction,” described by Delboeuf, that we gain access to this form of reasoning; and it is from this basic notion of *sameness*, which we find in the notion of homogeneity and of symmetry in general, and which, as Plato emphasises in the *Phaedo* (Hackforth et al.,

1972), is not known to us empirically, that Poincaré’s “synthetic a priori” propositions arise.

Morals for scientific methodology

As we have seen, unlike other neo-Kantian influences on contemporary philosophy of space and time, that of Cassirer and those of Helmholtz and Poincaré, Delboeuf’s neo-Kantianism affirms the apriority of Euclidean geometry. It may be argued that Delboeuf’s ideas, though perhaps interesting, have little relevance to present-day physics and philosophy of physics, since, after all, Delboeuf did not work directly in physics, his ideas had little or no influence on the development of Einstein’s theories, and, unlike Cassirer, his philosophy was not developed in response to these paradigm-shifting ideas. In what respect, then, should we take this account seriously?

We have seen throughout this essay that the central insight upon which Delboeuf grounds his philosophy of geometry—that is, of the relativity of magnitude—was not unique to him. It dates back at least to Wallis in 1663, and was recognised by a some other significant physicists and philosophers over the centuries. We also saw that Leibniz embarked upon a project very similar to Delboeuf’s in his attempt to find stable foundations for geometry. Moreover, we have seen that Delboeuf’s account is defensible in the context of the philosophies of geometry that were present at the time, it stands up to Russell and Poincaré’s fallacious *relative angles* objection (sec.2.3.2), it is both more Kantian and more self-consistent than Helmholtz’s allegedly neo-Kantian approach (sec.2.3.1), and it even resolves certain problems in the foundations of Poincaré’s philosophy of mathematics (sec.2.3.3).

Concerning the applications of these ideas to physics, it is clear that Delboeuf can only contribute on the *methodological* side of things. We may propose a strict distinction between *geometrical* space, conceived as a form, and *physical* space (or space-time), conceived as part of the content of this form. Indeed, as [Lehmkuhl \(2014\)](#) has shown, even Einstein showed some restraint against unequivocally accepting the geometrical interpretation of his theory; for instance in his review of Meyerson’s *La déduction relativiste*, he writes ([Lehmkuhl, 2014; Einstein and Metz, 1928](#)):

The fact that the metric tensor is denoted as “geometrical” is simply connected to the fact that this formal structure first appeared in the area of study denoted as “geometry.” However, this is by no means a justification for denoting as “geometry” every area of study in which this formal structure plays a role, not even if for the sake of illustration one makes use of notions which one knows from geometry.

Since the advent of general relativity, a vast literature of flat space alternatives or subtle modifications has been proposed.³² This literature raises a wide array of methodological advantages of working in flat space, including: (1) the recovery of a well-defined local gravitational energy and of global energy conservation laws (Rosen, 1940a,b; Logunov and Mestvirishvili, 1985),³³ (2) greater consistency with methods in particle physics (Lasenby et al., 1998), (3) avenues towards unification with particle physics and prospects of developing a theory of quantum gravity (Dicke, 1957; Lasenby et al., 1998; Pitts and Schieve, 2001), (4) the possibility of implementing various interpretations of Mach’s principle (Sciama, 1953a; Dicke, 1957), (5) the development of simplified models of gravity which reproduce some of the basic results of Einstein’s theory (Broekaert, 2005; Arminjon, 2002). Many of these models explicitly appeal to Poincaré’s notion of the conventionality of geometry to justify their methods, however, given the findings of the present chapter, we suggest that Delboeuf’s forgotten arguments provide a stronger foundation for these flat space approaches.

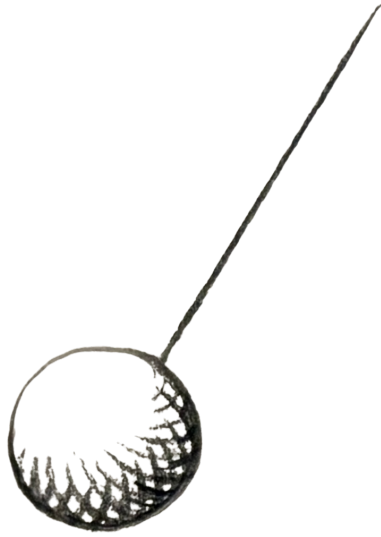
The topics that Delboeuf’s writings raise, however, are most relevant to certain recent developments in the physics and philosophy of cosmology, and for this reason they will also be relevant to later chapters of this thesis. In recent years, Julian Barbour has been attempting to extend the Machian research program to encompass a requirement for the scale-invariance of cosmological models (Barbour, 2010). If we refuse to accept the existence of epistemically inaccessible absolutes, then the universe must consist only in the relative configuration of its parts—i.e., its *shape*. This way of thinking has led to the development of the theory of *Shape Dynamics* (Barbour, 2012; Mercati, 2018). If we recognise that the shape of a body consists of the internal relations amongst its parts, while its size is an external relation to other bodies, then the universe as a whole, which has no external reference possesses only a shape. The central insight discussed in this essay—which was recognised by Wallis and Delboeuf—is that the reciprocal independence of shape and size implies the Euclidean nature of space. This essay may help provide grounds for Barbour *et al.*’s use of Euclidean space as a background for their models.

It is only in the context of cosmological models, rather than in the study of subsystems of the cosmos, that transformations of all bodies with respect to space, i.e. *Leibniz shifts* or transformations by *similarity*, can be considered. Outside of shape dynamics, the requirement for

³²See for instance: Rosen (1940a,b); Gupta (1954); Kraichnan (1955); Dicke (1957); Thirring (1961); Huggins (1962); Weinberg (1964a,b); Ogievetsky and Polubarinov (1965); Mittelstaedt and Barbour (1967); Nachtmann et al. (1969); Deser (1970); Fang and Fronsda (1979); Cavalleri and Spinelli (1980); Davies and Falkowski (1982); Logunov and Mestvirishvili (1985); Lasenby et al. (1998); Pitts and Schieve (2001); Arminjon (2002); Broekaert (2005).

³³In such theories, the principle of conservation of energy would appear, not as a contingent empirical fact, but as a guiding methodological principle.

the invariance of dynamics under similarity transformations in cosmology has been called “*dynamical similarity*,” and it is a growing area of research in cosmology ([Sloan, 2018](#); [Gryb and Sloan, 2021](#); [Bravetti et al., 2022](#)). We hope that the ideas discussed in the present chapter will help to provide some philosophical context and justification for these cosmological speculations.



INTERNAL CAUSES

One gay beer for my gay friend, one normal beer
for me because I am normal.

Ray, *In Bruges*, 2008

Is the law of inertia a fact or a definition?

Introduction

Classical mechanics is founded on a paradigmatic distinction between *inert* and *forced* motion. Causal influences are communicated through forces which allow bodies to induce changes in one another's state of motion. The concept of *force* is therefore completely essential to the *causal* scientific account of motion offered in classical mechanics. But in order to define *forced* motion, or any *change* in motion, it is necessary to have a definition of the motion which bodies resort to in the absence of forces, i.e. it is necessary to have some *law of inertia*. For this reason the law of inertia is absolutely central to the conceptual framework of classical mechanics. However as soon as this is accepted a tension appears: if the law of inertia is really part of our definitional framework, if it is merely a necessary condition for our defining what we mean by 'force,' why does it appear to have specific empirical content? In particular, the law of inertia establishes the existence of a set of *preferred reference frames* such that there is an *absolute* difference between a body that is still and one that accelerates or rotates. Accordingly, if we consider a body in isolation in an otherwise empty universe, there is a fact

of the matter concerning whether or not it is rotating, despite the *kinematical* equivalence of the two situations. This tension, between the *necessity* of the law of inertia in classical mechanics and its apparently anti-relationalist consequences, has been the starting point of a long debate, stretching over multiple centuries which has accompanied the development of classical mechanics through its rise, its stabilisation and its eventual unwinding. Although this debate continues to this day, in the present chapter we will be exclusively interested in the context of classical mechanics, in the period spanning from Newton’s *Principia* to the late 19th century. We will not be looking at later ideas that this debate has stimulated such as the development of Einstein’s relativity theories, or of relational “Machian” gravity models in the 20th century, as these topics will be covered in chapters 5 & 6 and chapters 7 & 8 respectively.

The present chapter will be split into three sections following the three phases of classical mechanics that we have outlined above. It is possibly the most *incomplete* chapter in this dissertation, since the historical figures that will be discussed are not treated in sufficient detail to give an adequate appraisal of their contributions. This is partly due to the long history that the chapter attempts to cover. For this reason, the chapter should not be read as a thorough history of the development of classical mechanics, nor even as a thorough review of issues connected to the law of inertia. Instead, the chapter should be read as an exploration of certain *argumentative themes* that have arisen from the controversy over the law of inertia and absolute motion over the course of the period.¹

This chapter will also serve as a prelude to the more in-depth look at Ernst Mach’s critique of the law of inertia we provide in the next chapter, in which we will engage in much more detail with secondary literature. As we will see, Mach was by no means the first to object to the classical formulation of the law of inertia, and his arguments bore both many similarities and some important differences to those of his predecessors. The sections discussing the work of Carl Neumann and Ludwig Lange (§3.3.1 and §3.3.2 respectively) will be referenced specifically in the next chapter and are therefore essential to setting up our discussion of Mach.

What we will argue at present is that the paradigm of classical mechanics can in an important sense be characterised by a *conflation* between two distinct concepts of space, and correspondingly between two concepts of inertia. This conflation originates with Newton but

¹For a more thorough history of classical mechanics, the reader can consult [Truesdell \(2012\)](#) for instance; additionally, I should point to the under-appreciated essay of Helmut Pulte ([Pulte, 2009](#)) concerning the rise of hypothetical thinking in 19th century rational mechanics, and to the roots of Carl Neumann’s paper, which the reader should consult as a more detailed account that supplements §3.3 of this chapter. See also [Darrigol \(2021b\)](#) for a discussion of many of the same historical figures and arguments that this chapter covers.

is crystallised in Euler’s writings and captured in the concept of “internal causes.”² According to Newton and Euler, the cause of a body’s inertial motion is inherent to the body itself, and does not lie in its relation to anything external. We will argue that this position is not viable since nothing about a body taken in isolation is capable of determining its relationship to the inertial frames, therefore this set of inertial frames must be interpreted as an *external* cause of inertial motion, and in this sense the inertial frames represent something *material*; moreover, the internal–external cause distinction also conflicts with the general aim of a scientific attitude, according to which all causes should be brought under a common concept (see previous commentary on this with regards to the concept of energy and rejection of appeals to the supernatural in §1.2). This essentially undermines the classical distinction between *inert* and *forced* motion in favour of a holistic view of causation according to which all motion is equally *caused*. This holistic view which we ultimately come to is exemplified in the work of Ernst Mach, as we will see in chapter 4. Nonetheless, the history of mechanics suggests that, despite the fallacy of the conflation mentioned above, it may have been necessary to have passed through a period in which this Newtonian-Eulerian dogma ruled in order to discipline the mind in these early days of the development of mechanics.

The holistic Machian view referred to above, despite its influence on the *emancipation* from the paradigm of classical mechanics that was brought about in the early 20th century, remains controversial and somewhat marginal to this day. For this reason, in the second half of this thesis, the views developed in this and the next chapter (as well as in previous chapters), will form the basis for a *critique* of our present mainstream understanding of contemporary physical theories (chapters 5 and 6) and will lead us to consider certain alternative hypotheses in later parts of the thesis (chapters 7 and 8).

3.1 The early days

Newton’s most direct and well known treatment of the tension between absolute and relative concepts of motion comes from his *Scholium* following some preliminary definitions pertaining to the *quantity of matter* (i.e. mass), *quantity of motion* (i.e. momentum) and *force*. Newton explains that the notions of time, space, place, and motion that he discusses, are understood by the “vulgar” in terms of the relations borne to “sensible objects,” from which

²Note that Euler did not introduce this notion of *internal causes* as it was already present in Newton who identified inertial phenomena as being due to a *vis insita* or “innate force” in matter (see [Newton \(1687, Def. III\)](#)). The distinction between internal and external causes in early modern mechanics can be seen as a transformation of the Aristotelian distinction between natural and violent motion. Although the Aristotelian framework was rejected, the explanatory contrast between motions grounded in the nature of bodies and those produced by external agents persisted in a modified form.

“arise certain prejudices, for the removing of which, it will be convenient to distinguish them into absolute and relative, true and apparent, mathematical and common.” He then defines absolute time and absolute space as (Newton, 1846, p.77):

I. Absolute, true, and mathematical time, of itself, and from its own nature, flows equably without relation to anything external, and by another name is called duration. [...] II. Absolute space, in its own nature, without regard to anything external, remains always similar and immovable.

Soon after this, however, a tension arises. In the first place, Newton justified the introduction of absolute time and space on methodological ground, explaining that ordinary measures of time are affected by forces and therefore require correction in astronomy (Newton, 1846, p.78): “For natural days are truly unequal, though they are commonly considered as equal, and used for a measure of time; astronomers correct this inequality for their more accurate deducing of the celestial motions.” On the other hand, Newton subsequently seeks to demonstrate the *actual* existence of absolute space and time by an appeal to the empirical reality of centrifugal forces. Acknowledging the “great difficulty” involved with distinguishing true motions from the apparent, Newton proposes that the observation of forces may help us in this endeavor. As an example of how absolute circular motion might be determined, Newton considers the following thought experiment:³

if two globes, kept at a given distance one from the other by means of a cord that connects them, were revolved about their common centre of gravity, we might, from the tension of the cord, discover the endeavor of the globes to recede from the axis of their motion, and from thence we might compute the quantity of their circular motions.

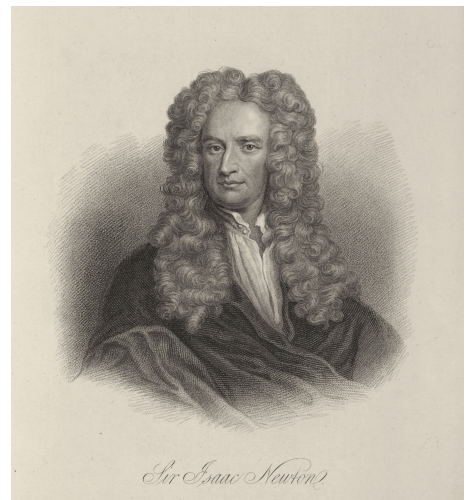


Figure 3.2: Isaac Newton.

³Newton’s famous ‘bucket experiment’ (see Newton (1846, p.81)), which was devised to undermine the Cartesian idea that relative motions alone exist, is also an example of this kind. According to Descartes, the motion of the water should be defined in relation to the sides of the bucket, but this motion, Newton shows, has no bearing on whether the water recedes up the sides of its container. Newton’s disagreements with Descartes are expressed more explicitly in his unpublished essay *de gravitatione* (Newton, 1962). See Barbour (2001, p.609-624) for a detailed discussion of this work.

It is not too difficult to notice however that although certain forms of motion such as circular motion might be deduced by such means, we can not know at what speed we are travelling rectilinearly through absolute space. This is because Newton's laws make no distinction between such cases. Newton, in fact, recognised this and stated it in his fifth corollary:⁴

The motions of bodies included in a given space are the same among themselves, whether that space is at rest, or moves uniformly forwards in a right line without any circular motion.

3.1.1 Empiricist and rationalist objections

The empirical existence of Newton's concept of absolute space was opposed by several of his contemporaries, notably: Berkeley, Huygens and Leibniz. Berkeley's critique is often recognised as an early precursor of Mach's ([Earman, 1989](#)). In fact it has been noted that there are a number of striking parallels between aspects of Berkeley's and Mach's philosophies ([Popper, 1953](#)). In *The Principles of Human Knowledge* Berkeley objected to Newton's concept of absolute space and absolute motion on the grounds that these were not observable; "it does not appear to me that there can be any motion other than relative; so that to conceive motion, there must be at least conceived two bodies" ([Berkeley, 1710](#), §112). For Berkeley, motion cannot be referred to space itself since space is not a distinct thing ([Berkeley, 1710](#), §116):

when I speak of pure or empty space, it is not to be supposed that the word 'space' stands for an idea distinct from, or conceivable without, body and motion. Though indeed we are apt to think [that] every noun substantive stands for a distinct idea that may be separated from all others, which has occasioned infinite mistakes.

By giving space a name then, we easily make the mistake of assuming that it must be a thing with respect to which motion may be defined. Poincaré, who was in fact an admirer of Berkeley's work, would echo this sentiment a couple centuries later: "Space is only a word that we have believed a thing" ([Poincaré, 1913](#), p.5).⁵

⁴We will return to Newton's fifth corollary in chapter 4 when we cover Mach's discussion of this.

⁵For evidence of Poincaré's admiration of Berkeley see his preface to the Halstead translation of *Science and Hypothesis*, in which he characterises Berkeley's work as a "natural and necessary stage in the development of English philosophy," and uses it as evidence that the English are "capable of making ascensions otherwise than in a captive balloon" ([Poincaré, 1913](#), p.4).

Although Leibniz came at the same issue from the *rationalist*, rather than the *empiricist* side, his view is much the same.⁶ For Leibniz, the property of homogeneity which Newton assigns to space and time make them “ideal” things rather than physically real entities (Leibniz, 1716, §29, §33):

The Parts of Time or Place, considered in themselves, are ideal things; and therefore they perfectly resemble one another, like two abstract Units. But it is not so with two concrete Ones, or with two real Times, or two Spaces filled up, that is, truly actual. [...] Since Space in itself is an Ideal thing, like Time; Space out of the World must needs be imaginary, as the Schoolmen themselves have acknowledged. The Case is the same with empty Space within the World; which I take also to be imaginary, for the reasons before alleged.

Leibniz’s characterisation of space here is quite similar to that of Delboeuf as we saw in chapter 2. Due to its homogeneity, that is the equivalence of all its parts, space is not empirically observable, it is therefore an ideal thing, and not physically real. However, Newton’s space is not homogeneous in all respects; in particular it establishes a set of inertial frames which provides a standard for absolute acceleration and absolute rotation. In other words it is not symmetric under rotations or accelerations of the reference frame. Leibniz is right that the points of Newton’s space, since they are *indiscernible*, are not truly distinct, so that if our universe is imagined shifted five feet to the right, no real motion has taken place. But in Newton’s space, a body in a non-inertial frame is distinguishable from that same body in an inertial one, therefore it cannot be said that there is no material difference; Newton’s space is not a purely *ideal* thing in this specific respect. From this point of view, we could say that Newton’s error is to have brought together into the same concept: (1) the purely homogeneous space which was motivated for methodological reasons, and (2) the empirically real ground of inertial frames. In doing so, Newton attributed the property of homogeneity to a physically real *inertial field*; this may be a pragmatically convenient move but is not metaphysically defensible. We will come back to these points shortly when we discuss Euler.

3.1.2 Dealing with inertia

Concerning Newton’s actual arguments for the empirical reality of absolute space, Newton’s contemporaries came into some difficulties. As we will see, two main objections at this time

⁶We do not have space to do justice to Leibniz’s views in detail here. For a much more thorough discussion of Leibniz’s views on the relativity of motion see Tho (2017). For more specifically on Leibniz’s correspondence with Clarke (which we quote from) see Vailati (1997).

were made against Newton: (1) an appeal to the existence of the “fixed stars” which could replace absolute space, or (2) some fallacious arguments based upon a misunderstanding of mechanical systems.

3.1.2.1 Berkeley’s fixed stars

In his response to Newton’s experiment with the rotating vessel in *The Principles of Human Knowledge*, Berkeley rejects the need to postulate something other than known bodies as the source of centrifugal forces; however, he does not specify precisely which bodies ought to be considered instead. In *De Motu* however, he revisits this problem and makes some particularly suggestive remarks. Responding to the thought experiment with the two globes, Berkeley writes (Berkeley, 1721, §59):

the circular motion of two globes around a common centre cannot even be conceived by the imagination. Thirdly, let us assume that the heaven of fixed stars is created; suddenly, it is possible to conceive motion by the concept of the approach of the globes to different parts of that heaven.

As Earman (1989) remarks, this is merely an epistemological objection, but it doesn’t address the question of the cause of the forces. As we will see in the next chapter, Mach’s proposal is quite similar, and there has also been much discussion of whether Mach’s proposal was merely an epistemological objection or represented a substantial physical hypothesis, however as we will see Mach was more explicit than Berkeley about suggesting an alternative hypothesis about the origin of inertia.⁷

3.1.2.2 Fallacious objections

Just after the above quoted passage from *De Motu*, Berkeley does attempt to give an account of the forces as he reconsiders the case of the rotating vessel:

As regards circular motion, many think that, as true circular motion increases, a body necessarily tries to recede more and more from its axis of rotation. This

⁷It is worth noting here that although it is commonplace to cite only Berkeley and Mach as advocates of this idea, prior to Mach, the French mathematician Jean-Marie Duhamel had expressed very similar views in his preface to the fourth volume of *Des Méthodes dans les Science de Raisonnement*: “Le temps n’a pas plus d’existence réelle que l’espace; il est peut-être encore moins saisissable. Ces deux prétendus êtres sont des créations fantastiques de l’imagination de l’homme, qui veut toujours aller au delà de ce qu’il peut saisir et comprendre. [...] Le système des étoiles est le plus considérable et le moins variable qu’il soit donné à l’homme de connaître; c’est à ce système, que l’on peut sans inconvénient considérer comme immuable, qu’il est convenable de rapporter les grands mouvements, comme ceux de la terre et des autres planètes.” (Duhamel, 1870, XVII, XIX). See Darrigol (2021b) for more discussion.

however results from the fact that, as circular motion can be seen to derive its source at each moment of time from two directions, one along the radius and the other at a tangent, then if the impetus is increased only in the latter direction, the body in motion recedes from the centre and its orbit ceases to be circular. But if the forces are increased in both directions equally, the motion will remain circular but accelerated by an effort [conatus], [...] It must therefore be said that water that is spinning in a bucket moves up the sides of the vessel because, as new forces are applied in the direction of the tangent to each particle of water, at the same instant no equal centripetal forces are applied to them. It does not follow by any means from this experiment that one can necessarily discern an absolute circular motion in the forces receding from the axis of motion.

Unfortunately, Berkeley's reasoning is fallacious here as it mixes up accelerations with velocities: in circular motion the velocity is always entirely tangential whereas the acceleration is entirely centripetal, however Berkeley characterises both as "forces" and places them on equal footing. Berkeley claims that the water in Newton's bucket rises along the sides only because tangential "forces" are applied to the water without equal centripetal forces, and not because the water is in absolute rotation. But this presupposes that both the tangential and centripetal forces are kinematical, i.e. they are part of the water's proper state of motion without reference to some external absolute space. In fact, the water as a whole is stationary in relation to itself, therefore one must appeal to something external to explain the concavity of its surface, and Berkeley had already implicitly admitted absolute space when he accepted that the water is *in motion*.

A similar fallacious argument was made by Huygens, who thought to have discovered that circular motion is inherent to a body without reference to something external:⁸

For a long time I had thought that rotational motion by means of centrifugal forces contains a criterion for true motion. Indeed, with regard to other phenomena it is the same whether a circular disk or a wheel rotates near me, or whether I circle round the stationary disk. However, if a stone is put on the circumference this will be projected only if the disk rotates, and therefore I formerly thought that circular motion is not relative to any other body. Still, this phenomenon showed only that the parts of the wheel, owing to the pressure acting on the circumference, are driven in relative motion among themselves in different di-

⁸Translation from Jammer (2013, p.125).

rections. Rotational motion is therefore only a relative motion of the parts, which are driven to different sides, but held together by a rope or other connection.

Like Berkeley, Huygens conflates velocity with acceleration by claiming that the two sides of the wheel are in relative motion with respect to one another. As [Jammer \(2013, p.125\)](#) explains: “In [a] rotating coordinate system the parts of the disk are at rest . The dynamic effect, however, referred to this system, does not vanish; the ‘pressure’ exerted by centrifugal forces has not been transformed away, as it should be were the centrifugal force but a dynamical effect of the relative motion of the particles.”

These two fallacious arguments demonstrate that even among learned natural philosophers of the time, it was easy to make mistakes about elementary questions in mechanics, since the fundamental concepts of this science had not been fully clarified, and the mind of scientists had not been trained through their application. As we will see later in this chapter, the same mistake would still be made by Kant in the late 18th century (see §3.2.5).

3.1.3 Conceding absolute acceleration

In Leibniz’s correspondence with Huygens of 1694, Leibniz recounts that when the two had met in Paris, Huygens had claimed that the true subject of motion can be distinguished in the case of circular motion. Recalling his reaction, Leibniz writes: “That stopped me for a while; and I remembered reading almost the same thing in Newton’s book. But that was when I thought that I had already seen that circular motion has no advantage in this. And I see that you now agree with me.” Leibniz goes on to assert the “equivalence of hypotheses” between the view that a body rotates and that other bodies rotate around it, citing his defence of the equivalence of Copernican and Ptolemaic astronomy ([Leibniz, 1989b](#), p.308). But despite asserting this equivalence, Leibniz does not venture to provide a mechanical account of the forces that one would need to introduce if the earth were supposed to stand still, nor of what the cause of such forces would be. Some years later, in 1716, in his fifth letter to Newton’s follower Samuel Clarke, Leibniz is forced to admit the reality of a certain “absolute true motion,” since he cannot shake away his anthropomorphic view of forces and admit their conventional nature ([Leibniz, 1716](#), §53):

However, I grant there is a difference between an absolute true motion of a Body, and a mere relative Change of its Situation with respect to another Body. For when the immediate Cause of the Change is in the Body, That Body is truly in Motion; and then the Situation of other Bodies, with respect to it, will be changed consequently, though the Cause of that Change be not in Them.

This immediately raises a question: if there is absolute motion, with respect to what is this absolute motion defined? Leibniz seems to circle around this point without fully coming to terms with the issue. However he maintains throughout his writings that space's homogeneity means that it is something ideal and it is not a real thing with respect to which real motion should be referred.

3.2 Stabilisation of mechanics

By the mid 18th century, the success of Newton's system had become clear, and it was no longer defensible to simply criticise the law of inertia on the basis of its supposed inconsistency. Nonetheless, the epistemic inaccessibility of absolute space and absolute time continued to cause tension. This tension is particularly apparent in some of Euler's works as we will see shortly. Leonhard Euler, known primarily as the most prolific mathematician in history—whose papers continued to appear in journals for some five decades after his own death—was also, as [Truesdell \(1968, p.106\)](#) put it, “the dominating theoretical physicist of the eighteenth century.” We will not discuss Euler's broader foundations of classical mechanics and contribution to the field in much detail since this would distract from the main topic of this chapter, for our purposes we will only comment in an incomplete way on his views on space, time, inertia and the relativity of motion.⁹

3.2.1 Euler's reflections

In a delightfully polemical essay of 1750, titled “*Réflexions sur l'espace et le tems*” ([Euler, 1750](#)), Euler contrasts the “metaphysical views” of “philosophers” (he is referring to Leibniz and Berkeley) to those of “mathematicians” (i.e. Newton and himself). While he concedes to the philosophers that “It is true that the senses are not capable of providing us with the ideas of space and of place, and that it is only by reflection that we form these ideas,”¹⁰ he insists that “one would be greatly mistaken, if one wished to maintain that there do not exist things of which we have no other ideas except by reflection.”¹¹ Space and time are known

⁹For more detailed expositions of Euler, consult recent scholarship: [Maltese \(2000\)](#); [Hepburn \(2007\)](#); [Verdun \(2015\)](#); [Calinger \(2016\)](#); [Hepburn \(2017\)](#); [Veldman \(2024\)](#); [Giovanelli \(2024\)](#), as well as the dissertation of Helmut Pulte ([Pulte, 1989](#)).

¹⁰Translation of: “Il est vrai que les sens ne sont pas capables de nous fournir les idées de l'espace et du lieu, et que ce n'est que par réflexion que nous nous formons ces idées” ([Euler, 1750](#), p.329).

¹¹Translation of: “Et on se tromperoit fort, si l'on vouloit soutenir, qu'il n'existe pas des choses, dont nous n'avons d'autres idées que par réflexion” ([Euler, 1750](#), p.329).

to exist, for Euler, not because they can be sensed, but because they are necessary for the proper functioning of mechanics: ¹²

The preservation of the state [of motion] of bodies is not determined by their relation to other bodies, but by place, as it is conceived in Mathematics. Now, one cannot say that this principle of Mechanics is founded upon something that subsists only in our imagination; and from this it must be concluded absolutely that the mathematical idea of place is not imaginary, but that there is something real in the world which corresponds to this idea. There is therefore in the world, besides the bodies which constitute it, some reality which we represent to ourselves by the idea of place.

Concerning the view that inertia might instead be defined in some way relationally, that is, with respect to other bodies, Euler explains that this is not possible since those bodies themselves move. He illustrates this by considering the example of a body in still water versus water which begins to flow. Although a body in flowing water will be observed to move along with the water, Euler explains that this is due to forces and not the inertia of the body: ¹³

Mechanics shows us very clearly that the body does not follow the current of the water except insofar as it is struck by the particles of the water; and that it is consequently an external force which sets this



Figure 3.3: Leonhard Euler.

¹²Translation of: “La conservation de l’état des corps ne règle sur le lieu, tel qu’on le conçoit dans la Mathématique, et point du tout sur le rapport aux autres corps. Or on ne sauroit dire que ce principe de Mécanique soit fondé sur une chose, qui ne subsiste que dans notre imagination : et de là il faut conclure absolument que l’idée mathématique du lieu n’est pas imaginaire, mais qu’il y a quelque chose de réel au monde, qui répond à cette idée. Il y a donc au monde, outre les corps qui le constituent, quelque réalité, que nous nous représentons par l’idée du lieu.” (Euler, 1750, p.329)

¹³Translation of: “Mais la Mécanique nous fait voir très clairement, que le corps ne suit pas le courant de l’eau, qu’autant qu’il est frappé par les particules de l’eau ; et que c’est par conséquent une force étrangère qui met ce corps en mouvement. Donc sans cette force le corps restera aussi bien en repos, dans l’eau courante, que dans la dormante, et partant le corps dans la conservation de son état de repos ne se règle point sur les corps, qui l’environnent immédiatement. De là il s’ensuit, que ce qui est nommé lieu dans la Mécanique ne souffre pas l’explication de la Métaphysique, par laquelle on veut, que le lieu ne soit autre chose, que la relation du corps par rapport aux autres corps qui l’environnent.” (Euler, 1750, p.327-8)

body in motion. Therefore, without this force the body will remain equally at rest in running water as in still water; and hence the body, in preserving its state of rest, is not regulated by the bodies which immediately surround it. From this it follows that what is called place (lieu) in Mechanics does not admit of the explanation of Metaphysics, by which one wishes to make place nothing other than the relation of the body to the other bodies which surround it.

This argument is essentially no different to Newton's bucket argument against the Cartesian notion that the immediate surroundings of a body determine its motion.

Euler then addressed Berkeley's proposal that it is with respect to the "fixed stars" that we should define inertia, and in so doing also anticipates Mach's objection to Newton's bucket. Euler's objection here is that this could only apply if those stars were far away and thus appear to be stationary, but this fails to be the case for bodies that are close-by (Euler, 1750, p.328): ¹⁴

If they said that it was with respect to the fixed stars that the principle of inertia ought to be explained, it would be quite difficult to refute them, since the fixed stars, being themselves at rest, are so far from us that the bodies which are at rest with respect to absolute space, as it is conceived in Mathematics, would also be so with respect to the fixed stars. But besides this, it would be a very strange proposition, and contrary to many other doctrines of Metaphysics, to say that the fixed stars direct the bodies in their inertia. Moreover, this rule would be equally false if it were permitted to us to apply it to bodies which are near to some fixed star.

3.2.2 Euler's theory of motion

In other texts, Euler takes a similar line: acknowledging on one hand the impossibility of perceiving absolute space and absolute time, but holding these as necessary for the proper functioning of mechanics. For instance, in the first two chapters of his *Theoria motus corporum rigidorum seu solidorum* of 1765 (Euler, 1765), which represents Euler's "mature" view of

¹⁴Translation of: "S'ils disoient que c'étoit par rapport aux étoiles fixes, qu'il falloit expliquer le principe de l'inertie, il seroit bien difficile de les réfuter, vu que les étoiles fixes, étant elles-mêmes en repos, sont si éloignées de nous, que les corps qui se trouvent en repos par rapport à l'espace absolu, comme on le regarde dans la Mathématique, le seroient aussi par rapport aux étoiles fixes. Mais outre cela que ce seroit une proposition bien étrange et contraire à quantité d'autres dogmes de la Métaphysique, de dire que les étoiles fixes dirigent les corps dans leur inertie, cette règle se trouveroit également fausse, s'il nous étoit permis d'en faire l'application aux corps qui sont proches de quelque étoile fixe." (Euler, 1750, p.328)

mechanics (Maltese, 2000), two apparently contradictory views concerning the relativity of motion are expressed. The first chapter involves a detailed criticism of the notion of absolute space, asserting that it is only known by “abstraction”, but if we base ourselves on experience, we find that we can only know relative positions: ¹⁵

Certainly, from the beginning rejecting such slippery abstractions, we ought to consider the thing as it immediately strikes the senses; and when consulting them, we are not permitted to judge otherwise concerning the position of any body, except by referring it to surrounding bodies, [...]

In the second chapter, however, Euler moves on from this purely kinematical discussion to consider the *causes* of motion. Importantly, he distinguishes between *internal* and *external* causes: ¹⁶

For whether a body is at rest or in motion, whether it persists in rest or receives motion and continues it in whatever manner, these phenomena must necessarily proceed from definite causes. [...] whatever reason there is, it must either be contained in the body itself about which the inquiry is made, or it must be sought outside of it; whence two kinds of principles, by which the motions of bodies are defined, must be established, of which the one are called *internal*, the other *external*.

In order to consider the *internal* cause of motion, Euler explains that it is necessary to be able to abstract away from external causes, and so consider “some body clearly solitary, and as if all the remaining bodies have been removed from the universe”, however he acknowledges that this notion may be problematic (Euler, 1765, cap. 2, §77). Once it is possible to consider a body abstracted away from all others, Euler explains that it is necessary that in this state it either be at rest or in motion. But since there are no other bodies with respect to which its state of motion can be defined, Euler has to introduce absolute space (Euler, 1765, cap.2, §80).

¹⁵translation of: “A principio certe hujusmodi lubricas abstractiones repudiantes, rem prouti in sensus immediate incurrit, perpendere debemus, quos consulentes de loco cujuspiam corporis aliter judicare non licet, nisi id ad alia corpora circumjacentia referendo, quorum respectu, quam diu id eundem situm servaverit, id in eodem loco perseverare, sin autem in alium situm pervenerit, locum mutasse pronunciare solemus.” (Euler, 1765, cap.1, §2)

¹⁶Translation of: “Sive enim corpus quiescat, sive moveatur, sive in quiete perseveret, sive motum accipiat, eumque quomodocunque continuat, haec phaenomena a certis causis proficisci necesse est. Quicquid scilicet in corpore ratione motus vel quietis contingit, id temere ac sine ulla ratione fieri, nullo modo statui potest. Quaecunque autem fit ratio ea, vel in ipso corpore, de quo quaeritur, inest necesse est, vel extra id sit quaerenda; unde duo genera principiorum, quibus motus corporum definiantur, constitui debent, quorum illa *interna*, haec vero *externa* appellantur.” (Euler, 1765, cap. 2, §76), emphasis in original.

After defining his law of inertia in both the cases of a body at rest and one in motion (axioms 2 and 3, respectively §82 and §85), Euler proceeds to explain that the denial of absolute space will make it impossible to even formulate laws of motion:¹⁷

Whoever wishes to deny absolute space falls into the gravest difficulty. For since he must reject absolute motion and rest as if they were empty and meaningless sounds, he must not only reject the laws of motion which rest upon this principle, but is also compelled to affirm that no laws of motion can be given at all. For if the question which has brought us to this point—*what would happen in a body separated from the connection with all others?*—is absurd in itself, then even those effects which might be produced in it by others will be uncertain and indeterminate, and thus all things would have to be supposed to occur rashly and without any reason. Or if he wishes to escape this, he will be forced to deny all motion, but in such a doctrine, even if he were to refute successfully every argument against it, he could not possibly rest content, since he would not even be able to say what rest is, which he would then be asserting throughout the whole world. But to resist such manifest absurdities seems most of all to establish the foundation of our own position.

It is necessary for Euler to consider how bodies might behave in the absence of any others, since he considers the cause of inertial motion as being “*internal*” to the bodies themselves. As we will see later on in this chapter and the next, the Machian critique of classical mechanics consists essentially in a rejection of Euler’s distinction between “*internal*” and “*external*” causes. According to the account which we will develop, all causes of motion are *external* since, after all, motion is performed relative to the external environment. Euler was forced to admit the reality of absolute space in order to make his “*internally*” caused motion possible. But in so doing he at the same time introduced a new *external* reference which bodies keep even if all other bodies are abstracted away (absolute space), thus undermining the claim that inertial motion is caused internally. To interpret classical mechanics along the lines of

¹⁷Translation of: “81. Qui spatium absolutum negare voluerit, in gravissima incommodo delabitur. Cum enim motum et quietem absolutam tanquam vanos sine mente sonos rejicere debeat, non solum leges motus, quae huic principio innituntur, rejicere debet, sed etiam ne ullas quidem motus leges dari affirmare cogitur. Namque si ea, quae nos huc perduxit quaestio, *quid in corpore a nexu reliquorum separato sit eventurum?* per se sit absurdum, etiam ea, quae ab aliis in eo effici possent, per se incerta et indeterminabilia erunt, sicque omnia temere ac sine ulla ratione evenire essent statuenda. Vel si haec effugere velit, motum omnem negare debebit, qua tamen in sententia, etiamsi omnia argumenta contra; eam alitata feliciter refutaverit, minime acquiescere poterit, cum ne dicere quidem valeat, quid sit quies, quam per totum mundum constituerit. Sed contra tam apertas absurditates pugnare simillimum nostrae sententiae fundamentum videtur.” (Euler, 1765, cap.2, §81)

Mach's principle, as we will see later on, is then to interpret absolute space as a physically real entity which is the external cause of inertial motion.

Euler's view of mechanics is not substantially different to how classical mechanics is understood today, although people now are more inclined to deny a real distinction between rest and uniform motion in this system. Euler rightly pointed out that it is necessary to have some definition of uniform rectilinear motion in order to even define force. At the same time, Euler was actively reckoning with the critiques made by philosophers of the notion of absolute motion due to its undetectability. The apparent contradiction in Euler's account in between the first two chapter of *Theoria Motus* reflects the difficulty which theorists continued to face with these ideas in the 18th century.

3.2.3 Objection to Euler

The answer which we develop in this dissertation is that the tension we have just spoken about can be resolved if we simply continue to carry through the distinction between the *empirical* regime in which absolute space is inaccessible and the *methodological* necessity of the concept in the definitional framework of classical mechanics. The difficulty faced by theorists in reconciling the empirical relativity of motion with the methodological necessity of absolute motion in classical mechanics can be put down to an insufficient insistence on the distinction between *a priori* and *a posteriori* sources of knowledge.

On the one hand, the principle of inertia represents an *empirical* state of affairs: it is found that if a solid body is sufficiently remote from the dynamical influences of its changing environment, it will move uniformly and rectilinearly with respect to that same environment, which can in this case, for convenience, be approximated as at rest, and homogeneous in time and space. Additionally, the causes of deviations from this rectilinearity and uniformity can in most cases easily be identified as originating in the influence of nearby matter according to known lawful interactions. These considerations do not establish an empirical basis for a distinction, not between “internally” and “externally” caused motion, but rather between two types of externally caused motions, the first of which—for the purposes of scientific model building—can be suitably idealised as homogeneous, and the second of which for the same purposes presents as inhomogeneous and can be traced to inhomogeneities in the environment.

On the other hand, the law of inertia can be regarded, contrary to this, as part of the *definitional framework* of classical mechanics, such that it will hold *a priori* and apply necessarily and universally, regardless of the situation. However, in this case there is an important caveat: if the law of inertia is considered as a definition *a priori*, it is not permitted for us

to say that this law only applies to a particular set of reference frames, since these frames of reference are not knowable *a priori* but must be determined empirically. For this reason, we can employ this definitional framework only if we recognise the process of *gauge fixing* to the empirically known “inertial frames” mentioned previously as something carried out *a posteriori*. *a priori* however, the law of inertia so conceived must apply equivalently to all reference frames, but this necessitates the introduction of a covariant *inertial field* which will act as an *external* cause of inertial forces in (empirically) non-inertial reference frames (such as accelerating or rotating reference frames).¹⁸

In Euler’s system, in the absence of all other bodies, a body moves in accordance with its *internal* causes: rectilinearly and uniformly. However, this presupposes a space and time existing apart from this body with respect to which this rectilinearity and uniformity will be defined; it is impossible for Euler or Newton to define their internal cause independently of a reference to space, i.e. to something external to the body. The situation considered of a universe containing only a single body is never in fact reproduced or approximated in experience, however even in a universe filled with many bodies like our own, absolute space and time play a major role in conditioning the motion of bodies such that it is easy to (approximately) experimentally determine the preferred set of inertial reference frames that they establish. Thus absolute space and time possess a property that is empirically observable, such that—in accordance with the definition of “matter” we have adopted in this thesis—we are forced to admit that absolute space and time—insofar as they establish a preferred set of reference frames—represent something *material*. But if this is the case, it is no longer appropriate for us to say that the cause of inertial motion is *internal* to bodies, since this takes place only in the presence of the material structure which establishes the preferred set of reference frames of “absolute space and time.” The distinction between *internal* and *external* causes of motion is thus illegitimate, in both cases the motion of a body is performed with respect to its environment, and the cause of this motion must be found in the relationship that the body has to its environment.

3.2.4 Kant’s inaugural dissertation

In the century following Euler, it appears that Newton’s and Euler’s doctrines were not subjected to the same level of direct criticism as had initially been stimulated by the publication of Newton’s *Principia*. This, however, is not to say that the period lacked substantial intellec-

¹⁸For now we only consider *rigid* reference frames since systems of coordinates which do not correspond to rigid frames lead to a different representation of the causal relations among spatially separated bodies. Questions concerning the viability of the use of a rigid reference system will be addressed in §6.3.

tual developments. A particularly notable figure in this regard is of course Kant, who became enormously influential as a philosopher. Indeed, many of the most important thinkers of the mid to late nineteenth century—those responsible for shaping the intellectual conditions that ultimately led to the surpassing of the classical paradigm—were themselves deeply influenced by Kant or engaged directly with the Kantian tradition. We will only give a brief overview of Kant’s contribution to this discourse here, there is a vast secondary literature of course on Kant that the reader can consult for more details.¹⁹

In his early writings, Kant was an Leibnizian relationalist, whom Euler would have characterised as a “philosopher” with a “metaphysical view”, accepting only the reality of relative motions. However, in the 1760s, in part due to the influence of Euler’s arguments, Kant decides to accept the reality of absolute space (Kant, 1768), and attempts to provide his own philosophical argument for its existence (whereas he characterises Euler’s arguments as “a posteriori”). Kant argues that in the absence of an absolute space there is no *internal* way to distinguish between a left hand and a right hand, since the mutual relations among their parts are the same, despite their incongruence. It is not clear that this argument is correct, since in response to this we could say that we would have no need to distinguish these two unless they occupied the same universe, at which point a distinction is possible, precisely due to their incongruence.

Two years after this publication, Kant reached his final view of space, which he would maintain throughout his “critical” period. According to Kant, space is not a physical reality, but a pre-conceptual condition which makes spatial representations possible. Kant first expresses this view in his *inaugural dissertation* of 1770, written for his appointment as Full Professor of Logic and Metaphysics at the University of Königsberg (Kant, 1894, p.65-66):

Space is not something objective and real, neither substance, nor accident, nor relation; but subjective and ideal, arising by fixed law from the nature of the mind like an outline for the mutual co-ordination of all external sensations whatsoever.

Those who defend the reality of space either conceive of it as an absolute and

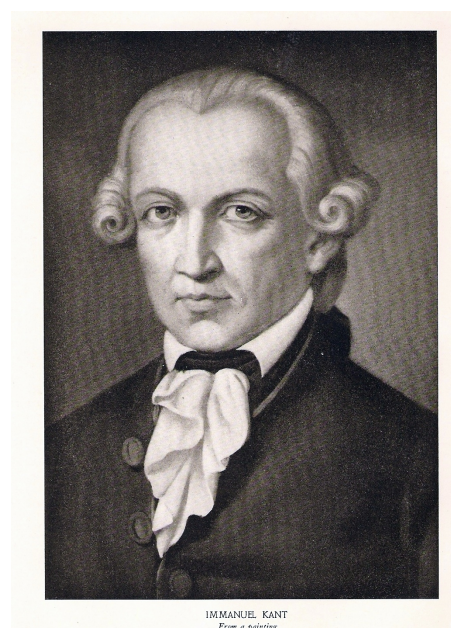


Figure 3.4: Immanuel Kant.

¹⁹See for instance: Friedman (1992, 1986, 2001); Watkins and Stan (2003); Watkins (1997, 2019); Stan (2009, 2013); Bird (2006).

immense receptacle of possible things, an opinion which, besides the English, pleases most geometricians, or they contend for its being the relation of existing things itself, which clearly vanishes in the removal of things and is thinkable only in actual things, as besides Leibnitz [*sic*], is maintained by most of our countrymen.

Essentially, Kant is trying to mark out a position in between that of Newton and that of Leibniz, rejecting both Newton's realism and Leibniz's relationism which would have space disappear in the absence of bodies. Kant continues:

The first inane fiction of the reason, imagining true infinite relation without any mutually related things, pertains to the world of fable. But the adherents of the second opinion fall into a much worse error. Whilst the former only cast an obstacle in the way of some rational or noumenal concepts, otherwise most reconcilable, such as questions concerning the spiritual world, omnipresence, etc., the latter place themselves in flat opposition to the very phenomena, and to the most faithful interpreter of all phenomena, to geometry. For, not to enlarge upon the obvious circle in which they become involved in defining space, they cast forth geometry, thrown down from the pinnacle of certitude, into the number of those sciences whose principles are empirical.

Kant's rejection of Leibniz's doctrine as we see is connected to his efforts, which are central to his later critical period, to give a satisfactory account of the apparently *synthetic a priori* status of geometrical propositions:

If we have obtained all the properties of space by experience from external relations only, geometrical axioms have only comparative universality, such as is acquired by induction. They have universality evident as far as observed, but neither necessity, except as far as the laws of nature may be established, nor precision, except what is arbitrarily made. [...] Though the concept of space as an objective and real thing or quality is imaginary, it is nevertheless in respect to all sensible things not only perfectly true, it is the foundation of truth in external sensibility. Things cannot appear to the senses under any form but by means of a power of the soul co-ordinating all sensations in accordance with a fixed law implanted in its nature.

Kant therefore recognises that simply saying that space is the "order of coexisting things" as Leibniz did is not sufficient since it does not adequately account for the *a priori* status of

geometry. As we saw in the previous chapter, the *a priori* status of Euclidean geometry can be adequately accounted for in terms of Delboeuf's idea of *homogeneity*, which is taken as fundamental to the *method* of science; Delboeuf's view is concordant with Kant's assertion that the geometric axioms are "the foundation of truth in external sensibility."

3.2.5 The paradox in Kant's metaphysical foundations

The particular problem which we deal with in this chapter—concerning the dual status of the law of inertia—is not discussed at much length in Kant's work. He does acknowledge the problem with great clarity however in one particular instance in the *Metaphysical Foundations of Natural Science* of 1786, following a discussion of the difference between rectilinear and circular motion with regards to the relativity of space:²⁰

Newton's scholium to the definitions that he prefixed to his *Principia* may be consulted toward the end on this matter, from which it becomes clear that the circular motion of two bodies around a common centre (and therefore also the axial rotation of the Earth), even in empty space, thus without any comparison with external space that could be possible through experience, can nevertheless be recognized through experience; that therefore a motion, which is a change in the external relations in space, can be empirically given, although this space itself is not empirically given and is no object of experience. This is a paradox that deserves to be resolved.²¹

²⁰I thank Matthias Schemmel for alerting me to this particular passage from the *metaphysical foundations*. Prior to this I had been under the impression that Kant never acknowledged the problem in his work. The passage cited is the author's translation of the original: "Newton's Scholium zu den Definitionen, die er seinen *Princ. Philos. Nat. Math.* vorangesetzt hat, gegen das Ende, hierüber nachgesehen werden, aus welchem erhellt, dass die Kreisbewegung zweier Körper um einen gemeinschaftlichen Mittelpunkt (mithin auch die Achsendrehung der Erde), selbst im leeren Raume, also ohne alle durch Erfahrung mögliche Vergleichung mit dem äusseren Raume dennoch mittelst der Erfahrung könne erkannt werden, dass also eine Bewegung, die eine Veränderung der äusseren Verhältnisse im Raume ist, empirisch gegeben werden könne, obgleich dieser Raum selbst nicht empirisch gegeben und kein Gegenstand der Erfahrung ist, welches Paradoxon aufgelöst zu werden verdient" (Kant, 1786b, Phenomenology, Proposition 2, Remark, Ak. 4:558).

²¹That the problem concerning the the law of inertia arises as a natural consequence of the Kantian view of space and time was also later recognised by Reichenbach (1965, p.8-9): "From the Kantian point of view, according to which space and time are only forms of order and not part of nature such as matter and forces, this principle [of coordinate invariance] is actually obvious. It is strange that philosophers have not long ago pointed out in objection to Galileo's and Newton's laws and also to the special theory of relativity, that the postulated invariance is not sufficient. There is no reason for the philosopher to single out the uniform translation. As soon as space is characterized as a scheme of order and not as a physical entity, all arbitrarily moving coordinate systems become equivalent for the description of events."

Later in the chapter, Kant attempts to resolve the paradox by appealing to the relative motion of the parts of a rotating body:²²

But although this motion involves no change in relation to empirical space, it is nevertheless not an absolute motion, but rather a continuous change of the relations of matters to one another. [...] This rests on the representation of the mutual continuous *distancing* of every part of the Earth (outside the axis) from every other part lying opposite to it at the same distance from the centre in the diameter. For this motion is real in absolute space, in that the decrease of the imagined distance that gravity alone would produce in the body—and indeed without any dynamically repelling cause (as can be seen from the example chosen by Newton in *Principia Philosophiae Naturalis*, p. 10, edition of 1714)—is continuously replaced by a real motion that is confined within the moving matter (namely the centre of the same) and not referred to external space.

This solution however ultimately rests on the same fallacious reasoning as Huygens and Berkeley has both engaged in before in their efforts to resolve the same problem, i.e. Kant is conflating *velocities* with *accelerations*. The “distancing” of the parts of a rotating sphere from one another, is not a constant motion or velocity, rather it is a constant acceleration or *change in motion*. Therefore this distancing is to be regarded—according to the conceptual setup of classical mechanics—as an *effect* of the circular motion of the body, not as the cause of other effects. The question concerning how this circular motion is to be known in the absence of absolute space then remains unresolved.²³

3.3 Splitting the seams

After a period of relative silence on the issue during the first half of the nineteenth century, the final third of the century saw a substantial revival of interest in the epistemological foun-

²²Translation of: “Dass aber diese Bewegung, ob sie gleich keine Veränderung des Verhältnisses zum empirischen Raume ist, dennoch keine absolute Bewegung, sondern continuirliche Veränderung der Relationen der Materien zu einander, obzwar im absoluten Raume vor- gestellt, mithin wirklich nur relative und sogar darum allein wahre Bewegung sei, das beruht auf der Vorstellung der wechselseitigen continuirlichen Entfernung eines jeden Theils der Erde (ausserhalb der Achse) von jedem andern ihm in gleicher Entfernung vom Mittelpunkte im Durchmesser gegenüber liegenden. Denn diese Bewegung ist im absoluten Raume wirklich, indem dadurch der Abgang der gedachten Entfernung, den die Schwere für sich allein dem Körper zuziehen würde, und zwar ohne alle dynamische zurücktreibende Ursache (wie man aus dem von Newton Princ. Phil. Nat. pag. 10. Edit. 1714 gewählten Beispiele ersehen kann), mithin durch wirkliche, aber auf den innerhalb der bewegten Materie (nämlich des Centrum derselben) beschlossene, nicht aber auf den äusseren Raum bezogene Bewegung, continuirlich ersetzt wird” (Kant, 1786b, Ak. 4:562).

²³See Stan (2016) for further discussion of Kant’s paradox and of his attempt at solving it.

dations of the law of inertia. In this section we will consider the contributions of Carl Neumann, Thompson & Tait and Ludwig Lange, which were all roughly contemporaneous with Mach. The contributions of Neumann and Lange will be particularly important for the next chapter as we will be contrasting Mach’s approach to these and critiquing both Neumann’s and Lange’s proposals accordingly in section 4.1. We distinguish two central strategies that emerged in this period: (1) that of Neumann which involves insisting that inertial motion must be referred to some material cause, and (2) that of Lange as well as Thomson and Tait, which involves analysing the law of inertia in order to pick apart the *conventional* from the *empirical-hypothetical* part.

3.3.1 Neumann’s ‘Body Alpha’

In his inaugural address to the University of Leipzig in 1869, the mathematician Carl Neumann discusses the foundations of mechanics and in particular, raises the question of whether the law of inertia is adequately defined.²⁴ Neumann remarks that no given body in space is adequate to once and for all determine the definition of inertial motion, since “a motion that appeared to be rectilinear when watched from the earth would appear curvilinear when watched from the sun” (Neumann, 1993). The true content of Newton’s reference to ‘absolute space’ for the definition of rectilinearity consists in the assertion that all motion must be referred to the *same* object:

For the character, the essence of the so-called absolute motion consists (as no one

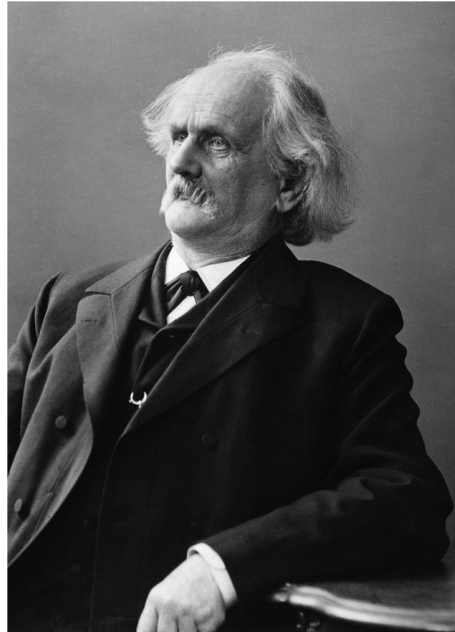


Figure 3.5: Carl Neumann.

²⁴For further discussions of Neumann’s contribution, see especially Thébaud (2021); Darrigol (2021a); Pulte (2009). Pulte in particular has brought attention to the influence on Carl Neumann of Carl G. J. Jacobi, who had founded the Königsberg seminar for mathematics and physics—along with Carl Neumann’s father Franz Ernst Neumann—which Pulte describes as “the ‘nucleus’ of German theoretical physics in the second half of the 19th century” (Pulte, 2009). The influence of Jacobi on Neumann and the importance of Jacobi more generally is a subject that deserved further attention; as Pulte explains, the Jacobi–Neumann line of investigation brought out reasons for the evolution of mechanics prefiguring the scientific revolution of the next century that were “independent of and prior to the empirical challenges (i.e. the integration of ‘new’ areas of phenomena like those of electrodynamics and thermodynamics), independent of the rise of the debate about Non-Euclidean geometries and also independent of the rise of modern phenomenalism (or empiricism) [i.e. Mach and his associates].”

can deny) in that all changes of place are referred to one and the same object — namely an object that is extended and unchanged although it cannot be assigned more concretely.

For this reason, Neumann introduces the notion of a hypothetical ‘*Body Alpha*,’ which specifies three coordinate axes, with respect to which rectilinear motion should be defined: “a material point left to itself proceeds in a straight line—i.e., in a path that is rectilinear in relation to this *Body Alpha*.” Neumann insists that this ‘*Body Alpha*’ has just as much right to exist as the “luminiferous ether or the electrical fluid”, since it is necessary as an object with respect to which inertial motion is to be defined. Thus Neumann presents three principles of Galilean-Newtonian theory which are given as follows:

- (1) “in some unknown position in space an unknown body exists, and indeed an absolutely rigid body, a body whose figure and dimensions will be immutable for all time.”
- (2) “a material point left to itself proceeds in a straight line — i.e., in a path that is rectilinear in relation to this *Body Alpha*.”
- (3) “Two material points, each of them left to itself, move in such a way that the equal paths of one of them always correspond to the equal paths of the other.”

The first two principles have to do with the definition of *rectilinearity* in Newton’s first law; the ‘*Body Alpha*’ is needed to define the rigid coordinate axes with respect to which particles move inertially. The third principle concerns the question of the *uniformity* of the motion. Neumann does not suppose the existence of an *Alpha Clock*, but rather requires that any two material points in inertial motion must move uniformly with respect to one another. It was on this basis that Lange would criticise Neumann’s solution to the problem of defining the inertial law.²⁵

3.3.2 Identifying empirical content

Another strategy to the question of inertia involves unpicking which parts of this law are hypothetical and which are merely conventional. This involves taking the standpoint of an investigator who naturally only has access to *relative* spatio-temporal information, and deducing from this what is the actual empirical content of the law of inertia. Progress in this direction was made both by Thomson & Tait and Ludwig Lange as we will see.

In their *Treatise on Natural Philosophy* of 1867, William Thomson (later known as Lord Kelvin) and Peter Guthrie Tait accompany their definition of Newton’s first law with the remark that “The meaning of the term *Rest*, in physical science is essentially relative. Absolute

²⁵We will come back to this inconsistency in Neumann’s treatment of the issue in §4.1.

rest is undefinable. [...] But it is remarkable that the first law of motion enables us to explain what may be called directional rest.” Thomson and Tait go on to tease apart which aspect of Newton’s first law is mere “convention” from that part which represents a genuine empirical fact. First of all, they split Newton’s law into two distinct statements [Thomson and Tait \(1867\)](#):

- (1) “The times during which any particular body, not compelled by force to alter the speed of its motion, passes through equal spaces, are equal.”
- (2) “Every other body in the universe, not compelled by force to alter the speed of its motion, moves over equal spaces in successive intervals, during which the particular chosen body moves over equal spaces.”

While the first statement here “merely expresses the convention universally adopted for the measurement of *Time*,” the second statement is an empirical observation which Thomson and Tait characterise as a “great truth of nature”; this is illustrated as follows:

A curling-stone, projected along a horizontal surface of ice, travels equal distances, except in so far as it is retarded by friction and by the resistance of the air, in successive intervals of time during which the earth turns through equal angles. The sun moves through equal portions of interstellar space in times during which the earth turns through equal angles, except in so far as the resistance of interstellar matter, and the attraction of other bodies in the universe, alter his speed and that of the earth’s rotation.

This belongs to the true empirical content of Newton’s law; it is not that bodies move uniformly through absolute space and time (for part of this is pure convention), but rather that they all do so with respect to one another.

In his paper of 1884, titled *On the law of inertia* ([Lange, 1885](#)), drawing both from Neumann and Thompson & Tait, Lange develops this further by proposing a way by which it may be possible to solve this epistemological difficulty while dispensing with the need of absolute space or any substitute such as the ‘Body Alpha’ of Neumann. Lange remarks that in Neumann’s third principle the uniformity of the motions of bodies is defined in terms of the mutual consistency in the behaviour of bodies, without the need to postulate some absolute structure.

We already have a fully valid substitute for the absolute time. [...] We have only, following Neumann, to base the measure of time on the following definition: Two time intervals are said to be equal in which a point left to itself passes through

equal spatial distances. [...] Under this viewpoint, the law of the “uniform” motion of all points left to themselves is, as Thomson and Tait correctly note, a pure convention for one such point, and it is more than convention, it is a research result, only insofar as it applies to any other points left to themselves.

What Lange sets out to do then, is to extend Neumann’s reasoning to the definition of rectilinearity in the inertial law:

The question now arises whether it is possible to eliminate also absolute space by a similar procedure. Indeed this is possible.

Lange observes that for any three given points in an empty space that move arbitrarily, “it is always possible to construct a coordinate system, indeed infinitely many coordinate systems, in relation to which these points move rectilinearly.” On the other hand, if more than three points are considered, this is only the case “under special circumstances, only contingently.” Just as [Thomson and Tait \(1867, §247-248\)](#) had formerly remarked that the uniform motion of a single material point is a *convention*, Lange now asserts that the rectilinearity of the motion of three material points is purely conventional. The true empirical content of the law of inertia thus consists in asserting that given three material points whose motion defines an inertial system, any fourth material point must move rectilinearly and uniformly with respect to the inertial system defined by the first three. In this way, by Applying Neumann’s reasoning in his third law to the definition of rectilinearity, Lange claims to have disposed with the difficulty regarding the definition of inertial motion, the ‘Body Alpha’ is not needed, and neither is absolute space; particles simply travel rectilinearly and uniformly with respect to one another.

The strategy discussed above explicitly ignores the need, which we have defended earlier in this chapter, to identify the *cause* of inertial motion, for which reason Lange rejected Neumann’s appeal to the ‘Body Alpha.’ As we will see in the next chapter, this strategy is not satisfactory, i.e. it is not sufficient simply to say that bodies move rectilinearly and uniformly with respect to one another without identifying the law by which those bodies would condition one another’s motion to ensure this behaviour. Nonetheless, the analysis given by Thomson & Tait and Lange certainly helped bring attention to the fact that the law of inertia indeed contains actual empirical content, since a purely relationalist analysis of this law does not reveal it to be mere convention.

Summary

In summary, the development of classical mechanics reveals a persistent tension between the empirical inaccessibility of absolute space and time and their apparent necessity for defining the distinction between inertial and forced motion. Early critics of Newton, such as Berkeley and Leibniz, attempted to defend the relativity of motion on philosophical grounds, but struggled to account for phenomena such as centrifugal forces. These difficulties were compounded by confusions concerning the relation between velocity and acceleration, which led several critics—including Berkeley, Huygens and later, Kant—to misinterpret Newton’s dynamical arguments.

By the mid 18th century, the success of Newtonian mechanics could not be overlooked, and Euler’s work helped greatly with the elaboration and establishment of the system. Euler defended the reality of absolute space and time on the grounds that they were necessary presuppositions for mechanics. For Euler in particular, this was tied to Newton’s view that inertial motion arises from causes inherent within bodies themselves. Kant, who was influenced by Euler to reject his initial Leibnizian views, developed the notion of the necessity of the concept of space by reinterpreting this as a subjective an ideal but wholly necessary *form* which the mind uses for the representation of phenomena. Kant recognised the “paradox” that this notion of space gives rise to in the face of the observability of absolute circular motion but was unable to resolve the problem successfully (and indeed repeated the error of Berkeley and Huygens).

In the late nineteenth century, renewed attention to the epistemological foundations of mechanics led to more refined analyses of the law of inertia. Figures such as Carl Neumann, Thomson and Tait, and Ludwig Lange attempted either to identify a material reference for inertial motion or to separate the conventional and empirical elements contained in Newton’s first law. These discussions revealed that the classical formulation of inertia combines definitional elements with genuine empirical claims.

The central claim of this chapter is that classical mechanics rests on a conflation between two distinct concepts of space—and correspondingly between two concepts of inertia. On the one hand, there is a methodological notion of space required for the formulation of mechanical laws; on the other, there is the empirically real structure that determines inertial frames. The classical distinction between internally and externally caused motion arises from failing to keep these two aspects separate. The distinction drawn here between an empirical and a methodological sense of the law of inertia is closely related to the distinction developed in chapter 2 between physical and methodological concepts of space. As we will see in the

next chapter, Mach's critique of classical mechanics can be understood as challenging the Newtonian–Eulerian distinction between internally and externally caused motion, replacing it with a more holistic conception in which all motion is equivalently *caused* by the body's relation to its environment, i.e. a rejection of the notion of internal causes.



CAUSAL HOLISM

The Universe as a whole is self-contained, and all its properties must, as it were, be accounted for in terms of itself.

Dennis Sciama, *Modern Cosmology*, 1971

What is the cause of inertial motion?

Introduction

Although established works in the latter half of the 20th century have painted Mach's contribution to the philosophy of physics in a generally disfavoured light (Bunge, 1966; Blackmore, 1972; Stein, 1977; Earman, 1989), more recent scholarship calls upon us to reconsider this verdict (DiSalle, 2002a; Wolters, 2011a; Banks, 2014, 2012; Thébault, 2021; Staley, 2021; de Paz, 2018, 2025). Historically, Mach has always been a divisive figure. In the early 20th century, Mach inspired the formation of the Vienna Circle which led to the highly influential school of logical positivism, while others such as Lenin (1909) abhorred his reduction of physics to sensation, and slandered his school of thought as a form of "reactionary idealism".

One of the major points of contention concerning Mach to this day is the famous "Mach's principle", a principle about which there are as many disagreements over the interpretation as there are differences in opinion concerning its validity and value. To start with, Mach never defined Mach's principle nor even used the term; it was popularised by Einstein, who



Figure 4.2: Ernst Mach.

extracted many diverse and sometimes inconsistent formulations of this idea from Mach’s writings. One central source of controversy concerns whether Mach’s principle proposes the relational definition of (1) inertial motion, or (2) inertial mass. This question will not be the subject of the present paper, instead, we will take for granted the first interpretation.¹ A second source of controversy concerns the question of whether Mach advocated a “mere redescription” of Newtonian mechanics, as Norton (1995) suggests, or whether he endorsed the development of a new hypothetical theory of inertia, as Barbour (1995) argues. While we will side with Barbour, we acknowledge that the controversy here is well-founded. As we shall see, Mach’s positivistic scepticism regarding the

concept of causation was responsible for both: (1) birthing his highly suggestive critique of the inertial law, and (2) obscuring the distinction between his epistemological concerns and his hypothetical speculations. Mach’s reluctance to use explicitly causal language has led to a poorly drawn boundary between what we here call “Mach’s principle” and “the Mach hypothesis”.

The central aim of this chapter is twofold: firstly, we clarify the distinction between Mach’s principle on the one hand, which is a purely formal requirement (see section 4.2.2), and on the other hand, Mach’s hypotheses, of which two possible classes are considered in his work (see section 4.2.2.1 and 4.2.2.2).² The second aim of this paper is to show that all of Mach’s reflections on the law of inertia are fundamentally informed by his critique of the classical conception of causality according to which singular causes bring about singular effects. The fundamental insight which Mach contributes is the suggestion that inertial motion is not, in fact, *uncaused* motion, but might instead be a motion caused by a roughly homogeneous environment. This is discussed in sections 4.1 and 4.2. Mach’s view is rooted in his characteristically *holistic* stance on causality, which goes beyond his specific critique of inertial motion.

¹See Barbour (1981) for a detailed justification of this interpretation, and a discussion of Einstein’s inconsistent formulations of Mach’s principle.

²Note that Guy Burniston Brown (1982, p.3) already made the point that what is usually called “Mach’s principle” is actually a hypothesis, writing: “Mach’s principle (as Einstein named it) is not a principle, i.e. a general law as guide to treatment of a problem. It is a genuine physical hypothesis, [...]”. I did not mention this in my original paper (Fay, 2024a) since I only found Brown’s remark two days after my paper was published.

In later sections of this work, we turn our attention towards some of Mach's more recent critics. Much of the controversy over the interpretation of Mach can be traced back to (1) the failure to distinguish Mach's principle from what I call the *Mach hypothesis*, and (2) the evolution that the content and style of Mach's thought underwent throughout his life. DiSalle's exemplary scholarship (DiSalle, 2002a,b) shows some awareness of both these issues, and presents a sober defence of Mach which deflects the disparaging attitude of prior critics towards Albert Einstein. We finish the chapter with a defence of the Machian status and legitimacy of the early Einstein's research programme.

4.1 Causality and determination

In this section we will bring attention to two issues concerning the concept of causal determination which are critical to understanding the different aspects of Mach's critique of Newton's first law. As we will see, Mach was aware of both these issues, however this is not always made clear in his writings, therefore it is important that we understand these issues clearly before moving on to interpreting Mach's critique.

4.1.1 Two issues

Issue 1 *Distinction between epistemological and causal determination.*

At the heart of the issues raised in our discussion of Neumann and Lange towards the end of the previous chapter was an ambiguity concerning the definition of the word 'determine'. In the first place there is an epistemological question: '*how can I determine the truth of this statement?*' Here the word 'determine' refers to the means by which my knowledge of something is reached. On the other hand, there is a question pertaining to physical causality: '*what is it that determines the motion of this body?*' Here the word 'determine' refers to the physical determination of the future state of a system by its prior state in accordance with the law of causality. Neumann's solution to the problem of the determination of inertial motion involves a conflation between these two. By introducing the 'Body Alpha', Neumann implicitly supposes that it is necessary to consider a cause of the rectilinearity of inertial motion,³ but neglects the task of describing this in terms of objects which we have epistemological access to. On the other hand, in his analysis of the uniformity of inertial motion, we find the

³Indeed Neumann explicitly acknowledges inertial motion as caused motion (Neumann, 1993, p.359): "*Inertia exists and, simultaneously, the attraction of the earth exists as well. In consequence of the combination of both these causes, arises the motion in which the stone traverses the parabolically curved path*" (original emphasis).

converse: Neumann ignores the need for such a causal determination, and focuses only on the task of describing this motion in terms of facts which we have epistemological access to. Lange, on the other hand, is not at all concerned with the postulation of real objects that would be responsible for causally determining inertial motion, but only with distinguishing the empirical content of the law from the conventional part. In this way, Lange leaves unanswered the question of causation but manages to express the content of Newton's law without reference to unobservable structures.

Issue 2 *Neglect of stable environmental factors in classical accounts of causation.*

The second issue concerning the notion of causality is particularly salient to Mach's critique of Newton. This is the idea that changes in the state of a system are not brought about by singular, isolated causes, but rather, it is the entire state of a given system that is responsible for bringing about the subsequent state. It is only our habituation with systems whose environment is relatively stable that leads us to assume that environmental influences can be ignored.

Both of these issues concerning the concept of causation are addressed by Mach in his fifth appendix to the second edition of *Die Mechanik* (1883). Concerning the first issue, Mach writes (Mach, 1893, p.516):

In the text I have employed the term "cause" in the sense in which it is ordinarily used. I may add that with Dr. Carus, following the practice of the German philosophers, I distinguish cause, or *Real-grund*, from *Erkenntnisgrund*.⁴

In his essay *History and Root of the Principle of Conservation of Energy* (henceforth *Conservation of Energy*), initially published in 1872, Mach discusses this second issue in some depth. Throughout the text, Mach traces the many forms of the principle of conservation of energy historically to their derivation from some requirement for the exclusion of perpetual motion. In the final chapter, titled *The Logical Root of the Theorem of Excluded Perpetual Motion*, Mach traces this theorem back to the *law of causality*. Mach emphasises that this law does not refer single causes to single effects, but rather (Mach, 1872, p.63):

the totality of the phenomena on which a phenomenon α can be considered as dependent, [should define] *the cause*.

⁴Mach's reference to "the practice of German philosophers" is quite appropriate since what Mach is saying concerning the law of causality was well understood by many philosophers in his day. For instance, as early as 1813, Schopenhauer discussed the very same two issues in his doctoral dissertation (Schopenhauer, 1813).

Thereby Mach expresses the law of causality as: “the effect is determined by the cause” (“die Wirkung ist durch die Ursache bestimmt”).

4.1.2 Archimedes’ proof and Mach’s changing language

To illustrate the colloquial misapplication of the causal law, or of a similar law, that of ‘sufficient reason’, Mach recounts Archimedes’s justification of the equilibrium of a scale which on both sides holds equal weight. Archimedes explains the observed lack of motion in terms of the absence of any *reason* why the bar should “turn in one direction rather than in the other.” Mach insists however that this is not expressed correctly:⁵

Only this is not expressed quite properly: we ought rather to say that there is a reason that, in these cases, nothing happens. For the effect is determined by the cause, and the one and only effect which is here determined by the cause is no effect at all. (Mach, 1872, p.66)

By the time he publishes *Die Mechanik* in 1883, Mach’s view seems to have changed a little. We have already mentioned his note from the appendix in which he agrees with Carus’s characterisation of cause and effect as “to a great extent arbitrary” (Mach, 1893, p.516). This view is more elaborately expressed in chp. IV, §IV, para. 3:

In speaking of cause and effect we arbitrarily give relief to those elements to whose connection we have to attend in the reproduction of a fact in the respect in which it is important to us. There is no cause nor effect in nature ; nature has but an individual existence; nature simply is.⁶ (Mach, 1893, p.483)

Mach goes on to discuss the views on causality of Hume, Kant and Schopenhauer, and situates himself closest to that of Hume. To make it even more clear that Mach’s thinking concerning causality had evolved since 1872, we need only look as far as chp. I, §I, para. 2 of *Die Mechanik*, in which Mach discusses exactly the same justification for the equilibrium of a scale by Archimedes as he had discussed in *Conservation of Energy*. In his treatment of the issue now however, Mach makes absolutely no mention of the word ‘cause’, although, just as in *Conservation of Energy*, Mach directs his criticism towards the neglect of the surrounding circumstances (Mach, 1883, p.9):

⁵This passage also demonstrates that Mach had a broader, less anthropomorphic conception of causality than Leibniz, since Leibniz raises no objections to Archimedes’ use of the principle of sufficient reason in his own treatment of the passage (Clarke, 1717, para. 1). As we will argue, it is this very departure from the traditional anthropomorphic notions of force and inertia that enables Mach, unlike Leibniz, to develop a relationalist resolution of the problem of inertial forces.

⁶The words used for “cause and effect” in the original German text are: “Ursache und Wirkung”.

We might suppose that this [equilibrium] was self-evident entirely apart from any experience, agreeably to the so-called principle of sufficient reason; that in view of the symmetry of the entire arrangement there is no reason why rotation should occur in the one direction rather than in the other. But we forget, in this, that a great multitude of negative and positive experiences are implicitly contained in our assumption.

The “negative” experiences referred to here are the circumstances which experience has taught us play no role in the determination of the equilibrium, such as the “colors of the lever-arms” or “the position of the spectator”. The “positive” experiences on the other hand are the other neglected determinative circumstances such as the distances of the weights from the supporting point. Mach then concludes that:

By the aid of these experiences we do indeed perceive that rest (no motion) is the only motion which can be uniquely determined, or defined, by the determinative conditions of the case.⁷ (Mach, 1883, p.10)

Which is an analogous conclusion to that which Mach pronounced in 1872: “the effect is determined by the cause, and the one and only effect which is here determined by the cause is no effect at all” (Mach, 1872, p.66), except that by 1883 Mach has dropped any reference to the concept of causation, but chooses instead only to use the word “determined”.

It is clear from this comparison to his earlier work that much of the content of Mach’s views on causality has not changed; he is still chiefly concerned about the neglect of environmental circumstances (“negative and positive experiences”), in our colloquial use of sufficient reason or causality. However, by the time he comes to write *Die Mechanik*, Mach more fully embodies his mature positivist style, and therefore shows a much greater reluctance to use the word “cause”. This point should be kept in mind when we move on to analysing Mach’s critique of the law of inertia which will be covered in the next chapter, in particular, we should remember that when Mach refers to the notion that the motion of a body is *determined* by such and such a circumstance, he is not merely making an epistemological claim. Rather, he is referring to that which in 1872 he would have called a *cause* (i.e. “*Ursache*” or “*Real-grund*”) of the phenomenon.

⁷Here the German word which is translated as “determined” is “bestimmte”.

4.2 On Mach's hypothesis

In the context of 19th century discussions of *force*, the following two quotes by Poisson and Maxwell give us a good impression of the generally accepted view. [Poisson \(1811\)](#) expresses himself as follows:

if we consider a body at the instant it passes from a state of rest to a state of motion, we may always observe, that this change is owing to the action of an extraneous cause, [...] Any cause which excites motion in a body, [...] is called force.

Similarly, [Maxwell \(1876\)](#) defines a force as:

in every case in which we find an alteration of the state of motion of a body, we can trace this alteration to some action between that body and another, that is to say, to an external force.

But for a 'change' or 'alteration' to be able to take place, there must be some standard by which motion will be determined in the absence of such changes. By necessity therefore, we require some '*law of inertia*'.

Now if we apply Mach's criticism of the neglect of environmental circumstances (exemplified by his treatment of Archimedes' proof) to the traditional conception of *force*, we find that we will need to call into question this paradigmatic distinction between inertial and forced motion. Are we truly ever justified in isolating the influence of a single body on another? Who is to say what would happen to the two bodies if the rest of the world did not exist?⁸ By singling out the effect of a single body and regarding the rest as negligible, we commit an epistemological *sin*: the neglect of the stable background environment, which we implicitly assume can play no part in the causal determination of the motion considered. Mach therefore proposes that the inertial law does not define motion in the absence of causes, but rather represents the motion caused by the stable background environment constituted of the distant masses of the universe.

If the *variability* in motion of a body can be traced, as Maxwell states, to a corresponding change in another body, might we suppose that the *invariance* of inertial motion, which, after all, we define epistemically with respect to the fixed stars, is a consequence of the relative invariance of this same cosmic environment? This is the intuition that underlies Mach's famous hypothesis. In this way, it is due to his enlarged conception of causation, his insistence

⁸Poincaré raises very similar questions concerning the definitions of forces and inertial motion in [Poincaré \(1902b, chp.VI\)](#).

that the *environment* cannot be neglected when we consider the cause of a given motion (issue 2), that Mach hypothesises the action of the distant stars in the determination of inertial motion. This becomes apparent in his reflections shortly following his discussion of the hypothesis in *Die Mechanik*:⁹

The most important result of our reflexions is, however, *that precisely the apparently simplest mechanical principles are of a very complicated character, that these principles are founded on uncompleted experiences, nay on experiences that never can be fully completed, that practically, indeed, they are sufficiently secured, in view of the tolerable stability of our environment.* (Mach, 1883, p.237)

By referring to “uncompleted experiences” (“unabgeschlossenen [...] Erfahrungen”), Mach means to say that our experiences of the phenomenon of inertia does not include both sides of the relation. Since absolute space is not an object of knowledge, whatever real thing it stands for in our mechanics has not been sufficiently mapped out by our experience.

The interpretation of Mach’s views given so far follows logically from the above discussion of his views on causality expressed in his early writings. However, the interpretation of Mach is a contentious issue; moreover, DiSalle (2002a) and Thébault (2021) in particular have raised the importance of placing Mach’s writings in the context of the evolution that his thought underwent throughout his life. As Thébault (2021) points out, there is a split in the secondary literature concerning Mach’s critique of inertia, where DiSalle and Norton (1995) argue that Mach is “proposing a *redescription* of Newtonian mechanics without absolute concepts”, while Barbour (1995) “takes Mach to be proposing a *new theory of inertia*”. Indeed, Norton (1995) traces this split in opinion all the way back to the time of Mach’s own writings, at which time Paul Carus endorsed the “mere redescription” interpretation, opposing himself to the interpretations of Philip Frank and others. In this paper, we defend an interpretation of Mach that aligns more with that of Barbour; the opposing view of Norton will be addressed in section 4.2.2, while the somewhat more subtle account given by DiSalle will be examined in section 4.3.

4.2.1 Mach’s principle: inertia in rotating frames

Unlike Neumann and other commentators on the law of inertia who took for granted the empirical validity of this law, Mach was the first to seriously raise the question of whether this law might only be right by approximation. Mach’s first published comments on this issue

⁹Emphasis in original.

appeared in the *Notes* to his formerly discussed *Conservation of Energy* (Mach, 1872). Having addressed this issue prior to Neumann in a series of lectures in 1868, Mach felt emboldened to publish his thoughts in print following Neumann's inaugural address of 1870. Although Mach finds that he and Neumann had identified exactly the same difficulties with the definition of the inertial law, he insists that their solutions differ.

The difficulty which Mach refers to is expressed as follows (Mach, 1872, p.76-77):

Obviously it does not matter whether we think of the earth as turning round on its axis, or at rest while the celestial bodies revolve round it. Geometrically these are exactly the same case of a relative rotation of the earth and of the celestial bodies with respect to one another. Only, the first representation is astronomically more convenient and simpler. But if we think of the earth at rest and the other celestial bodies revolving round it, there is no flattening of the earth, no Foucault's experiment, and so on—at least according to our usual conception of the law of inertia. [...] The law of inertia must be so conceived that exactly the same thing results from the second supposition as from the first.

About a decade later, in his first edition to *Die Mechanik* (1883), Mach expresses the same concern (Mach, 1883, p.232):

Relatively, [...] the motions of the universe are the same whether we adopt the Ptolemaic or the Copernican mode of view. Both views are, indeed, equally correct; only the latter is more simple and more practical.¹⁰ [...] The universe is not twice given, with an earth at rest and an earth in motion; but only once, with its relative motions, alone determinable. It is, accordingly, not permitted us to say how things would be if the earth did not rotate. We may interpret the one case that is given us, in different ways. If, however, we so interpret it that we come into conflict with experience, our interpretation is simply wrong. The principles of mechanics can, indeed, be so conceived, that even for relative rotations centrifugal forces arise.

In both cases, we see that Mach is asserting a *kinematical* equivalence between two situations, and insisting that our laws be expressed in such a way that the same *dynamical* evolution be observed. In *Die Mechanik*, which is written a decade later than the first, Mach shows more caution, emphasising that if experience were shown to be inconsistent with the

¹⁰It is worth noting that the equivalence of the Copernican and Ptolemaic perspective had already been recognised by Leibniz in his essay of 1689 *On Copernicanism and the Relativity of Motion* (Leibniz, 1989a) who, like Mach, appealed to the notion of simplicity in defence of the Copernican view.

Ptolemaic picture, the imposition of an equivalence ought to be abandoned. However, Mach subsequently aims to show that it is possible to formulate the laws of mechanics in such a way that the rotation of our frame becomes a genuine symmetry of the system.¹¹

Since this equivalence is only a *formal* requirement, and does not place constraints upon the possible content of our physical laws, we cannot call it a *hypothesis*. Rather, we may call it ‘*Mach’s principle*’, and define it as follows:

Principle 1 *The dynamical evolution of an isolated system depends only upon its relational kinematical configuration.*

In other words, it is the observable relations between the parts of a system that have causal power, not the values of certain unobservable absolutes.¹²

The immediate corollary of this principle is that the laws of physics ought to be invariant under transformations of our coordinates that keep the relational configuration of real things invariant, as Mach recognises.

Corollary 1.1 *The laws of physics ought to take the same form under any reference frame related to an inertial frame by a rigid transformation.*

Since this principle does not specify the content of this relational kinematical configuration, it in no way constrains the possible content of physical laws. For instance, Newton’s mechanics may be saved if we simply regard “absolute space” as a physical entity distinct from geometrical space, or else imagine that Neumann’s privileged ‘Body Alpha’ exists.

4.2.2 Mach’s hypotheses

The problem which Mach had identified in the definition of the law of inertia does not lead directly to Mach’s hypothesis about the fixed stars, in both *Conservation of Energy* and *Die Mechanik*, Mach is more cautious than to claim this. In *Conservation of Energy*, Mach identifies two possible solutions to the problem:

1. That which Neumann prefers: that “all motion is absolute,” or referred to some hypothetical ‘Body Alpha’.

¹¹As Reichenbach (1965, p.8) remarks, the need to generalise the relativity of motion to rotating frames of reference is a natural consequence of the Kantian conception of space. Although Mach approached the question from a strictly empiricist standpoint, it is worth remarking that Mach was deeply influenced by Kant, in particular by Hatfield (2004); see Mach (1890, p.65–66) and Mach (1872, p.16).

¹²This type of formulation of Mach’s principle was recognised by Barbour and Bertotti (1982) who drew from Poincaré’s reflection in (Poincaré, 1913, p.83–85, p.107–114) to formulate a more precise expression of Mach’s principle. For a discussion of Poincaré’s contribution, see Mercati (2018).

2. That the law of inertia is wrongly expressed, and in particular that “in its expression, regard must be paid to the masses of the universe.”

In the first edition of *Die Mechanik* (1883), we find a similar choice of two possible approaches to the problem, one which would secure Newton's law and another which would imply an alternative formulation. These will each be discussed in what follows.

4.2.2.1 First hypothesis: stars as collateral

The first possible solution to the problem of the origin of inertial motion is presented in *Die Mechanik* prior to the passage quoted above. After directing a purely epistemological critique at Newton's formulation of the law of inertia, by insisting that the empirical basis of our knowledge of inertial motion always consists of a relative motion between the given body K and the other bodies in the universe $A, B, C, \dots$, Mach proposes a possible solution to the problem ([Mach, 1883](#), p.230-231):

It might be, indeed, that the isolated bodies $A, B, C, \dots$ play merely a collateral role in the determination of the motion of the body K , and that this motion is determined by a *medium* in which K exists. In such a case we should have to substitute this medium for Newton's absolute space. [...] In itself such a state of things would not belong to the impossibilities.

Although the transition here from a question of epistemological determination to a question of causal determination is not made explicit as Mach again avoids using the word ‘cause’, we can safely assume that he is referring to a causal determination or ‘*Real-Grund*’, since the terms “determined” (“bestimmt wäre”) is here used in a very similar context as his discussion of Archimedes's proof. Furthermore, if Mach were merely concerned with providing an account of our epistemological determination of inertial frames, he would have no reason to appeal to the hypothetical existence of this “medium”, to which we do not even have direct epistemological access.¹³

Instead of the absolute position and velocity of bodies being responsible for *causally* determining their evolution, Mach proposes that this may be determined by a form of relative motion and position; a motion relative to a hypothetical medium. By considering this hypothesis Mach demonstrates that he is not so naive as to think that inertial motion must be

¹³This in itself is enough to show that the view of [Norton \(1995\)](#) according to which Mach is proposing a “mere redescription” of Newton's mechanics, and is not concerned with identifying a material cause of inertial motion, is mistaken. We will return to this in section [4.2.3](#).

causally determined by some relation between objects which we already have direct epistemological access to, but rather, that this motion should be determined by some relations which are *in principle* epistemically accessible. Indeed Mach goes on to praise the potential fruitfulness of this hypothesis if its pursuit might allow us to discover the other physical properties of this medium.

Hypothesis 1 *The motion of bodies observed as rectilinear and uniform with reference to the fixed stars, which is identified as ‘inert’ in classical mechanics might be caused by the action of a medium in which these bodies and the stars are embedded.*

In later editions of *Die Mechanik*, Mach likens this *medium* solution to that of Budde (1890, p.133-136), who, like Mach, was troubled by the idea of having a preferred class of reference frames without these being determined by some relation to a real thing. Budde postulates that space is some kind of medium which is responsible for determining the motion of the bodies contained. Mach simply adds to this in his appendix (Mach, 1893, p.547):¹⁴

I have no objections to Budde’s conception of space as a sort of medium (compare page 230), although I think that the properties of this medium should be demonstrable physically in some other manner, and that they should not be assumed *ad hoc*.

4.2.2.2 Second hypothesis: stars as fundamental

Having covered the possibility of the existence of this medium, Mach puts aside this particular hypothesis in order to consider his own favored hypothesis according to which a relation to the distant stars would play a “fundamental” (“wesentliche”) role rather than a “collateral” (“zufällige”) role in the determination of inertial motion. We can take this to mean that Mach would now like to consider the hypothesis that the fixed stars are responsible for the *causal* determination of the motion of inertial bodies rather than merely being our means of gaining knowledge of inertial frames (determined by the hypothetical medium).¹⁵ To illustrate how this might work, Mach considers motion in a non-inertial frame of reference. As Newton observed, it is necessary to introduce inertial forces (centrifugal and Coriolis) in this frame if we want to recover an empirically adequate description of the dynamics. But *where* do these

¹⁴It is worth noting that Mach’s contemplation of this *medium* solution lends credibility to accounts by Weyl (1922, p.218-219) and Brown (2005, p.159-160) according to which general relativity may be regarded as Machian since the existence of this “medium”, i.e. *space-time*, is indeed demonstrably by other means (through phenomena of gravitation).

¹⁵We may also note that Mach’s use of the terms “fundamental” and “collateral” here is due to his deliberate avoidance of using more metaphysically loaded language related to the concept of causation.

inertial forces come from? For Newton, these forces are *fictitious*, they are not real, but rather they are simply an artefact of our choice of reference frame that is rotating in absolute space. According to the hypothesis of a physical medium, or likewise Neumann's hypothesis of the 'Body Alpha', if this body/medium is assumed to be causally responsible for determining inertial motion, we may suppose that these inertial forces are produced by a relative rotation with respect to the body or medium. But what if we want to describe inertial forces in the absence of this physical medium, the 'Body Alpha,' or of absolute space? In this case, Mach proposes that the inertial forces might be (Mach, 1883, p.232):

produced by [a] relative motion with respect to the mass of the earth and the other celestial bodies.¹⁶

Similarly, if we were to choose a frame of reference in which no inertial forces are required, it would likewise be the same "celestial bodies" (which would in this case be approximately stationary) that are responsible for causing the rectilinear and uniform motion of a given body that would be observed. In both cases however, what is important is that inertial motion is wholly determined by a body's relations to other physical bodies, and these relations are independent of whether we choose our frame to rotate.

Hypothesis 2 *The Mach Hypothesis:* *The motion of bodies observed as rectilinear and uniform with reference to the fixed stars, referred to in classical mechanics as 'inert', might be caused directly by the action of these distant stars upon those bodies.*

In paragraph 7 of this same section, Mach goes on to consider possible ways of formulating such a relational replacement of Newton's law of inertia:

Instead of saying, the direction and velocity of a mass μ in space remain constant, we may also employ the expression, the mean acceleration of the mass μ with respect to the masses $m, m', m'' \dots$ at the distances $r, r', r'' \dots$ is $= 0$, or $d^2(\sum mr / \sum m) / dt^2 = 0$. The latter expression is equivalent to the former, as soon as we take into consideration a sufficient number of sufficiently distant and sufficiently large masses.

Accordingly, Newton's law of inertia would not be true absolutely, but only true approximately provided that the universe is populated by a sufficiently large number of masses. Unlike Lange and Neumann, Mach is genuinely raising the possibility of an alternative physical law. Mach emphasises however, that this alternative formulation should not be taken as a

¹⁶Here the verb "produced" is a translation of "geweckt werden", which can also be translated as "awoken".

definitive replacement of the law of inertia, rather, it is simply intended as a demonstration that it is possible to construct a (so far) empirically adequate relational account of inertia as the action of the distant stars. However, with his empiricist humility, Mach leaves the task of developing a more precise replacement for the law of inertia to future investigators:

It is impossible to say whether the new expression would still represent the true condition of things if the stars were to perform rapid movements among one another. The general experience cannot be constructed from the particular case given us. We must, on the contrary, wait until such an experience presents itself. Perhaps when our physico-astronomical knowledge has been extended, it will be offered somewhere in celestial space, where more violent and complicated motions take place than in our environment.

4.2.3 Norton's interpretation

It is interesting to remark here, that while we have taken the last two passages quoted as clear evidence for the idea that Mach is exploring the possibility of a new physical law (in accordance with hypothesis 2), [Norton \(1995\)](#) cites the very same passages as purported evidence that Mach is proposing a mere “redescription” of Newton’s law. Although from the perspective developed so far, this view seems rather strange, it can be made to seem somewhat more credible if we follow Norton’s reasoning closely. From Norton’s perspective, the proposal of an alternative expression $d^2(\sum mr/\sum m)/dt^2 = 0$ is merely an attempt at an imperfect redescription of the Newtonian inertial law ([Norton, 1995](#)):

The project is clearly just one of redescription of existing laws and not the proposal of a new mechanism. Indeed Mach soon makes it very clear that his new expression for the principle of inertia is not intended to be applied to cases remote from experience.

Norton goes on to cite the second passage quoted above: “It is impossible to say whether the new expression would still represent the true condition of things if the stars were to perform rapid movements among one another [...]”. To Norton, this latter qualification is an admission by Mach that his relational law is imperfect since it would fail to adequately approximate the true law of inertia—that of Newton—in a more violent astronomical environment. In other words, Norton understands the term “true conditions of things” as a reference to Newton’s first law. However, this reading is, in fact, inconsistent with what is said; since it is already clear that the relational expression contradicts Newton’s law, and no empirical scrutiny is

needed to prove that. Empirical results will instead be able to tell us *which* expression, be it Mach's proposal, Newton's law, or some other as yet unknown expression, is correct.

Norton is not alone, however, in his interpretation of Mach. Indeed there is a long tradition of interpreting Mach's comments as proposing a mere redescription of Newton's law in terms of observable relations which stretches back to Paul Carus. The reason for this ambiguity in the interpretation of Mach's writings is of course Mach's reluctance to use what he saw as metaphysically loaded, causal language. This reluctance is less pronounced in his earliest writings (Mach, 1872), in which we see the germs of his sceptical attitude towards the common notion of cause and effect articulated more clearly.

4.2.4 Mach's critics

4.2.4.1 Early Mach's excesses

In his *Notes from Conservation of Energy*, Mach expresses his pleasure to have discovered that Neumann shared his concern regarding the definition of the law of inertia:

Although I was sorry to have lost the priority in this important matter, yet the exact coincidence of my views with those of so distinguished a mathematician gave me great pleasure and richly compensated me for the disdain and surprise which almost all the physicists with whom I discussed this subject showed. (Mach, 1872, p.76)

Over the course of the 20th century, Mach's views gained a lot more popularity, especially due to Einstein, who coined the term 'Mach's principle' and marketed this idea as a fundamental insight that contributed towards the development of his theory of general relativity. Mach's principle, and Mach's philosophy in general became associated with logical positivists such as Reichenbach and Schlick, who played a leading role in the philosophical interpretation of Einstein's relativity theories. In the latter half of the 20th century however, the influential works of a more realist school of philosophers of physics, mostly coming out of Chicago (Bunge, 1966; Stein, 1977; Earman, 1989; Friedman, 1983) painted Mach's contribution in a much less favorable light. Stein in particular launches an aggressive rhetorical attack on Mach, characterising his style of thinking as a form of "abusive empiricism" and going on to dismiss each of his arguments and claims.¹⁷ Stein's attack is reminiscent of the "disdain and surprise" that Mach recounts as the typical response to his ideas from his contemporaries.

¹⁷Given Stein's own abusive treatment of Mach one may wonder whether his choice of the term be due to some kind of psychological projection.

Although Stein is excessively dismissive, there is a sense in which we can agree with him that, especially in his early writings, Mach's empiricism overlooks the significance of Newton's methods, failing to appreciate what is of value in the postulation of absolute space and time.

After quoting Newton's views on space, time and inertia in *Die Mechanik*, Mach immediately jumps to accusing Newton of acting "contrary to his expressed intention only to investigate *actual facts*" on the basis that absolute space is not observable (Mach, 1883, p.229). But this ignores the methodological significance of the concept of absolute space, which had been recognised by other commentators prior to Mach.¹⁸

4.2.4.2 Late Mach's doubts

In later editions of *Die Mechanik*, Mach seems to soften his criticism of Newton. This appears to be stimulated in particular by his reading of Lange (1885), who's investigation Mach greatly admires for its "methodical movement" which "wins at once the reader's sympathy." It is somewhat peculiar that Mach appreciates Lange's piece so much, given that, as we have seen Lange does not even attempt to solve the *causal* question concerning the determination of inertial motion. For this reason, Mach's admiration of Lange's works raises doubts as to whether Mach even understood his own arguments. Furthermore, in these later editions Mach seems to revisit Newton's ideas from a refreshed perspective, citing Newton's Corollary V (the Galilean principle of relativity) as a formulation of the inertial law which is consistent with Mach's views.

In order to have a generally valid system of reference, Newton ventured Corollary V of the Principia. He thought of a [...] coordinate system for which the law of inertia holds, fixed in space without rotation relative to the fixed stars. He could also allow an arbitrary origin and uniform translation of this system [...] without losing its usefulness. Newton's laws of force would not be thereby altered; only the initial position and velocity, and the constants of integration could vary. By this formulation, Newton specified *precisely* the meaning of his *hypothetical* extension of the Galilean law of inertia. One can see that the reduction to absolute space was in no way necessary, since the reference system is no less relatively determined as in any other case. (Mach, 1933, p.227)

Stein interprets this difference to Mach's other views as an indication of Mach's confusion and inconsistency: "This point of view is precisely the appropriate one for Newtonian dynamics; and it rests, as Mach entirely fails to notice, not indeed upon absolute space, but

¹⁸See for instance Laplace (1796, chp.1) or Kant (1786a, p.16).

nonetheless upon 'absolute uniform motion' as a *vera causa*—not explicated through phenomena of relative motion" (Stein, 1977).

Stein's motives for giving this highly rhetorical account should be questioned. As Banks (2014) and Wolters (2011b) have remarked, the "second wave of Mach scholarship"¹⁹ to which Stein belongs, was "instrumentalized for a battle against the institutionalized positivism" (Banks, 2014, p.9). Other authors have suggested that the apparent inconsistencies in Mach's views indicate an evolution in his thinking (DiSalle, 1990, 2002a,b; Thébault, 2021). It is certainly plausible that Mach's views evolved due to the inherent tension among his ideas. One such tension, which Norton identifies, is between the requirement for economy and the need to reduce theoretical entities to experience (Barbour and Pfister, 1995, p.56):

There was a tension [in Mach's writings] between the need for the descriptions to be restricted to observation and for them to be economical. [...] the price of the economy is talk of entities that transcend observation. So it is with spacetime structures; they are unobserved, but, [...] they do enable just the systematization we want.

Another tension in Mach's views is that between his strict empiricism and the aprioristic implications of his principle. Moritz Schlick in particular was keen to point this out (Schlick, 1915). For Schlick, Mach's assertion that two kinematically equivalent situations ought not to differ dynamically is insufficiently empiricist since it represents an *a priori* restriction on our possible theories. In Newton's theory, for instance, two kinematically identical situations may differ dynamically, but this is not a problem, because the difference is accessible to sense-experience (Schlick, 1915, p.168):

We can also ascertain the absolute rotation of a body, according to the Newtonian view, through muscular sensation, for we will find with its help that centripetal forces are needed for the body to keep its shape and to hold together its parts.

Although this Machian requirement does not in fact restrict the possible empirical content of theories, but only informs their formal aspect, it is plausible that, as an avowed empiricist, Mach felt unsettled by his own ideas. After all, the history of 20th century physics has produced many examples, whatever one may think of their plausibility, of broad and far-reaching aprioristic speculations based off of Mach's hypothesis.²⁰ The view that Mach was

¹⁹This term originates in Blackmore (1972).

²⁰These include: Hofmann (1995); Reissner (1995); Schrödinger (1925); Sciama (1953a,b); Brans and Dicke (1961); Barbour and Bertotti (1982); Assis (1989) to name just a few examples other than Einstein's project of course.

disturbed by his own ideas is supported by the account of Hugo Dingler who alleges that Mach's explanation of centrifugal forces in terms of a relation to the fixed stars "contradicted his sensibilities" (Dingler, 1921, p.157). Dingler also cites Mach's son, Ludwig Mach, who claims that his father was tormented by the consequences of his hypothesis (Dingler, 1921).²¹

It is quite plausible that these tensions in Mach's thought may explain why he repeatedly feels the need to fall back on cautious epistemological remarks in his treatment of the inertial law, and refrains from further developing his hypothetical speculations. After all, unlike his 20th century acolytes, Mach did not embark very far upon the project of constructing a new hypothetical physical model that would embody his ideas. Towards the end of his life, he seemed determined to take up the defensible standpoint of a humble empiricist.

4.3 DiSalle and Einstein

While Stein saw the contradiction between Mach's ideas as evidence of his confusion, DiSalle on the other hand presents a more sympathetic interpretation which takes these passages as evidence of an evolution in Mach's thinking: "in accepting the abstract formulation of the laws of motion, Mach revealed that he had come to understand something about the foundations of Newtonian mechanics that neither he, nor very many others, had understood before. And this improved understanding clearly arose from his study of the literature on inertial systems" (DiSalle, 2002a, p.175). DiSalle goes on to argue that Mach had learned to distinguish between (1) the "external question" about whether the law of inertia could be replaced with an alternative formulation, and (2) the "internal question" of how we must go about constructing inertial reference frames. These two questions essentially correspond to the distinction mentioned earlier between (1) the *causal* question of what physical phenomena may be responsible for inertial motion, and (2) the *epistemological* question of how we may derive knowledge of an inertial system. As we saw, these two questions were conflated in Neumann's approach, whereas Lange did not conflate them but only attempted to answer the first question. The fact that Mach shows his great admiration for the work of Ludwig Lange demonstrates that he was not merely concerned with the causal question, but that both of these questions troubled him. It is highly likely that in his early work, Mach did not

²¹Dingler's account should be taken with a certain degree of skepticism since, as Norton (1995) remarks, "by 1921, Dingler had become an outspoken critic of relativity theory and, as a disciple of Mach, may well have been overeager to seek reasons to remove Mach's support from relativity theory" (Norton, 1995). Moreover, Gereon Wolters has raised serious doubts concerning the reliability of Ludwig Mach's accounts of his father (Wolters, 2019, 2011a, 1987), so this too should be viewed with skepticism.

distinguish precisely between the causal and epistemological problems, since after all he did not notice how these two problems were conflated in Neumann's work. Moreover, Mach's unwillingness to use explicitly causal language certainly did not help to shed light on the distinction.

4.3.1 DiSalle's criticism of Einstein

In defending Mach however, DiSalle deflects the criticism that Stein and others had directed at Mach towards Mach's 20th century acolytes, notably Einstein. DiSalle claims that Einstein and Reichenbach's ideas "frequently involve confusion about the nature of the principle of inertia." Einstein argued that inertial frames in Newton's theory function as a "factitious cause" of inertial effects in Newtonian mechanics and special relativity; he attributed this idea to Ernst Mach and illustrated it using a thought experiment in [Einstein et al. \(1952, p.112-113\)](#). This thought experiment can be paraphrased as follows: we consider two bodies fixed in space, S_1 and S_2 , which are in relative rotation with respect to one another around the line which connects each to the other. S_1 is found to be perfectly spherical while S_2 bulges at the equator. Here Einstein asks, what is the reason for the difference between the two? According to Newtonian Mechanics, Einstein claims: "The laws of mechanics apply to the space R_1 , in respect to which the body S_1 is at rest, but not to the space R_2 , in respect to which the body S_2 is at rest. But the privileged space R_1 of Galileo, thus introduced, is a merely factitious cause, and not a thing that can be observed." Einstein then offers the Machian explanation:

The only satisfactory answer must be that the physical system consisting of S_1 and S_2 reveals within itself no imaginable cause to which the differing behaviour of S_1 and S_2 can be referred. The cause must therefore lie outside this system. We have to take it that the general laws of motion, which in particular determine the shapes of S_1 and S_2 , must be such that the mechanical behaviour of S_1 and S_2 is partly conditioned, in quite essential respects, by distant masses which we have not included in the system under consideration.

In his essay, DiSalle claims not only that Einstein commits a "philosophical mistake" in his reasoning here, but also that these ideas are in fact in contradiction with Mach's own thoughts. We will examine both these claims in what follows.

DiSalle's rebuttal of Einstein is founded on what he sees as a comparatively subtle understanding of inertial frames which he explicates as follows: "an inertial system is not itself a cause, but constitutes the framework within which causal efficacy is measured, through

the accelerations that causal agents produce in one another.” Furthermore, DiSalle argues that this is the view that Mach came to understand in his later writings based on his discussions of Newton’s fifth corollary. According to DiSalle, Mach was not granting causal status to the fixed stars, rather he was just claiming that “in actual practice, the stars constitute the *empirical* framework within which causal influences, at least among the celestial bodies, are measured.” Concerning Mach’s speculative hypothesis about inertia coming from some action of the fixed stars, [DiSalle \(2002a\)](#) writes:

Of course Mach’s speculation about the origin of inertia suggests that the stars may be playing a causal role as well, but Mach clearly understood this as a separate issue—a question about a possible alternative theory, rather than the identification of an internal “epistemological defect” of the theory of inertial systems.

Einstein, on the other hand, had confused these two issues, claiming that the Machian hypothesis about the fixed stars implied an “epistemological defect” in classical mechanics.

4.3.2 Defence of Einstein

Now, while we can grant to DiSalle that perhaps Einstein did not express himself entirely accurately, it would be a stretch to claim that his ideas here are not reflective of Mach’s own writings. What we will argue is that Einstein’s argument takes as its premise one of Mach’s hypotheses, but that given this premise, Einstein is correct in viewing the question of inertial reference frames as an epistemological defect in classical mechanics. To clarify the situation, we will need to return to our discussion of Mach’s views concerning causality.

At the heart of Mach’s critique of Newton, as we have argued in earlier parts of this chapter, is an attempt to overturn the prevailing view of causation according to which singular causes, represented by forces, are responsible for singular changes in motion. This view is epitomised by the definitions of *force* provided by Poisson and Maxwell cited previously. Accordingly, the classical law of inertia is a necessity since it defines the standard for how motion proceeds in the absence of external causes effecting it. Now it is important to note that DiSalle’s defence of Newtonian inertial systems, on the basis of which he claims that Einstein commits a “philosophical mistake”, is predicated upon this very conception of causality ([DiSalle, 2002a](#), p.181):

an inertial system is not itself a cause, but constitutes the framework within which causal efficacy is measured, through the accelerations that causal agents produce in one another.

But we have seen that, at least in his early writings, Mach contradicts this view. Mach's primary innovation, we have argued, is in fact to raise the possibility that inertial motion is not *uncaused* motion, but rather could be a motion causally determined by the regularity of the cosmic environment constituted of the distant stars. According to *this* view, which is one of the two hypotheses which Mach proposes as alternatives to that of Newton, inertial frames would only need to make an appearance in our physics when analysing subsystems of the universe. This is because, when the universe as a whole is taken into consideration, it would become totally arbitrary whether we use a coordinate system in which the bulk of matter is stationary or rotating around a common point. Our inertial system, which would be necessary to consider in a subsystem of the universe, would thus be acting as an abbreviated reference to the rest of the matter in the universe; and it would indeed, in this case, be *causally* responsible for determining inertial motion in that subsystem. Einstein's argument therefore is perfectly Machian. Furthermore, it is not based on a misunderstanding of inertial systems, rather, Einstein is merely taking for granted Mach's second hypothesis (hypothesis 2) and the expanded Machian view of causality implied in it. According to this view, inertial systems *do* appear as the "factitious cause" of inertial effects, therefore there *is* an "epistemological defect" in the theory if we cannot identify some observable structure as causally responsible for these effects.

But why then, we might ask, did Mach seem to go against this view in his later years? It is not that Mach abandoned the views of his younger years, but rather, as DiSalle recognises, that he came to appreciate Lange's solution to the epistemological problem concerning the definition of Newton's first law, and even recognised the possibility of interpreting Newton's fifth corollary in a similar way. Mach had found, in Lange's solution, the true empirical content of Newton's law, which he could now recognise as a genuine "*hypothesis*". Stripped of its metaphysical reference to absolute space, Newton's law now became a hypothesis among others that could be subjected to empirical scrutiny by future experiments. The possibility of alternative hypotheses, involving different conceptions of the causal relations of things, such as the hypotheses Mach explored in his early writings, is not ruled out by Mach.

4.3.3 Einstein's further hypotheses

In addition to what we might call the *classical inertial hypothesis*: that in the absence of identifiable forces, all bodies move rectilinearly and uniformly with respect to one another, two other *implicit* hypotheses accompany classical mechanics. In his formerly quoted essay, Einstein identifies these implicit hypotheses and expresses them as follows ([Einstein et al., 1952](#), p.112):

- (1) “To two selected material points of a stationary rigid body there always corresponds a distance of quite definite length, which is independent of the locality and orientation of the body, and is also independent of the time.”
- (2) “To two selected positions of the hands of a clock at rest relatively to the privileged system of reference there always corresponds an interval of time of a definite length, which is independent of place and time.”

The first is connected to the Euclidean nature of absolute space, while the second depends on the homogeneity and universality of absolute time. Newton’s mechanics can be interpreted in two ways: either we could consider that these laws are the laws of geometry, they belong to the space-time structure of Newtonian theory and need no causal explanation. Or else, we might postulate that some ‘*medium*’ or ‘*metrical field*’ is causally responsible for determining the size of rigid bodies and the relative speed of different clocks. The first interpretation corresponds to the view DiSalle defends, while the second opens itself to Machian criticism.²² Einstein asserts that in his theory of general relativity, Newton’s hypotheses are overturned; indeed in Einstein’s theory these laws are deemed to be only approximate and contingent truths that will hold if the matter distribution in the cosmos is entirely homogeneous and certain further conditions about cosmic structure are satisfied. In other words, Newton’s hypothesis was wrong, and since it was wrong, is it not reasonable to consider whether the error may stem from the classical conception of causality which Newton and others in his time assumed? It is particularly revealing that while DiSalle characterises Einstein’s Machian reasoning as a “philosophical mistake”, he admits that “it has to be considered a fortunate one for the history of physics.” For *what world do we live in after all?* A Newtonian world, in which there is no reason to question whether our inertial reference frames might be conditioned by the action of other matter? Or do we live in a world in which such conditioning does take place? If Einstein’s Machian intuitions were, at least to some extent, correct, does it matter that his reasoning contradicts the mathematical formalism of inertial systems?

Closing comments

At the core of Mach’s principle is a challenge to the classical conception of inertia as uncaused motion. In the standard approach to Newtonian mechanics, a force acts as a cause, producing

²²Concerning the relational definition of a preferred temporal metric, [Mittelstaedt \(1980\)](#); [Barbour \(1981\)](#) have explored this idea and called it the ‘second Mach’s principle’ (Although Mach himself did not explicitly tackle this issue). Barbour and Mittelstaedt do not consider similar arguments for the relational definition of scale, although this has more recently become an important part of Barbour’s work ([Barbour, 2010, 2023](#)).

a change in motion. By necessity, therefore, a law of inertia is conceived which defines motion in the absence of causes. Mach insists however, that all motion is equally caused by the surrounding circumstances. We are not justified in our treatment of some effect to single out some particular circumstance as the cause but must take heed of the influence of the entire universe (causal holism). We must interpret the homogeneity of inertial motion as an effect of some homogeneous aspect of the environment with respect to which this motion is performed. Although this revision of the view of causality that we here claim was operative in Mach's critique of inertia was not precisely articulated in Mach's treatment of the issue, in part due to Mach's deliberate resistance to using causal language, we saw that Mach does explicitly discuss this revision of causality in his early work, at a time which coincides with his original speculations on the origin of inertia.

Second, we distinguished Mach's principle, which is a purely *formal* affirmation of relationalism, from Mach's hypotheses, of which we identified two classes. The second class of hypotheses is the one proper to Mach, which we may thereby call the *Mach hypothesis* (hypothesis 2). According to this hypothesis, Mach proposes that the distant masses of the universe may be responsible for the causal determination of so called "inertial" motion. This intriguing thought has been the subject of much speculation ever since. In his early years, Einstein took it quite seriously, and used it as a guiding principle during the development of his theory of general relativity. While DiSalle questions the motivation behind Einstein's reasoning, defending the mathematical formalism of reference frames as an adequate account of the phenomenon of inertia, and argues that Mach came to acknowledge this view, we saw that this conception contradicts Mach's original intention to regard inertia as a form of *caused* motion.

Going forwards, there is no doubt that the insights drawn from this study will help to clarify the basis and motivation underlying speculative Machian theories. Moreover, the distinction drawn between Mach's principle and the Mach hypotheses may shed some light on the controversial question of the Machian status of general relativity.

In Part II we move from these considerations of scientific methodology to discuss actual scientific theories, and what they tell us about the natural world. Distinguishing formal artifacts from empirical content remains a major concern in this second half of the thesis, and the analysis we give of various theories is motivated by the results of Part I

Part II

NATURE



POKING BEARS

Sievers, no more ether!

Professor von Franz, *Nosferatu*, 2024

Why are the laws Lorentz-covariant?

Introduction

In the previous chapter, we ended with a defence of Einstein's Machian programme for the relativisation of inertia. We defended Einstein based on his thought experiment with two rotating spheres, which was initially published in [Einstein \(1916\)](#), shortly after the publication of his general theory of relativity. As is well known, however, contrary to his initial belief, Einstein's 1915 theory did not fully implement Mach's idea, and Einstein subsequently faced many difficulties in his attempts to develop a fully Machian theory of gravity. Whereas [Hoefer \(1994\)](#) especially has discussed the history of Einstein's struggle in detail in the context of general relativity (GR)—which we will discuss in chapter 6—little has been said concerning the status of “Mach's principle” in special relativity (SR). This question, while worthwhile to investigate for its own sake will also help us reveal what were the problems with Einstein's approach to Mach's principle more generally. As we shall see, Einstein's methods and approach to SR in 1905, although perhaps superficially aligned with Mach's philosophy, was already implicitly in contradiction with Mach's principle, and this essential error was never corrected in Einstein's work. Nonetheless there are several reasons why people have sometimes claimed that SR was at least aligned with the aims of a generally Machian programme.

First of all, Mach's philosophy did influence Einstein in his rejection of the existence of Lorentz's ether, as [Darrigol \(2021a, p.202\)](#) has noted. While Poincaré adopted the relativity principle without entirely rejecting the ether's existence, Einstein associated the adoption of this principle with the ether's rejection. Einstein's view here reflected an epistemological trend—popularised in part by Mach—to reject the existence of theoretical entities lacking empirical counterparts. Since the ether frame is not observable directly, Einstein was drawn towards the intuition underlying Mach's *second* hypothesis, that only so called “ponderable bodies” which are directly observable are physically real.

Einstein's rejection of the ether was also related to Mach's principle more directly. His view is expressed quite well by [Lorentz \(1927, p.219\)](#) who retrospectively contrasts it to his own perspective and that of most other physicists at the time:

Physicists of former days would say that [inertia has its] seat in the ether, - that the earth rotates relative to this medium and that the g_{ab} 's have their normal values in a system of axes that is at rest in the ether. [...] On the other hand, according to Einstein there is no such thing as the ether of which we have just spoken. [...] Einstein thinks that [inertia is] fixed in some way or other relative to the fixed stars, seen or perhaps unseen.

In this passage we see that Lorentz is relating Einstein's rejection of the ether with what we have called Mach's *second* hypothesis, whereas Lorentz himself endorsed Mach's other (medium) hypothesis about the cause of inertial motion. But while this may indeed reflect Einstein's intentions, Mach's second hypothesis was not successfully implemented in Einstein's theories. Although it can be debated whether general relativity can be rendered consistent with Mach's second hypothesis—we discuss this in chapter 6—in the context of SR, inertia is defined with respect to the Minkowski spacetime structure which allows one to define absolute accelerations, there is no indication that masses will be responsible for inertial forces, and yet the existence of the ether is rejected.¹

Quite apart from Einstein's own thoughts on the matter, in a paper from 1978, Jon Dorling has argued that SR in fact adequately implements the relativity of acceleration and that general relativity was therefore superfluous to this purpose ([Dorling, 1978](#)). For Dorling, the issue with absolute accelerations in classical mechanics is a violation of the principle of sufficient reason, the spotting of which he attributes to Einstein. Referring to Einstein's thought experiment which we discussed in the last chapter, Dorling explains that: a body is observed

¹Later in this chapter we will come back to this point and raise further issues with this motivation for Einstein's rejection of the ether.

to take the form of an ellipsoid rather than a sphere, presumably due to centrifugal forces. The problem, however, is that the explanation of this phenomenon—i.e. rotation with respect to absolute space—is itself a phenomenon which is only known due to the deformation of the sphere. This problem would disappear, according to Dorling, “provided we had a way of distinguishing between uniform and accelerated motion which was independent of the dynamical effects of the latter.” In other words, in classical mechanics there is a dynamical difference between two situations without a kinematical counterpart. According to Dorling then, the problem disappears in SR because a rotating or otherwise accelerating body is no longer kinematically identical to one which is non-accelerating. SR therefore successfully relativises acceleration and general relativity or the “neo-Machian programme” are superfluous to this goal (Dorling, 1978).

Clearly, according to our discussion in the previous chapter, Dorling’s solution is not sufficient. What was essential about Mach’s principle was that there is some *real* physical thing with respect to which a body rotates that is *causally* responsible for the dynamical difference. The so-called “kinematical difference” can only be an indication of this entity. As we saw, Mach proposed two hypotheses: that this entity is a medium, or the sum of masses of the universe, either of which would provide a substantial physical difference that may be causally responsible for inertial forces. It would not make any sense to say, however, that the inertial forces are caused by a difference in the measurements of clocks along trajectories. The latter although classed in SR as “kinematical”, are—like the dynamical differences—effects of the sphere’s rotation.² Moreover, as we have already seen and will discuss further shortly, the real impetus behind Mach’s principle is in effect a rejection of the notion of kinematical or default behaviours. It is precisely because SR gives a kinematical explanation of effects such as time dilation that it is in tension with Mach’s principle. Mach’s principle, in the context of SR, we will argue, consists precisely in the rejection of a kinematical explanation, and the insistence that these effects should be dynamical and therefore have a *cause*. In analogy with the problem of the source of inertial forces, this requirement then leaves the possibility open for two classes of hypotheses concerning the cause of Lorentzian effects: motion with respect to a medium, or motion with respect to distant bodies.

We divide this chapter into three sections. In section 5.1 we discuss the history and development of the principle of relativity as it appears in SR. Much of the present confusion

²In this respect Dorling’s solution is similar to the fallacious solutions to the problem of rotation that were advanced by Huygens, Berkeley and Kant (as we saw in chapter 3). In those cases, we rejected the solutions since they were founded on a conflation between velocity and acceleration and therefore mistakenly interpreted acceleration as kinematical. In the case of Dorling, he is right that in special relativity there is a kinematical distinction between a stationary and a rotating body, however we take this to be a problem with the causal structure of special relativity rather than a resolution of the problem of rotation.

concerning the interpretation of this theory is rooted, we claim, in an incorrect interpretation of this principle. We will be applying an analysis of this relativity principle analogous to that which was given to the Galilean relativity principle of classical mechanics in chapter 1. In section 5.2, we draw from our analysis of Mach’s principle in chapter 4 to argue that Einstein’s kinematical formulation of SR, at least as it is usually interpreted, is directly philosophically opposed to Mach’s principle. The key features of Mach’s principle which we draw on here were explored and discussed in the previous chapter and are as follows: (1) Mach’s revision of the classical conception of causality and his corresponding rejection of effects deemed “kinematical”, (2) Mach’s attention to the causal influence of the surrounding environment usually deemed irrelevant in scientific modelling. From these considerations, we argue that, like classical mechanics, special relativity is based on certain idealising assumptions that limit the scope of its applicability, making it inappropriate for this theory to be applied to the cosmos as a whole. This is essentially the point that Wallace (2022a,b) makes when he criticises the “cosmological assumption” often made in the interpretation of theories, although Wallace did not consider applying this critique to the case of special relativity in his papers.

In section 5.3, motivated by the need to reject Einstein’s relativistic kinematics on the basis of Mach’s principle, we ask whether the *dynamical* approach may have other things to offer. In particular, speculate on whether an alternative interpretation of special relativity may provide an *explanation* of Einstein’s postulates and of the Lorentz-invariance of dynamical laws. In this context we will engage dialectically with works of Michel Janssen, and draw from lines of reasoning that have been presented in certain physics papers such as Barceló and Jannes (2008) and Schmid and Kroupa (2014).

It should be noted at the outset that I do not attempt to provide a comprehensive exposition of the kinematical approach to special relativity, nor a detailed discussion of the motivations that have been advanced in its favour. Such a task would take us beyond the scope of the present chapter, whose purpose is instead to develop a particular criticism of this approach. Detailed defences of the kinematical interpretation of special relativity can be found in the work of Michel Janssen and collaborators, especially (Janssen, 2009, 1995; Balashov and Janssen, 2003).

5.1 The “relativity principle”

As has sometimes been remarked, the name given to the theory of “special relativity” (SR) of Einstein is somewhat misleading, since, after all, relativity under boosts was already opera-

tive in the much older classical mechanics in the form of the principle of Galilean relativity. While the naming of this theory was most likely due to Plank and was never adopted entirely comfortably by Einstein (Pais, 1982; Stachel, 2016), the emphasis on the relativity principle represents the key difference between SR and most other competing theories of the electrodynamics of moving bodies of the time, rather than a difference with classical mechanics. The conception of light as an electromagnetic wave implied the existence of an all-pervasive medium through which light should propagate. This would then allow a common point of reference with respect to which the motion of any body could be defined, and this motion should, in principle be detectable empirically. However, the failure to detect the earth’s motion through the ether in a series of optical experiments conducted in the latter half of the nineteenth century, posed a challenge to theorists.

5.1.1 Origins of the principle

In 1892, Lorentz proposed that the null result of the Michelson–Morley experiment could be explained by assuming that bodies undergo a deformation in the direction of their motion through the ether, a hypothesis he developed independently of FitzGerald, who had suggested a similar idea in 1889 (FitzGerald, 1889; Lorentz, 1892). This postulated deformation of material bodies—which Lorentz did not restrict uniquely to longitudinal contraction—was intended to eliminate second-order optical effects proportional to v^2/c^2 , such as those expected in interferometry. Already in 1892 Lorentz had derived the Fresnel drag coefficient from his theory, and a few years later Lorentz (1895) provided a more general treatment of first-order effects by introducing the notion

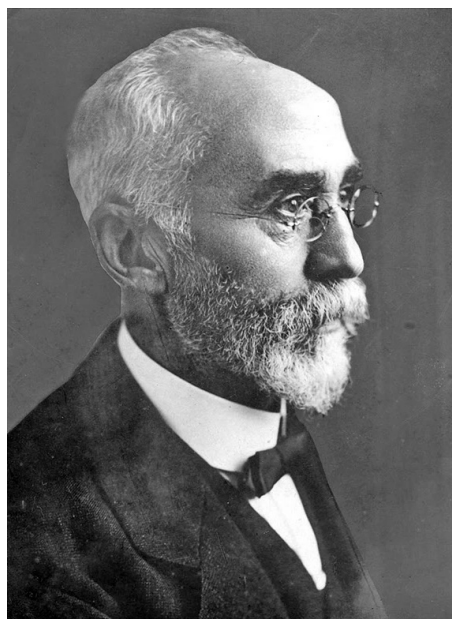


Figure 5.2: Hendrik Lorentz.

of “local time” and the theorem of corresponding states, according to which electromagnetic processes in a system moving uniformly through the ether can be obtained from those in a system at rest by a suitable transformation of coordinates and fields. This allowed him to account for stellar aberration, Fizeau’s experiment on the partial dragging of light in moving water, and the optical Doppler effect—phenomena sensitive to terms linear in v/c that had long been successes of Fresnel’s stationary-ether framework, in which the ether remains stationary in empty space but is partially dragged within refractive media. The difficulty was

specific to Lorentz's own version of the ether theory, which rejected any dragging of the ether by moving matter: in the absence of a drag mechanism, reproducing these first-order results required the non-trivial auxiliary hypothesis of local time, thereby extending the explanatory reach of his theory while significantly complicating its conceptual foundations.

In his 1900 Festschrift essay honouring Lorentz, Henri Poincaré proposed a more ambitious generalization: not only first- and second-order effects, but all higher-order terms in v/c should be undetectable in any optical or electromagnetic experiment. Poincaré's remarks in this essay—which ended up featuring as part of *Science and hypothesis* (chapters IX and X)—represent the first formulation of the *principle of relativity* in the context of electrodynamics (Poincaré, 1900, p.22-23):³

Would it not also be a coincidence — and a strange one — if one circumstance came along just in time to cancel the first-order terms, and another entirely different circumstance, equally well-timed, took care of canceling the second-order terms? No, we must find a single explanation for both; and then everything leads us to think that this explanation will also apply to higher-order terms, and that the mutual cancellation of these terms will be rigorous and complete.

The following year, Poincaré (1901) returns to the question of the detectability of motion through the ether in response to new experimental efforts by Victor Crémieux (a doctoral student of Gabriel Lippman at Sorbonne who's work Poincaré was closely involved in advising and supporting). In this paper, which also appears later in *Science and Hypothesis* (chapter XIII), Poincaré expresses concern that Crémieux's experiments could reveal the detectability of the ether frame and thus contradict the relativity principle as he had construed it. However, he then raises a potential defence of the relativity principle that a follower of Lorentz might make:⁴

No, reply the partisans of Lorentz; what one would measure in this way is not their absolute velocity, but their relative velocity with respect to the ether, so that the principle of relativity remains intact.

³Translation of: "Ne serait-ce pas aussi un hasard que ce singulier concours qui ferait qu'une certaine circonstance viendrait juste à point pour détruire les termes du premier ordre, et qu'une autre circonstance, tout à fait différente, mais tout aussi opportune, se chargerait de détruire ceux du second ordre? Non, il faut trouver une même explication pour les uns et pour les autres, et alors tout nous porte à penser que cette explication vaudra également pour les termes d'ordre supérieur, et que la destruction mutuelle de ces termes sera rigoureuse et absolue."

⁴Translation of: "Non, répondaient les partisans de Lorentz; ce que l'on mesurerait ainsi, ce n'est pas leur vitesse absolue, mais leur vitesse relative par rapport à l'éther, de sorte que le principe de relativité est sauf."

From Poincaré’s perspective however, this state of affairs is unsatisfactory, since he had already been drawn to speculate that the undetectability of the ether frame might constitute a general law, and there is no doubt that Poincaré (always drawn to symmetry and mathematical perfection) found the indication of this symmetry in nature to be appealing.

This mention of a potential Lorentzian defence of the relativity principle in the passage quoted above shows something quite important for our purposes in this section. The “principle of relativity” that is defended here is not merely a reference to the empirical undetectability of the ether’s rest-frame, in this case it refers to the relativity principle in the broader *a priori* sense which we have spoken of in some detail in chapter 1, and which—as we discussed briefly in chapter 2—Poincaré speaks of many times in *Science and Hypothesis*. This *a priori* relativity requirement is safeguarded regardless of the detectability of or lack thereof of the ether. This passage shows that at this time, for Poincaré as for others, the principle of relativity employed as means of explaining null results in ether drift experiments was essentially *conflated* with the more general analytic principle of relativity, and this is why, later on, Einstein would associate the relativity principle with the non-existence of the ether. It is important that we emphasise here that these two senses of “relativity” were conflated for Poincaré, since, as we will see, the reasons for each are in fact quite different; moreover, it is this conflation that is the source of the mistaken view that special relativity is somehow aligned with Mach’s philosophy.

Concerning the surprising experiments of Crémieux, it was later shown (in 1903) through a collaboration between Crémieux and the American experimentalist Harold Pender (a student of Henry Rowland), which was organised by Poincaré himself, that Crémieux’s initial results were due to some anomalous behaviours of the dielectrics he used (Indorato and Masotto, 1989). The new result thus restored Poincaré’s confidence in the generality of the original “relativity principle”, i.e. the local undetectability of the ether frame. Consequently, Poincaré made some modification to his text for the 1908 edition of *Science and Hypothesis*, omitting a couple of paragraphs at the end of the section that had formerly highlighted the importance of Crémieux’s future experiments, and in addition, adding the following remark at the end of the formerly quoted paragraph:⁵

Since then, Lorentz has, moreover, found a more subtle but much more satisfactory response.

Here, Poincaré is not referring to an explicit rebuttal to Crémieux and Lippmann by Lorentz, but rather to the theoretical developments introduced in Lorentz’s 1904 paper. In that work,

⁵Translation of: “Depuis, Lorentz a d’ailleurs trouvé une réponse plus subtile, mais beaucoup plus satisfaisante.”

Lorentz substantially revised his electrodynamics of moving bodies, extending the transformations he had previously developed and moving closer to a fully covariant formulation. He introduced a consistent treatment of “local time” dilation and further clarified the behaviour of Maxwell’s equations under motion through the ether, aligning his theory more fully with the principle of relativity as Poincaré had formulated it in 1900. In this paper, Lorentz also gave his own tentative formulation of a relativity principle ([Lorentz, 1904](#)):

It would be more satisfactory, if it were possible to show, by means of certain fundamental assumptions, and without neglecting terms of one order of magnitude or another, that many electromagnetic actions are entirely independent of the motion of the system.

Shortly after, in his famous 1905 memoir *Sur la dynamique de l’électron* ([Poincaré, 1905, 1906](#)), which is regarded by some as the first full expression of the theory of special relativity,⁶ Poincaré enthusiastically endorsed Lorentz’s adoption of his own principle, and developed the theory further. In this paper, he derived the exact form of the Lorentz transformations and showed that they form a *group* whose elements leave the Lorentz–Maxwell equations invariant ([Darrigol, 2021a](#), p.176).

5.1.2 Einstein’s kinematics

Much has been written about the difference between Poincaré’s and Einstein’s formulation of SR, for our purposes however we will emphasise only one aspect of Einstein’s contribution, that is, the shift from a consideration of *active* to *passive* transformations; it is also this shift however that we will be most critical of. It is essentially this shift that allows the Lorentz transformations in Einstein’s theory, unlike the former work of Lorentz and Poincaré, to be interpreted *kinematically* rather than dynamically.

In his 1905 paper *Zur Elektrodynamik bewegter Körper* ([Einstein et al., 1905b,a](#)), Einstein introduces the theory now known as “special relativity” by postulating two fundamental principles. First, the *principle of relativity*, adopted from Poincaré and Lorentz, which asserts that the laws governing physical processes are the same in all inertial frames, so that no experiment can determine absolute uniform motion. Second, the *light postulate*, which states that light propagates in a vacuum with a constant velocity c , independent of the motion of the emitting body or the observer. These two postulates together lead to the derivation of the Lorentz transformations from first principles rather than as mathematical adjustments to fit

⁶Many authors prefer to attribute the first full expression of SR to [Einstein et al. \(1905b\)](#).

experimental results. Now, this would be particularly impressive if there were fundamental reasons why we should accept Einstein’s two postulates (we will discuss some possibilities in section 5.3). In the absence of an explanation however, it is unclear whether or not Einstein’s *principle* approach is merely a *convenience*, and that the postulates should really be regarded as theorems of some more explanatory theory.

It has long been a complaint against this theory that these two postulates seem to contradict one-another.⁷ The theory implies that the speed of light should be the same relative to two observers who are moving in relation to one-another. Whereas in the Lorentz-Poincaré theory, even though the speed of light will be locally measured the same c by all observers, this is merely an epistemological constraint and there is still a *matter of fact* as to what the actual speed of light is with respect to a given observer, since light travels through a *medium* at c , and this medium will be in a definite state of motion with respect to that observer. In Einstein’s theory on the contrary, all frames are not only *epistemically* indistinguishable, but in an important sense they are also *ontologically* equivalent. This move is reminiscent of Leibniz’s famous *principle of identity of indiscernibles*, which is intimately connected with the *a priori* reasons for the relativity of motion in general as we saw in chapter 1, as well as with Mach’s principle. However, Einstein’s move here would only truly be on par with Leibniz’s principle if the indiscernibility were complete. As we will see in 5.2 the indiscernibility here is somewhat misleading as it is limited to *local* measurements; after all, the special principle of relativity does not preclude that the universe as a whole may have a centre of mass, or that some different reference frames would measure different dipole anisotropies in the *cosmic microwave background* (CMB). As long as the transformations do not involve the entire universe the indiscernibility will not be absolute.⁸

By raising the epistemic indistinguishability to an ontological equivalence, Einstein’s theory also implies a reinterpretation of Lorentz’s notion of a frame dependent “local time”. As he put it in 1907 (Einstein, 1907b):

Surprisingly, however, it turned out that a sufficiently sharpened conception of time was all that was needed to overcome the difficulty discussed. One had only to realize that an auxiliary quantity introduced by H. A. Lorentz and named by him “local time” could be defined as “time” in general.

⁷See for instance Pierre Duhem’s “Quelques réflexions sur la science Allemande” (Duhem, 1915), in which he accuses “German Science”, including the Einstein-Minkowski-Laue relativity and the geometry of Riemann, of sacrificing intuitive, common-sense nuance (“esprit de finesse”) for the sake of abstract logical deduction (“esprit de géométrie”). Duhem’s essay is widely regarded as having been tainted by the influence of anti-German wartime propaganda.

⁸Note that for this reason questions concerning the relevance of the conceptual framework of special relativity to cosmology have arisen previously as we will see.

In turn, this implies that notions of absolute time and simultaneity must be abandoned. This way of thinking becomes somewhat less paradoxical if we ask ourselves on *what* our prior (Galilean) intuitions were based, according to which there is a well-defined notion of simultaneity between spatially separated points. In particular, if an observer a sees a body x move at velocity v , and another observer b moves with respect to a at co-linear velocity u , our habituation with the Galilean velocity addition law tells us that according to observer b the velocity of the body should be $v' = v - u$. But if this law was taught to us by experience, it is also perfectly possible for experience to refute it. Special relativity instead tells us that the velocity that will be observed by b will be given by $v' = \frac{v-u}{1-vu/c^2}$, where c is the speed of light. This velocity addition law ensures that observed velocities will always be capped at the speed of light, and that this maximum speed will be the same for all observers. For small velocities $v \ll c$ the Lorentzian velocity addition law reduces approximately to the Galilean, which explains why, in every day life, we are accustomed to assume the Galilean addition law.

In the interpretation of Lorentz and Poincaré, there is always a fixed background defined by the ether's natural frame of reference. For this reason, *Lorentz boosts* must always be interpreted as *active* transformations of a given system with respect to the ether (and any other features of the system's environment). For Einstein on the other hand, the background is eliminated since the existence of a preferred ether frame is rejected, so it becomes possible to interpret Lorentz boosts as *passive* transformations. [Janssen \(2009\)](#) has also identified this as a key difference between the two approaches:

In Lorentz's theory, Lorentz invariance is a symmetry of all dynamical laws but not of the space-time structure posited by the theory. In special relativity it is both. Switching from an active to a passive reading of the Lorentz transformation, the relativist can thus look upon the boosted version of the situation at rest as a redescription of the same situation from the vantage point of an observer with respect to whom the system is moving. This makes it clear that the relevant features of the moving system do not call for a dynamical explanation in special relativity. They are kinematical in the sense that the moving system is just exhibiting default spatiotemporal behavior. In Lorentz's theory, they are kinematical only in the broad sense of being independent of the details of the dynamics.

This shift from active to passive transformations is similar to the analogous shift we saw in the context of classical mechanics in chapter 1, and for similar reasons, as we will see soon

in the next subsection, it is problematic.

5.2 Mach against kinematics

The key lesson we drew from our analysis of Mach’s principle in chapter 4 was the following: Mach rejected the traditional distinction in classical mechanics between (1) what we called “uncaused” or *default* motion in the absence of causes characterised by Newton’s law of inertia and defined with respect to the kinematical framework of absolute space and absolute time, and (2) the *caused* motion subject to the action of some material body which induces a deviation from inertial motion. For Mach, the empirically observed motion of a given body which we call “inertial” is actually dynamically caused by some (roughly) homogeneous part of the body’s environment, and the “inertial frames” in which this motion is seen as rectilinear and uniform are not a feature of the kinematical spacetime structure but are dynamically determined by this feature of the environment. In relation to this, we can also note that for Mach, the associated principle of Galilean relativity in classical mechanics—which is essentially equivalent to the law of inertia—must be due to the homogeneity of the environment of the system under consideration, and not due to the homogeneity of absolute space and time.

Applying this Machian critique now to the case of special relativity, we see that it is precisely the kinematical view, which was developed due to Einstein, that we have to call into question. It was found that rods and clocks boosted with respect to their environment measure lengths and lapses of time differently to those which are stationary, and according to the kinematical view this is a *default* behaviour due to the Lorentzian signature of space-time. The Machian critique consists therefore in insisting that no such “default” behaviour exists. Instead, rods and clock run differently due to their different state of motion with respect to their environment. By making these effects kinematical behaviours due to space-time structure, we obscure the need to find their material cause, in just the same way that Newton, by making inertial motion a kinematical effect of his absolute space and absolute time, obscured the need to find its material cause.

At this stage it is important to emphasize a crucial point. Had the Galilean transformations held for all velocity boosts, so that rods and clocks in uniform motion relative to their environment still measured the same lengths and time intervals, this consistency—according to the Machian perspective we are developing here—would *also* be an effect of their motion with respect to the environment. For Mach, the absence of a difference is just as much *caused* as the presence of a difference.

In recent decades Harvey Brown, often in collaboration with some of his colleagues in Oxford, has reignited the debated between the “kinematical” and “dynamical” approaches to special relativity.⁹ According to Brown, it is not satisfactory to explain special relativistic effects through a simple appeal to “space-time” structure, for what we mean by this can only be the chrono-metrical effects that bodies are subjected to when they move with respect to each other. It is not space-time that is the *reason* for Lorentz contractions, rather, it is the Lorentz-covariance of the dynamical laws which govern the physical constitution of rods and clocks. In my view, Brown is essentially correct on this point, and while we will get to Michel Janssen’s response in the next section, it is worth noting that despite taking up this somewhat bold and unorthodox position, Brown is keen to stress that he does not mean to endorse the actual existence of a preferred reference frame. For instance in the preface to *Physical Relativity*, Brown writes (Brown, 2005, p.vii):¹⁰

I feel, from dire experience, I must emphasize from the outset that this approach does *not* involve postulating the existence of a hidden preferred inertial frame! The approach is not a version of what is sometimes called in the literature the *neo-Lorentzian interpretation* of special relativity.

Unfortunately, we will not be able to join the ranks of Brown and others as partisans of the now quite reputable *dynamical* approach to special relativity, since it is exactly the rejection of the preferred frame of reference that we have found, for essentially Machian reasons, to be epistemologically problematic. But since we have already defended a multitude of controversial positions throughout this thesis it surely cannot harm us very much to take up another.

Let us then examine this question of Einstein’s elimination of the ether frame. As we have seen, the principle of special relativity, which is empirically grounded, embodies the result that the motion of a system with respect to the optical medium is not locally discernible. Regardless of Earth’s position in its orbit around the Sun, ether drift experiments consistently produced null results. However it is insufficient to infer from this the non-existence of the preferred frame, as Einstein, and apparently Brown and others, have done. But in order to infer its non-existence, we need good reason to believe that if it did exist, these experiments would allow us to detect it. As we will see in the next section, there is good reason in fact to suppose the opposite. Furthermore, if we permit ourselves to draw this inference, we could also argue that the rest of the universe’s matter—relative to which a boosted system likewise

⁹See especially Brown and Pooley (2001); Brown (2005); Brown and Pooley (2006); Brown and Read (2018).

¹⁰Emphasis in original.

changes its state of motion, and which also appear to have no physical influence on the system—does not have a rest frame. Yet while Einstein is often credited with having shown that the ether does not exist, no one has, to my knowledge, leveraged the results of the Michelson-Morley experiment to argue that the distant bodies of the universe do not exist; nor to make the weaker claim that they lack a preferred rest-frame. No one denies the reality of the CMB simply because its rest frame cannot be detected in these *local experiments*; we accept its existence on independent theoretical and experimental grounds. A similar line of reasoning appears in approaches that posit an underlying medium for electromagnetic phenomena, which appeal to the wave-like character of light and its constant propagation speed to infer that light is an excitation of some sort of medium. On this view, the fact that the speed of light is always measured to be the same regardless of the motion of the observer is a result that stands in need of explanation, rather than something to be elevated to the status of a *postulate* solely on account of its generality.

Just like Newton’s kinematical approach to classical mechanics, the kinematical approach to SR conflicts with Mach’s ideas on two counts: Firstly, as we saw, it attributes certain effects to the default behaviours of bodies in the absence of dynamical causes, whereas Mach insisted that all effects even if they have the appearance of being kinematical are in fact due to external causes. Secondly, as we have just seen, it involves the assumption that the distant bodies of the universe do not act on the given physical system. Yet once again, as Mach has taught us, just because the effect of the environment is constant does not mean that this environment has no effect.¹¹

As in the case of classical mechanics, these two issues are not particularly problematic as long as it is understood that special relativity is a theory adapted to certain *subsystems* of the universe, and that its symmetry represent a useful idealisation, but have nothing to do with the a priori symmetry of a *space* regarded as abstracted from all matter. In other words, special relativity is fine as a mathematical framework, so long as we do not make what [Waller \(2022a,b\)](#) calls the “cosmological assumption,” i.e. the unjustified assumption that one’s theory extends to a description of the entire cosmos (see discussion also in §1.3). Einstein’s kinematical interpretation of special relativity in this regard has not helped the physics community avoid this mistake.¹² The historical record shows that this mistaken “cosmological assumption” with regards to SR appears to be the default view among physicists. The cos-

¹¹This second issue is particularly relevant in the context of certain recent theories, for instance that of [Unnikrishnan \(2022\)](#) who proposes that Lorentzian effects are in fact precisely due to motion with respect to the centre of mass of the universe, and that these effects are induced by the universe’s gravitational potential.

¹²It should be noted however that, as we saw in §5.1 the essential mistake had already been made by Poincaré insofar as his defence of the relativity principle in the face of Crémieux’s experiments showed that he conflated this subsystem symmetry with the analytic requirement for the relativity of motion in general.

mologists Hermann Bondi and Thomas Gold encountered this dogma when they presented their Steady State theory in 1948 (Bondi and Gold, 1948). Since their theory depended upon the continuous creation of matter in space, it was necessary to specify the state of motion that newly created matter should be in. Noting that “It is evident that this velocity vector associated with creation cannot be relativistically invariant, nor can invariance be introduced by assuming a random distribution of velocities, since the only invariant distribution diverges,” Bondi and Gold were then forced to abandon Lorentz invariance for the cosmos as a whole. Bondi would reflect on this episode at the Tübingen conference on Mach’s principle of 1993 (Bondi, 1995):

Forty-five years ago, when we were propagating the steady-state theory, one objection often met was: “I go all along with you, but how does the newly created matter know with what momentum to move?” When we said that the universe was not Lorentz invariant, we encountered shocked disbelief, although this was quite a clear and obvious consequence of the isotropy of the red shift in those days.

5.3 Why Lorentz-invariance?

While in the previous section we raised some issues concerning the kinematical interpretation of special relativity, especially in light of Mach’s critique of the classical distinction between *caused* and *default* behaviours, in this section we begin by revisiting some advantages of the kinematical view articulated in the work of Michel Janssen.

The main objection to Lorentz’s theory, according to the Janssen (2009) is that “it seeks to provide dynamical explanations for a class of phenomena, namely all manifestations of Lorentz invariance, that special relativity revealed to be kinematical.” By “kinematical,” Janssen clarifies, he means that “is that it is just an instance of some generic feature of the world, in this case instances of default spatio-temporal behavior.” Our main objection to Janssen here is, as we have seen, that the classification of these effects as “kinematical” precludes the possibility that they may have a material cause. The *reason* for the effects, in contrast to their *cause*, according to the kinematical interpretation, is the Minkowski signature of our space-time. Interestingly however, Janssen is not “substantivalist” about space-time, nor a formalism fetishist, indeed Janssen (2009) explicitly mocks what he calls the “angle-bracket-M-O-sub-i religion” spread primarily (in those days) by the zealotry of John Earman and Michael Friedmann. In fact, Janssen agrees with Brown concerning the relational *ontology* of space-time, endorsing Brown’s statement of this position: “I see the absolute geometrical structures

of Minkowski space-time as parasitic on the relativistic properties of the dynamical matter fields” (Brown, 2005, p.100). To say that space-time is “Minkowski” means nothing more than to say that the dynamical matter fields are Lorentz-covariant. Where Janssen’s view differs from Brown’s account is in his insistence that the reason for the Lorentz-covariance of dynamical laws will not be found in the particular laws (Janssen, 2009): “Lorentz invariance is [...] a property that transcends individual laws even though they remain the bearers of it.” Interestingly, however, Janssen leaves open the question of *what* may be the ultimate explanation for this property of natural laws, accepting that an appeal to “Minkowski space-time” is not sufficient:

Minkowski space-time is unlikely to stand as the final answer to the question of what is responsible for this property of the individual laws. But whatever the deeper answer may turn out to be, it is as unlikely that we shall find it in the individual laws as that we would find the explanation of rectangularity [of painting frames] in individual paintings.

In this section, we will tentatively endeavour to provide an answer, or rather point towards a potential answer, to this question: *Why are all the dynamical laws governing “matter fields” Lorentz-covariant?*

5.3.1 A unifying scheme

Now, as Janssen acknowledges, if it were the case that all matter was constituted electromagnetically, we could easily explain the Minkowski structure of space-time in terms of the Lorentz-covariance of Maxwell’s equations. But there are other forces in nature that also govern some aspects of the material constitution of bodies, and this is why Janssen insists that the explanation for Lorentz-covariance will not be found in particular laws. As Janssen (1995, p.278) remarks, the electromagnetic view of nature was no longer a live option by 1906, the Lorentz covariance of other laws of nature appears to the ether theorist as an unexplained coincidence:

Barring the discovery of a *new unifying scheme* to replace the now bankrupt electromagnetic view of nature, the ether theorist has to accept some unexplained coincidences in the physical world, whereas the relativist can account for these coincidences simply by pointing to the space-time structure posited by her theory.¹³

¹³Emphasis added.

However, since, for reasons explained in the previous section, we are not satisfied with a kinematical account of relativistic effects, it is just this other possibility of a “new unifying scheme” that we must investigate. Suppose for instance that all dynamical laws governing the material constitution of bodies do not operate independently, but are in fact each aspects of a common underlying field. In this case the ether theorist would be able to defend their case by simply identifying this underlying field with what they have called “the ether.” If Einstein’s achievement in his relativistic kinematics was to show that all of the dynamical laws of nature must be Lorentz-covariant, could we not also interpret this move from the dynamicist perspective as the statement that all dynamical interactions propagate through a common underlying medium? It is not necessary to have a specific *unified field theory* already figured out in full for us to speculate on the consequences that a scheme such as this might have for physics. All that is necessary for now is that we consider the possibility that fundamentally all forces come from the same underlying field.

Now Einstein’s achievement in his kinematical theory, as Janssen emphasises, is that the reason for Lorentz covariance is no longer a consequence of the electromagnetic constitution of bodies but a general fact which applies to any force laws that might constitute physical objects. If we insist, as we have, on grounds of Mach’s principle, that Lorentzian effects are not kinematical but must be due to one’s motion with respect to the electromagnetic or other *medium*, the inductive step in Einstein’s reasoning must then be interpreted as the thesis that this medium is *universal*, and not merely electromagnetic. The content of Einstein’s theory therefore, stripped of a metaphysical commitment to the non-existence of an ether frame and to the kinematical origin of Lorentzian effects (and having this committent replaced with the default understanding of light as an excitation of a medium), consists in the claim that the medium—who’s motion experimentalists were trying to detect in the late 19th Century—is not merely the electromagnetic ether, rather, it is a *universal medium* out of which all the matter we build rods and clocks with is necessarily constituted. Again, the making of this inference does not require that we have a particular unified field theory already figured out.

5.3.2 The reason for Lorentz symmetry

Whether we accept the kinematical interpretation of Lorentz covariance or the dynamical one I have suggested above, the question still remains: *why Lorentz transformations, and not some other set of transformations?* The answer we will give, it seems to me, works in favour of our dynamical *unified field* interpretation of relativistic effects.

To investigate the question of why special relativity involves Lorentz transformations—since, as we have seen, the theory itself does not give a justification for its postulates—we

naturally begin by asking: *where else* in physics do we come across transformations such as these? For if we find Lorentz transformations appearing outside of their usual domain in relativistic field theories, these analogies may help give us some perspective on the reason for these transformations.

The Lorentz transformations emerge naturally as the symmetry group of the classical wave equation, independent of any relativistic interpretation. Consider the standard scalar wave equation in three spatial dimensions:

$$\frac{\partial^2 \phi}{\partial t^2} = c^2 \nabla^2 \phi. \quad (5.1)$$

This equation describes the propagation of waves (e.g., sound, light, or other disturbances) at a constant speed c . Mathematically, it is a hyperbolic partial differential equation, and its solutions exhibit a well-defined causal structure: disturbances propagate within light-cones defined by the relation $r = ct$. The key feature is that the form of this equation remains invariant under a specific class of coordinate transformations—namely, the *Lorentz transformations*—which mix space and time coordinates in such a way that the interval

$$s^2 = t^2 - \frac{x^2 + y^2 + z^2}{c^2} \quad (5.2)$$

is preserved. These transformations leave the d'Alembert operator

$$\square = \frac{\partial^2}{\partial t^2} - c^2 \nabla^2 \quad (5.3)$$

invariant, meaning that if $\phi(x, t)$ is a solution in one inertial frame, then $\phi'(x', t')$ is also a solution in another frame related by a Lorentz transformation.

The emergence of Lorentz symmetry from the wave equation has wide-ranging implications across physics. In classical electrodynamics it accounts for the invariance of Maxwell's equations, since demanding the d'Alembert operator remain form-invariant yields the Lorentz transformations (Heras, 2016). In acoustics and elasticity, although the wave speed will differ from c (as here we deal with the speed of sound), the wave equations still exhibit a similar hyperbolic structure, and transformations preserving that structure are analogues of Lorentz transformations (Gregory et al., 2015; Visser, 1998). In condensed matter physics, low-energy excitations in materials such as graphene and Type I Weyl semimetals obey effective Dirac or wave equations with emergent Lorentz symmetry; notably, Type II Weyl semimetals are defined precisely by the violation of this emergent symmetry. Lorentz invariance of the wave operator is also central in the theory of hyperbolic PDEs and field quantization. These diverse connections show that Lorentz symmetry is a general feature of any field theory with

a finite invariant signal speed—not just a consequence of relativity theory, but a structural feature of wave behaviour in general.

The fact that Lorentz symmetry appears as a generic feature of systems obeying the wave equation has proved particularly useful for physicists working on *analogue gravity* models, since the physics associated with fields embedded in curved backgrounds can be reproduced in condensed matter systems, allowing an indirect way to probe the physics of gravitational systems for the cases in which actual experiments are currently inaccessible; the most famous case being the acoustic black holes analogue originally proposed by Unruh (1981). Within this literature, a 2008 paper titled “A Real Lorentz-FitzGerald Contraction”, by Barceló and Jannes is particularly instructive for our purposes (Barceló and Jannes, 2008). In this paper, Barceló and Jannes show that “internal observers” in a condensed matter system, interacting solely with emergent relativistic fields and quasi-particles, cannot detect their uniform motion relative to the medium they inhabit. The authors construct a thought experiment: a Michelson-Morley interferometer is built, not from external lab materials, but from emergent quasi-particles within the condensed matter system. They show that such a quasi-interferometer experiences a *real* Lorentz-FitzGerald contraction due to interactions mediated by effective relativistic fields, where the speed of sound in the medium plays the role of the speed of light. This contraction ensures that interference patterns remain unaffected by uniform motion through the medium, thus rendering it impossible for *internal observers* to detect any absolute rest frame, even though one exists *externally* (the laboratory frame).

A very similar argument was made in a paper by Schmid and Kroupa from 2014, titled “The Spheronic Toy Universe: How Special Relativity may be Visualised to Emerge from a Wave-Nature of Matter” (Schmid and Kroupa, 2014). Here, they consider a Euclidean space filled with a classical medium which allows for sound waves and admits certain standing spherical wave solutions known as “spherons”. They show that this setup will lead to a theory of special relativity if we assume the point of view of observers which are themselves made up of this spheronic matter. Of course, if there also existed bodies in this universe which were not constructed out of this “spheronic matter”, and therefore—using the nomenclature of Barceló and Jannes (2008)—were “external” to the medium, it would still be possible for “internal” observers to deduce their velocity with respect to the medium, as Todd et al. (2021) for instance have shown. For Schmid and Kroupa, since propagating spherons “show properties which can be described as being of quantum mechanical nature” and “automatically obey the laws of special relativity”, their thought experiment:¹⁴

¹⁴Later in the paper, after showing how Lorentz contraction and time dilation follow naturally from their

shows how special relativity can be seen as possibly being a direct consequence of the wave nature of matter [and] may be a useful didactic argument for introducing the concepts of quantum mechanics and special relativity. (Schmid and Kroupa, 2014)

Indeed in conversation, Kroupa has even gone so far as to suggest that had quantum mechanics been discovered prior to special relativity, the latter might not have needed its own revolution and could simply have been interpreted as a *theorem* of quantum mechanics, following naturally from the wave-nature of matter.

This idea, that special relativity follows generically from the “wave nature of matter”, is what we propose as the answer to the question which Janssen left open: *what is responsible for the Lorentz-covariance of the dynamical laws of nature?* The crucial difference between Lorentz invariance in special relativity and Lorentz invariance for “internal” observers constituted out of some given medium, is that in the latter case, objects “external” to this medium—i.e. which are not themselves materially constituted out of waves of this medium—exist. According to this way of thinking, the postulates of SR consist in nothing more than the hypothesis that there are *no external observers*, from which follows the generality of the relativity principle and the light postulate as epistemic constraints on what can be ascertained from *local* measurements.

5.3.3 Janssen and Balashov’s “no Newtonian a priori” objection

The final objection that we will address here was expressed clearly by Balashov and Janssen (2003). As we will see, given the contents of earlier chapters of this thesis, we are in a particularly suitable position to be responding to it. Janssen and Balashov are defending relativistic kinematics against what they call the “neo-Lorentzian” view which attributes the cause of Lorentz-contractions to the motion of a rod with respect to the ether which is itself embedded within a background *Newtonian* space-time:

why is a rod in motion shorter than a rod at rest *rather than equally long*? For those who share the Newtonian presupposition implicit in this why-question, the neo-Lorentzian interpretation provides a very satisfactory answer to this question. Contrary to what one would expect in Newtonian theory, the forces holding the rod together are not Galilean invariant but Lorentz invariant. As a

spheronic model, they express this as “*It may thus be argued that contraction and time dilation are no presents from above but that they are rather simple consequences of the wave-nature of matter*” (emphasis in original).

consequence, the equilibrium state of a rod in motion with respect to the privileged frame is shorter than the equilibrium state of a rod at rest. For co-moving observers, however, it will *appear* to be the other way around since their clocks will not read the true time of the privileged frame but the Lorentz-transformed time of the moving frame (Dorling, 1968). For those who do not share the Newtonian presupposition of the why-question, this answer completely misses the mark. Instead, such a person would simply point out that the presupposition is wrong. There is no *a priori* reason to think that space and time will be Newtonian. In fact, the universality of the behavior of the rod (i.e., any physical system whatsoever will exhibit the exact same contraction) suggests that space and time are Minkowskian. Length contraction is part of the normal spatio-temporal behavior of systems in Minkowski space-time. There is nothing further to explain.

A little later in the paper, Balashov and Janssen reiterate the same crucial point in a footnote:

Since the invariance group (Lorentzian or Galilean) of the dynamical laws essentially is the space-time structure for a relationist, the (effective) Lorentz invariance of the dynamical laws in a sense does seem to explain for a relationist why the (effective) space-time structure is Minkowskian. Such an explanation, of course, does not amount to an explanation of why the space-time structure is Minkowskian *rather than Newtonian*, unless the relationist has some *a priori* reason to expect the structure of space and time to be Newtonian.

What we will argue is not that there is an *a priori* reason to regard space-time as *Newtonian*, but there *is* an *a priori* reason to consider space and time, abstracted from the universe's material contents, as Euclidean (for reasons which we have discussed in chapter 2). To be more specific, we should consider it as a Euclidean geometry stripped of any physical content, such as a prescription (like the law of inertia) for the default behaviours of bodies which relies on the existence of a preferred set of reference frames.

Like the symmetry identified by Galileo, the Lorentz transformations are established as the correct transformations only when a relatively small body or system is set in motion relative to the rest of the universe. Indeed, we can make the stronger claim that they are valid *only* in this regime, since if the system contains a sufficiently large mass the Lorentz symmetry characteristic of special relativity would break down due to *frame dragging*. Now, if the Lorentz group were an *a priori* form, these transformations should apply to the universe as a whole. But we could not then guarantee that they would also apply to subsystems moving with respect to the rest of the universe, since this would remain a question that should

be answered *a posteriori*. Instead, we find the opposite, the Lorentz transformations do *not* apply to boosts of the universe as a whole (the considerations by Bondi discussed at the end of §5.2 make this clear), nor to systems containing large amounts of mass; even if we treat the substantial ether as a fiction and consider a boost (in a Leibniz-shift type scenario) of the universe as involving only the so-called “ponderable bodies,” the frame-dragging induced by their motion will distort the usual Lorentzian effects that apply when a small subsystem is boosted. The Lorentz transformations are only a *local* symmetry of relativistic systems, their applicability arises from epistemic constraints on *local* measurements (which, following Schmid and Kroupa (2014), we have suggested is related to the “wave nature of matter”), and they are not grounded in any *a priori* sense of the *relativity of motion* of which we spoke earlier in this thesis, unlike what Poincaré—and following him, Einstein—seemed to believe. Lorentz transformations are therefore valid only for sufficiently small subsystems of the universe, and epistemically justified only in this restricted regime.

Now, the transformations that *do* apply to the universe as a whole, as we have argued previously, are the group of *shape-preserving* transformations (comprised of the rigid transformations and scale transformations). Unlike symmetry requirements applied to subsystems, the requirement of this symmetry for the universe as a whole has no empirical content; these are not *actual* transformations. The purpose of requiring this symmetry for the universe as a whole rather is not at all based on empirical considerations, it is instead entirely *methodological*, they are employed as a means of elucidating the *causal relations* among things (as was also the case as we saw for the principle of conservation of energy). In this case, our requirement tells us that since the Lorentz symmetry of subsystems *breaks* the shape-preserving symmetry of the whole, the cause of this deviation must be sought in the material with respect to which the given subsystem is boosted; regardless of whether that cause is found in some substantial ether, or for instance in the remainder of the universe’s masses (as is suggested by the phenomenon of frame-dragging). The same applies to accelerated frames: inertial forces also break the symmetry, and their cause must be sought in the material relative to which the acceleration occurs, as we discussed in the previous chapter.

We can now see that our earlier defence of Delboeuf’s conception of space is closely tied to Mach’s principle and leads naturally to an unconventional interpretation of special relativity. The Machian critique of Newtonian spacetime, as we have developed it, requires that we identify a cause of inertial motion, since such motion breaks the symmetry of the shape-preserving group by selecting preferred frames. By the same reasoning, we must also identify a cause for Lorentzian effects, insofar as they too break this symmetry. In both cases the causes must be located within the universe, and the symmetry-breaking should arise only

from real or active transformations of subsystems relative to their surroundings.

Summary

The central argument made in this chapter was that the “relativity principle” of special relativity doesn’t seem to have very much to do with the *relativity* of space, but rather, along with the constancy of the light speed, appears to be a natural consequence of the wave-equation, or as [Schmid and Kroupa \(2014\)](#) put it, “special relativity can be seen as possibly being a direct consequence of the wave nature of matter”. We also argued that the Machian position on special relativity (by Machian I mean consistent with our analysis of “Mach’s principle” in chapter 4) ought to be opposed to any kinematical interpretation of Lorentzian effects, and should not be seen as aligned with the rejection of the ether’s existence, unlike what is often argued. Thirdly, we gave a novel defence of the apriority of Euclidean geometry by distinguishing on one hand *a priori* symmetries of the universe as a whole from *empirically rooted* symmetries of subsystems moved with respect to their environment. We also saw that this reasoning is intimately linked to the Machian critique of Newtonian kinematics, according to which the symmetries of subsystems should be explicable in terms of the character of their environment, but not of some conceptual constructions such as “space” and “time.”

In the next chapter, we will examine the complicated relationship of Mach’s principle with both the development and aftermath of Einstein’s second “relativity” theory, general relativity (GR). It is important for us to address this topic before we move on in the two final chapters to consider suggestions that have been made from outside of the context of GR. As we will see, some of the interesting themes that were developed in models separate from GR have also made an appearance in the GR centred literature. Moreover, for the last 111 years, GR has been positioned as the dominant framework for thinking about space, time, and gravitation, and has thereby played a decisive role in setting the boundaries of what is regarded as legitimate philosophical reflection on these topics. Our contemporary understanding of Mach’s principle has likewise been filtered through its complex and often ambivalent relationship with GR. It is therefore essential that we examine how GR came to appropriate, reinterpret, or sideline Mach’s idea, which will give us crucial context for understanding how to interpret the alternative approaches that we turn to later.



SECRET HOPES

Ho, ho, ho, that is gut, Ja!

Albert Einstein, 1955

Is relative motion at a distance even possible?

Introduction

The purpose of this chapter is to explore the status of “Mach’s principle” in Einstein’s theory of “general relativity,” as well as to examine some of the role it has played over the decades in guiding research on cosmology. We have chosen to limit the scope of our discussion by focussing on two key figures: John Wheeler and Dennis Sciama, who both played important roles in fostering the intellectual environment that enabled what is often referred to as the “renaissance of general relativity” (spanning roughly 1955-1975).¹ Both were deeply concerned with “Mach’s principle,” although as we shall see the lessons they drew from this core idea diverged substantially. Whereas Wheeler advocated extensively for the association of Mach’s principle with the idea of a *closed* universe (due to arguments which originated with Einstein), Sciama, in his early work leveraged Mach’s principle to derive an intriguing relation $U = c^2$ (see §6.2), which has brought about speculation of an entirely different nature, often associated with the critical density condition for a flat cosmos. While our discussion

¹This of course leaves out the work of Robert Dicke on the “Brans-Dicke theory” which unfortunately we will not be addressing directly in this thesis due to a lack of space.

of Wheeler sheds light on the widespread lack of clarity concerning Mach's principle that characterised the intellectual environment in the lead up to the renaissance period, our discussion of Sciama helps to clarify the reasons for the eventual decline of research on Mach's principle in the 1980s, and why its resurgence in decades since has been limited and largely remained marginal to the broader physics community.

The other key figures that we will spend some time discussing here are Hermann Weyl and Albert Einstein. Both men belonged to the previous (i.e. the original) generation of relativists, both died in 1955, at the designated start of the "renaissance" period, and both were also deeply influential on Wheeler and Sciama. Weyl had a particularly keen grasp of the conceptual content of general relativity and its implications for Mach's principle as we will see in section 6.1.3. Einstein, as the originator of the theory, is of course responsible for setting all of these ideas in motion, however, he was also responsible for some unfortunate conceptual mistakes that have led to a certain obfuscation of the Machian programme. Ironically, just as we saw in the previous chapter that Einstein's theory of special relativity is in conceptual tension with what we called *Mach's principle*, we will argue here that Einstein's general theory wound up being conceptually incompatible with what we have called *Mach's hypothesis* (see section 6.1.3).

The distinction we developed in chapter 4 of this thesis between Mach's principle and Mach's hypothesis is not generally respected in the literature that we will discuss in the present chapter. Indeed this is part of the reason why, we claim, there has been so much dispute historically about the definition of Mach's principle. In the proceedings to the 1993 Tübingen conference on Mach's principle, Barbour and Pfister (1995, p.530) identify 21 distinct possible meanings of "Mach's principle" which are discussed at various points throughout the volume. These include: a general appeal to the principle of sufficient reason, some kind of extension of the principle of special relativity to accelerations, a hypothetical inductive interaction for inertial forces, and a requirement for spatial closure to eliminate cosmic boundary conditions (as we will discuss in this section). Some of these definitions are explicitly *cosmological* in their scope whereas others do not immediately appear to be so. What we can say with some clarity however is that at the core of what has been called 'Mach's principle' there is a *hypothesis* about the origin of inertia, which in a more or less vague manner implies the *unity of the universe*, and thereby connects the physics of every-day systems to their place within this undivided whole. It is essential to this form of reasoning that it is possible to consider the totality of the material world in some sense as a single object, which (importantly) lacks any reality outside of itself, or with respect to which it might be referred.

In these more abstract and general terms, Machian thinking has often been generalised

beyond the specific domain that Mach discussed (for example [Barbour \(2010, 2001, 1981\)](#)). It thereby becomes fair to describe as “Machian” various realisations that the universe as a whole should have no particular *size, position, time, velocity* or overall *angular velocity* on the basis that these spatio-temporal specifications can only be defined *relationally*, between two or more real things. These considerations bring “Mach’s principle” in close association with certain “relativity principles”, and this is often how Mach’s principle has been understood. However it is important to note that if Mach’s principle is taken as a mere requirement for the relativity of spatio-temporal quantities, Mach’s principle is essentially agnostic to the empirical content of physical theories (see chapter 4); something must be added to these requirements—a hypothesis—in order to consider Mach’s principle as more than just a formal requirement on theory construction. This key ingredient is what [Misner et al. \(1973, p.547\)](#) call the “democratic principle” that not only is inertial motion relationally defined, but that all of the massive bodies of the universe play a proportional part in this relational determination (specifically a part proportional to their mass and usually inversely proportional to their distance from the point in question).

This chapter is split into three parts. In section 6.1, we discuss the argument—first put forwards by Einstein and subsequently promoted more widely by Wheeler—that Mach’s principle requires that the universe should be spatially closed. In section 6.1.3 we discuss Hermann Weyl’s 1924 dialogue “Massenträgheit und cosmos” which will help us to clarify the status of Mach’s principle in GR and conceptual difficulties it faces. Finally, in section 6.2, we discuss a wholly different manner in which Mach’s principle may be leveraged to place constraints on models of cosmology, focussing especially on the promising developments in Dennis Sciama’s early work just prior to the renaissance period and the subsequent decline of this promise by the mid 1970s. In our discussion of Wheeler’s promotion of the universe’s closure we will be citing extensively from his *relativity notebooks*.² Our discussion of Dennis Sciama’s engagement with Mach’s principle is informed by close reading of his doctoral thesis ([Sciama, 1953a](#)).³

²We will be citing especially from Wheeler’s first relativity notebook, which we abbreviate as WR1 following the citing convention of [Blum and Brill \(2020\)](#). Citations from the second relativity notebook as well as the seventh will be abbreviated WR2 and WR7 respectively. These notebooks are to be found in the John Wheeler Papers, held at the American Philosophical Society in Philadelphia, Section V, Volumes 39, 40 and 54.

³Sciama’s thesis has not previously been cited in published literature and was recovered by the present author from the University of Cambridge archives.

6.1 Wheeler’s closed cosmos

To understand how Einstein and Wheeler both came to associate Mach’s principle with the closure of the universe, we have to look closely both at the historical context of their ideas: How did Einstein’s winding path towards GR lead him to favour the closed universe model? What was the context through which Wheeler got interested in GR and how did this lead him to follow Einstein’s preference?

6.1.1 Einstein

It is often forgotten just how central was the role of Mach’s principle in Einstein’s initial development of general relativity.⁴ Two principles initially motivated and guided Einstein in his development of the theory: the equivalence principle and Mach’s principle, which were seen as complementary to one another. Whereas the equivalence principle suggests that gravity and inertia ought fundamentally to be unified such that gravitationally affected motion in one frame may be interpreted as inertial motion in another, Mach’s principle suggests that, like gravity, inertia has its origin in mass-mass interactions. The two together thus harmoniously pointed towards a theory in which the inertia of a mass would be defined with respect to other masses, and the medium through which the relativisation of inertia would be accomplished would be the gravitational field.

After Einstein completed the field equations in November 1915 ([Einstein, 1915](#)), he initially hoped his theory would easily vindicate the ‘relativity of inertia.’ Einstein’s Machian convictions about his theory are expressed in many of his correspondences, for instance in a letter to Schwarzschild he writes:⁵

The essence of my theory is precisely that no independent properties are attributed to space itself. Jokingly one may put it this way. If I let all things in the world disappear, according to Newton the Galilean inertial space remains, according to my perception, however, nothing remains.

However, it soon became clear that certain non-Machian features—especially vacuum solutions and the need for boundary conditions that effectively re-introduce inertial structure ‘at infinity’—could not easily be eliminated. Einstein spent 1916–1918 trying to secure a more

⁴For a much more thorough and detailed discussion of Einstein’s engagement with Mach’s principle throughout his career, see [Hofer \(1994\)](#).

⁵Einstein to Schwarzschild, January 9, 1916. Schwarzschild *Nachlass*, Niedersächsische Staats- und Universitätsbibliothek Göttingen, N 193, 3–5. Extract translated by [Schemmel \(2005\)](#). See also [Schulmann et al. \(1998, Doc. 181\)](#).

fully Machian framework by restricting admissible solutions, discussing these issues most of all with Willem de Sitter (Hoefler, 1994).⁶ Einstein at first suggests that the theory will be made Machian if the components of the metric at infinity are either 0 or ∞ such that they are immune to coordinate changes, however around December 1916, possibly prompted by objections from de Sitter, Einstein became convinced that this strategy was no longer viable and suggested instead to remove the need for boundary conditions altogether which would be achieved if the universe is of overall positive curvature such that it is spatially closed.⁷ His argument would be published in Einstein (1917) where he also famously introduces his cosmological constant Λ in order to ensure that his closed universe with non-zero mass density does not collapse upon itself. In 1918, Einstein adds a section on cosmology to the new edition of his popular booklet “Über die spezielle und allgemeine Relativitätstheorie: Gemeinverständlich” (Einstein, 1918b) in which he argues that the universe should be spatially closed provided that there is an average matter density greater than zero everywhere in space (Einstein, 2015):

If we are to have in the universe an average density of matter which differs from zero, however small may be that difference, then the universe cannot be quasi-Euclidean. On the contrary, the results of calculation indicate that if matter be distributed uniformly, the universe would necessarily be spherical (or elliptical). Since in reality the detailed distribution of matter is not uniform, the real universe will deviate in individual parts from the spherical, i.e. the universe will be quasi-spherical. But it will be necessarily finite. In fact, the theory supplies us with a simple connection between the space-expanse of the universe and the average density of matter in it.

All this of course is prior to the development of experimental cosmology and evidence of Hubble expansion, which would undermine the argument since the critical density will no

⁶It is in the resulting paper, “Prinzipielles zur allgemeinen Relativitätstheorie” (Einstein, 1918a), that Einstein for the first time names “Mach’s principle” (*Machsches Prinzip*), presenting it alongside the relativity principle and the equivalence principle as one of the three principles on which general relativity is supposed to rest, and formulating it as the requirement that the $g_{\mu\nu}$ -field be fully determined by the masses of bodies.

⁷It is worth noting that the possibility of a spatially closed universe as a solution to Einstein’s field equations had in fact first been raised by the astronomer Karl Schwarzschild in a letter to Einstein as early as February 1916, where he noted that elliptic geometry could be derived from the theory under a suitable assumption about the stress-energy content of the universe (Schemmel, 2005). Einstein showed no sign of having absorbed the suggestion at the time, and although he discussed Mach’s principle with Schwarzschild explicitly, it was only through the subsequent debate with de Sitter that he was moved to develop the idea of the closed universe himself; I am thankful to Matthias Schemmel for recently bringing this to my attention. See also Hoefler (1994, p.311-312) for a discussion of Einstein’s rejection of his initial *divergent-boundary conditions* approach and the role de Sitter played in this.

longer be zero.

In some later texts Einstein emphasised the connection with Mach's principle. For instance, in his 1934 collection of essays *Meine Weltbild* (Einstein, 1934b), he endorses the connection between Mach's principle and the closed universe (Einstein, 1934a):

In my opinion the general theory of relativity can only solve [the Mach problem] satisfactorily if it regards the world as spatially self-enclosed. The mathematical results of the theory force one to this view, if one believes that the mean density of ponderable matter in the world possesses some ultimate value, however small.⁸

In summary, Einstein endorsed the spatially closed spherical universe model for two separate reasons: (1) the necessity, as he saw it to ensure that boundary conditions at infinity are eliminated, (2) working prior to knowledge of the Hubble expansion, Einstein thought that a universe of mean finite density, no matter how small should be spatially closed.

Both of these motivations would ultimately be undermined by advances in experimental cosmology, and many of Einstein's contemporaries would reject his arguments. Nonetheless, the notion that the universe should be closed on grounds of Mach's principle survived well into the 1980s, and this has a lot to do with the influence of John Wheeler on a new generation of relativists in the renaissance period of general relativity. In the next section, we will examine the development of Wheeler's thoughts on this issue through the lens of his relativity notebooks, focussing in particular on the first notebooks which includes the notes Wheeler recorded as he began teaching his first class on general relativity at Princeton in 1953.

6.1.2 Wheeler

Like Einstein, Wheeler's interest in gravitation was intimately bound up with the Machian programme from the start. The reason for this was that over the course of the 1940s, Wheeler had collaborated with his student Richard Feynman to develop an action-at-a-distance (AAD) reformulation of electrodynamics involving a combination of advanced and retarded potentials propagating from particles in both directions time-wise (Wheeler and Feynman, 1945). For Wheeler this was part of a programme to reconsider physics from the perspective of a particle-only ontology in which all electromagnetic radiation must have a source and a

⁸We know also that this particular passage influenced Wheeler as he quotes from it in the epigraph to his section on Mach's principle in the influential textbook *Gravitation* (Misner et al., 1973), which was based on the lecture series he taught at Princeton on general relativity.

destination (an “absorber”) (Blum and Brill, 2020). It was in this context that Wheeler began turning his attention towards gravitation in the late 1940s, hoping that he would be able to recover general relativity in a similar manner through an AAD framework. Wheeler struggled to develop this theory, and gradually came to the conclusion that he should focus on studying general relativity on its own terms, and naturally, Mach’s principle was his way in as it provided the perfect analogue of the absorber condition in AAD electromagnetism. Since there is no better way to learn a subject than to teach it, Wheeler persuaded the physics department at Princeton to let him teach their first course on GR. This was an important moment for the renaissance of GR as many of Wheeler’s students in this period would go on to become important relativists.

The notebook entries discussing his first classes teaching evidence Wheeler’s own evolving knowledge of general relativity. Of particular interest are Wheeler’s notes from a class given on the 31st of March 1953 concerning the interpretation of the Schwarzschild solution. The issue is raised that the solution seems to contradict Mach’s principle since it is Minkowski at infinity, meaning that a test particle infinitely far away from the central mass will still have inertial properties despite there not being any other matter with respect to which these inertial properties would be defined. Wheeler uses this issue to explain to his class that the field equations alone are insufficient as a mathematical realisation of Mach’s philosophy and that implicitly, the Schwarzschild solution “corresponds to a many mass



Figure 6.2: John Archibald Wheeler.

world—point mass, plus masses at large distances” (WR1, p.103).⁹ Wheeler then also claims that “there is a one-to-one correspondence between mass distribution and metric only when space closes up on itself,” a point which he will at various times later on remind himself to search for a proof of.¹⁰ He then recounts introducing the idea of the closed universe:

⁹Incidentally, a similar point has been made very recently by Shi (2025), who argues that the apparent vacuum freedom of standard vacuum solutions (paradigmatically Schwarzschild) is often a modelling artifact: by “curtaining” off matter, one replaces it with boundary conditions that encode an “echo” of external or ignored sources. On this reading, non-vanishing Weyl curvature in a vacuum region represents influence from outside the modelled subsystem, rather than “gravity from nothing.”

¹⁰See WR1 (p.105) in which he highlights this question, adding a note in the margin: “Very important question of principle.”

Describe how many such masses allow a closing up of the space on itself. Connection between mass density and curvature qualitatively of form $\frac{1}{\text{rad of curv}} \sim \rho_m$.

At this point, it seems as though what Wheeler has in mind is the original Einstein universe in which any positive non-zero density “ ρ_m ” will lead to closure. Interestingly, it is his students that raise the objection: “Question raised in class whether mass density enough to permit open or closed universe, in view of expansion rate” (WR1, p.104). Wheeler then responds that this must mean that the density must be assumed to be above the critical threshold emphasising that:

closure so fundamental to whole Mach idea that in present state of knowledge think of density value as having to yield precedence to Mach principle.

Interestingly, Wheeler then adds another argument in favour of the closure of the universe on grounds of Mach’s principle: “if think of inertial properties of a particle in a given region of space as coming from summation over all other regions of space, à la $1/r^2$, that system should have to close up to allow convergence. Recall presence of sum in acceleration analysis of inertia” (WR1, p.104). It is not clear that Wheeler is right about this however, since if the integration is assumed to be performed over the past light cone for instance, one would need to justify why the mass content of the universe is not integrated over infinitely many times. Moreover, the condition that an integral over the mass of the universe must converge might better be leveraged as an argument for the universe’s temporal horizon rather than an argument for it’s spatial closure. Wheeler does show some awareness that there may be further issues here, writing a note in the margin: “Investigate whether light ever gets to cross universe (closed boundary conditions).” Later on in the notebook Wheeler also begins to flirt with the question of whether temporal closure should be required in addition to or perhaps instead of spatial closure;¹¹ the clearest passage expressing this is (WR1, p.134):

We have been concerned¹² how to formulate the condition that the universe close up in such a way, or with such completeness, either in space or in time or in both, that the connection between distribution of matter and metric shall become unique.

As his teaching of the course went on, Wheeler took the chance when possible to consult with various colleagues including Hermann Weyl, Eugene Wigner and John von Neumann.

¹¹Mentions of possible temporal closure can be found in p.108, p.122 and p.134 of the first relativity notebook.

¹²Here the word “concerned” replaced the original word “worried” which Wheeler crossed out.

On the 13th of April, Wheeler recounts asking von Neumann about the question of uniqueness between the metric and matter distribution in a spatially closed universe, noting that von Neumann “had no feel for uniqueness question” (WR1, p.120). In general, Wheeler received pushback from his colleagues who raised various issues with the viability of Mach’s idea in GR. Weyl appears to have been the colleague with whom Wheeler discussed the issues the most, indeed Weyl’s comments were much more substantive than those of other colleagues which has a lot to do with the fact that Weyl had written a charming and perceptive dialogue discussing the status of the Machian programme in the aftermath of general relativity back in 1924.

Weyl’s dialogue remains to this day one of the most perceptive analyses of the issues that Mach’s principle faced in aftermath of general relativity, and already contained all the answers that Weyl needed to give Wheeler. In the next subsection, we take a break from our story concerning Wheeler to examine in detail some of the key lessons that Weyl’s dialogue has to teach us. As we will see, Weyl’s account of the philosophical issues surrounding Mach’s principle in this dialogue is very similar to that which we have given in earlier chapters of this thesis.

6.1.3 The problem of relative motion at a distance

Weyl’s dialogue “Inertia and cosmos” involves a conversation between two philosophically inclined physicists, Petrus and Paulus, who are discussing the status of Mach’s principle a few years after the publication of Einstein’s general relativity in 1915. Petrus is troubled by Paulus’s apparent turn away from Mach’s principle in the years since they had both initially embarked on a study of Einstein’s theory (Weyl, 1924):¹³

Dear friend! When we met again last night after a long separation, I couldn’t help but constantly think back to the time of 1915, when we first joined forces in an enthusiastic study of the theory of relativity, in shared passion and shared dreams for the future. Back then, we almost believed we already had the law of the universe within reach, explaining all phenomena completely! Since then, I, too, have learned to be critical and become “wiser.” However, it has pained me

¹³Translation of: “Lieber Freund! Als wir uns gestern Abend nach langer Trennung wiedersahen, mußte ich während unseres Gesprächs beständig an die Zeit von 1915 zurückdenken, die uns zuerst in gemeinsamem eifrigen Studium der Relativitätstheorie zusammenführte, in gemeinsamer Begeisterung und gemeinsamen Zukunftsträumen. Damals glaubten wir ja fast, das Weltgesetz schon in Händen zu haben, das alle Erscheinungen restlos erklärt! Seither habe auch ich wohl Kritik gelernt und bin „weiser“ geworden. Aber das hat mich doch fast schmerzlich betroffen, daß du dich sogar von der Grundidee losgesagt zu haben scheinst, die ich nach wie vor als den Kernpunkt der neuen Lehre ansehen muß.”

deeply that you seem to have abandoned even the foundational idea, which I still consider the central point of the new theory.

Paulus has said that he can no longer believe the central point of the theory, which Weyl denotes as proposition **M**: “*the inertia of a body arises from the interaction of all the masses in the universe*”,¹⁴ which corresponds to what, in this dissertation, we have called the *Mach hypothesis* (hypothesis 2). Petrus then gives a brief and elegant explanation of this idea as follows:¹⁵

Newton’s opinion was: the plane in which the pendulum swings remains fixed in absolute space; the fixed stars also remain almost still in absolute space. Thus, the pendulum plane moves with the fixed stars and rotates relative to the Earth. But Einstein explained: there are only relative motions; the intermediary concept of absolute space is as questionable as it is superfluous. Not this phantom, but the truly existing immense masses of the fixed stars of the entire cosmos maintain or guide the pendulum plane. The Earth flattens because it rotates—not absolutely, but relatively—against the fixed stars. If you deny this view, I don’t know what remains of the general theory of relativity.

Paulus responds by at first agreeing with the appeal of Mach’s hypothesis and acknowledging that it was a central motive for the development of Einstein’s theory:¹⁶

if that interpretation tracing back to Mach could truly be carried out, I too would find it extraordinarily appealing; it provides a simple, vivid, and internally powerful answer to the problem of motion. There is also no doubt that this interpretation—alongside the equivalence of inertial and gravitational mass—was the main motivation for Einstein in developing the general theory of relativity.

¹⁴Translation of: “*die Trägheit eines Körpers durch das Zusammenwirken aller Massen des Universums zustande kommt.*”

¹⁵Translation of: “Newtons Meinung war: die Ebene, in welcher das Pendel schwingt, bleibt erhalten im absoluten Raum; die Fixsterne stehen auch fast still im absoluten Raum. Deshalb geht die Pendelebene mit den Fixsternen mit und rotiert relativ zur Erde. Einstein aber erklärte: Es gibt nur relative Bewegungen; das Zwischenglied des absoluten Raumes ist so fragwürdig wie überflüssig. Nicht dieses Gespenst, sondern die wirklich vorhandenen ungeheuren Fixsternmassen des ganzen Kosmos halten oder führen die Pendelebene. Die Erde plattet sich ab, weil sie – nicht absolut, sondern relativ – zu den Fixsternen rotiert. Wenn du diese Auffassung ableugnest, so weiß ich nicht, was überhaupt noch von der allgemeinen Relativitätstheorie übrigbleibt.”

¹⁶Translation of: “Wenn jene auf Mach zurückgehende Deutung sich wirklich durchführen ließe, wäre sie auch mir außerordentlich sympathisch; sie gibt eine einfache, anschauliche und in sich kräftige Antwort auf das Problem der Bewegung. Kein Zweifel auch, daß sie – neben der Gleichheit von schwerer und träger Masse – für Einstein das wichtigste Motiv war zur Ausbildung der allgemeinen Relativitätstheorie.”

Furthermore, he comments on the physical vacuity of the principle of general covariance, which Einstein had initially mistaken for a direct implementation of Mach's hypothesis:¹⁷

Finally, I agree with you that one must seek the core of the theory in such a concrete physical statement rather than in a formal-mathematical principle, such as the principle of the equivalence of all coordinate systems. This principle, which unfortunately gave the theory its name, is fundamentally empty of content; for the laws of nature can, under all circumstances and regardless of their form, be expressed as “invariant under arbitrary coordinate transformations.”

The principle of “equivalence of all coordinate systems”, more commonly known as “general covariance”, is what we discussed in the third section of chapter 1 as the broader class of *passive transformation*. We distinguished from these the “Leibniz-shifts” which are a shape-preserving subset of these. Likewise, in Weyl's dialogue, Paulus also distinguishes general covariance from the “kinematic principle of the relativity of motion” which he notes is also physically vacuous in the absence of a “physical premise”:¹⁸

Likewise, the kinematic principle of the relativity of motion is meaningless by itself unless supplemented by the physical premise that **C**: *all events are causally determined by matter, i.e., by the charge, mass, and state of motion of the elementary components of matter.*

Petrus then expresses his strong agreement with the principle, noting that “indeed, all our causal thinking in physics is guided by it.” At this point, the account we have given in earlier chapters differs slightly from that of Weyl, since, following [Schopenhauer \(1819\)](#), we had made it the *definition* of matter that it include anything to which the law of causality applies (see §1.2). In this case, Weyl's principle **C** would appear as *analytic*. For Weyl on the other hand, the term “matter” refers specifically to masses, charges and their motion, but not to what he will later call the “guiding field”.¹⁹

¹⁷Translation of: “Endlich bin ich mit dir darin einverstanden, daß man in einer derartigen konkreten Aussage physikalischen Inhalts den Kernpunkt der Theorie suchen muß, nicht aber in einem formal-mathematischen Prinzip wie dem von der Gleich- berechtigung aller Koordinatensysteme. Dies Prinzip, das unglücklicherweise der Theorie ihren Namen gegeben hat, ist ja im Grunde ganz inhaltsleer; denn die Naturgesetze lassen sich unter allen Umständen, sie mögen lauten wie sie wollen, “invariant gegen- über beliebigen Koordinatentransformationen” formulieren.”

¹⁸Translation of: “Ebenso ist das kinematische Prinzip von der Relativität der Bewegung für sich nichtssagend, wenn nicht die physikalische Voraussetzung hinzutritt, daß (c) *alle Geschehnisse kausal eindeutig bestimmt sind durch die Materie, d. h. durch Ladung, Masse und Bewegungs- zustand der Elementarbestandteile der Materie.*”

¹⁹This is the first indication that Weyl had not fully taken on board the more fundamental consequence of Mach's critique of classical causation, a point which we will return to shortly.

Now we come to Paulus's actual objection to Mach's hypothesis, which hinges on his reluctant rejection of principle C:²⁰

You are correct: for my part, I cannot uphold Principle C because I see the impracticality of Principle M a priori. I claim that (A) *according to the general theory of relativity, the concept of relative motion between several separate bodies is just as untenable as the concept of the absolute motion of a single body.*

The issue is more elaborately explained a little later:²¹

What our sense of sight testifies as rotating around the Earth are not the stars themselves but the "stellar compass," which, at the point where I stand, is formed by the directions of the light rays coming from the stars and striking my eye at any given moment. And this is a significant difference because between the stars and my eye lies the "metrical field", which determines the propagation of light and is, according to the theory of relativity, just as mutable as the electromagnetic field. This metrical field is as important for the direction in which I perceive a star as the star's actual location itself.

Paulus then explains how formerly our notion of the relative motion of bodies at a distance was made possible due to our notion of a rigid reference system:²²

But how, in the general theory of relativity, is it possible to compare the states of motion of two *separate* bodies? In Mach's time, of course, when the rigid reference body still existed, this was possible; one could ideate an island of mass—like

²⁰Translation of: "Du hast recht: Ich für meine Person kann C nicht aufrechterhalten, weil ich die Undurchführbarkeit von M a priori einsehe. Ich behaupte nämlich, daß (A) *nach der allgemeinen Relativitätstheorie der Begriff der relativen Bewegung mehrerer getrennter Körper gegeneinander ebensowenig haltbar ist wie der der absoluten Bewegung eines einzigen.*"

²¹Translation of: "Was sich nach dem Zeugnis unseres Gesichtssinns um die Erde dreht, sind nicht die Sterne, sondern der "Sternen- kompaß", welcher hier an der Stelle, wo ich mich befinde, gebildet wird von den Richtungen der Lichtstrahlen, die in einem Augen- blick von den Sternen her auf mein Auge treffen. Und das ist ein wesentlicher Unterschied; denn zwischen den Sternen und meinem Auge befindet sich das "metrische Feld", welches die Lichtausbreitung determiniert und nach der Relativitätstheorie ebenso veränderungsfähig ist wie das elektromagnetische. Dieses metrische Feld ist für die Richtung, in der ich einen Stern erblicke, nicht minder wichtig wie der Ort des Sternes selbst."

²²Translation of: "Aber wie sollte es in der allgemeinen Relativitätstheorie möglich sein, den Bewegungszustand zweier getrennter Körper miteinander zu vergleichen? Zur Zeit Machs freilich, als man noch den starren Bezugskörper hatte, war das möglich; da konnte man sich eine Masseninsel, wie es unsere Erde ist, als starren Körper, dessen Maßverhältnisse ein für allemal durch die euklidische Geometrie festgelegt sind, ideell über den ganzen Raum erweitert denken, und dann etwa konstatieren, daß die Sonne sich relativ zu ihm bewegt. Aber unter den Händen Einsteins hat sich das Koordinatensystem so erweicht (Einstein selber spricht ja gelegentlich von einem "Bezugsmollusken"), daß es sich simultan der Bewegung aller Körper in der Welt anzuschmiegen vermag; du kannst sie, wie sie sich auch bewegen mögen, mit einem Schlage alle "auf Ruhe transformieren."

our Earth—as a rigid body whose dimensions were fixed once and for all by Euclidean geometry, imaginatively extended throughout the entire space, and then state, for example, that the Sun moves relative to it. But under Einstein's hands, the coordinate system has softened so much (Einstein himself occasionally speaks of a “reference mollusk”) that it can simultaneously conform to the motion of all bodies in the world; you can transform all of them “into rest” with one stroke.

Paulus proceeds to give a visual demonstration of this fact by considering picturing space-time as a “mass of plasticine, threaded with fibers that represent the [non-intersecting] world lines of material particles”. Via continuous deformations of the plasticine, all fibers can be made to “become vertical straight lines” such that:²³

both the Earth and all the fixed stars are at rest in the reference system represented by the plasticine. However, the stellar compass still rotates relative to the Earth exactly as we observe it.²⁴

Weyl's analogy here beautifully demonstrated how the actual role of the principle of general covariance was in effect to render the “metrical field” into something physically real. It is in this sense that we claim the theory of general relativity, while aligned in a sense with *Mach's principle*, is in fact directly opposed to *Mach's hypothesis*, according to which the physical properties of the “metrical field” ought to be reducible to the relative motion of masses at a distance.

After a somewhat defeated response by Petrus, Paulus introduces Weyl's concept of the “guiding field”, which had formerly been based on a “formal-geometric structure of the world”, but now, through its assimilation of gravitational phenomena, the guiding field is “variable and depends on matter”. He then explains that the true achievement of Einstein's theory is this principle **G**, that is:²⁵ “to *replace the geometric distinction between uniform and*

²³Translation of: “So ruhen die Erde und alle Fixsterne in dem durch das Plastelin dargestellten Bezugssystem, aber der Sternenkompas dreht sich dennoch in bezug auf die Erde genau so, wie wir es beobachten.”

²⁴Note that in Wheeler's notes on his conversation with Weyl of the 2nd of April 1953, he recounts Weyl making an argument along these lines to him and notes (WR1, p.111): “JAW did not argue for his own strongly held contrary view, that things that count (spacetime coincidence) are real + well defined + independent of choice of coordinate system, and that it is therefore indeed reasonable to have one to one correlation between distribution of matter + metric. Should try to prove this pt via coincidence philosophy.” The name “JAW” here refers to himself (John Archibald Wheeler).

²⁵Full passage reads: “Von der alten zu der neuen Auffassung G der Dinge übergehen, heißt: den geometrischen Unterschied zwischen gleichförmiger und beschleunigter Bewegung ersetzen durch den dynamischen Unterschied zwischen Führung und Kraft.”

accelerated motion with the dynamic distinction between guidance and force:²⁶

I argue that what I have explained so far, concisely formulated in the two propositions **G**, alone enters into physics and underpins the specific investigations of the problems of relativity theory. The much broader Machian principle **M**, according to which the fixed stars mysteriously influence the course of earthly events, remains pure speculation, has only cosmological significance, and will only become relevant to natural science when astronomical observation gains access to the entire universe, not just an island of stars.

This then leads into the second part of Weyl’s dialogue which involves the reconsideration of Mach’s hypothesis, which has now evolved into a speculative question about cosmology, which we will come back to shortly.

6.1.4 Influence on Wheeler

As we saw, the main problem that Weyl had identified in the dialogue was that GR, by undermining the classical notion of a rigid reference system, poses serious difficulties for the central presupposition of Mach’s proposal, that is, the possibility of unmediated spatio-temporal relations at a distance (Weyl, 1924):

according to the general theory of relativity, the concept of relative motion between several separate bodies is just as untenable as the concept of the absolute motion of a single body.

Nonetheless, the dialogue is sympathetic towards Mach’s principle, acknowledging it’s appeal and emphasising its centrality to the motivations for the development of GR.

Wheeler’s first discussion with Weyl concerning Mach’s principle takes place on the 2nd of April 1953, here Wheeler recounts proposing to Weyl that he support “Einstein’s view that metric and distribution of matter only then have a one to one relationship when it is demanded that solutions $R_{ik} = 0$ or T_{ik} close up.” Weyl, Wheeler reports, is “inclined to regard this view as completely unconvincing”. Some days later, on the 14th of April 1953, Wheeler

²⁶Translation of: “Ich meine: was ich bisher dargelegt und in den beiden Sätzen **G** knapp formuliert habe, das allein greift in die Physik ein, liegt den tatsächlichen Einzeluntersuchungen von Problemen der Relativitätstheorie zugrunde. Das weit darüber hinausgehende Machsche Prinzip **M** aber, nach welchem die Fixsterne mit geheimnisvoller Macht in den Gang der irdischen Geschehnisse eingreifen sollen, ist bis jetzt reine Spekulation, hat lediglich kosmologische Bedeutung und wird darum für die Naturwissenschaft erst von Belang werden können, wenn der astronomischen Beobachtung nicht mehr nur eine Sterneninsel, sondern das Weltganze zugänglich ist.”

has Weyl round to dinner and makes a note following their chat (WR1, p.125): “Doesn’t believe that Ein. theory = Mach’s prin. See Petrus - Paulus in die Naturwiss,” which is clearly a reference to the dialogue. On the 18th of April (WR1, p.129), Wheeler shows that he has read the dialogue taking note of two of the main points that Weyl raised: (1) the impossibility of defining unmediated relative motion at a distance, and (2) that Mach’s principle is “pure speculation until we can observe the universe as a whole.” Despite the pushback that Wheeler received from Weyl, Wigner and von Neumann, he nonetheless stubbornly continues to stand up for what he saw as Einstein’s position that the closure of the universe will resolve his difficulties (WR1, p.121): “I am inclined to agree with Einstein, + disagree with Weyl + von N. + Wigner but haven’t yet seen way to do a proof”.



Figure 6.3: Hermann Weyl.

Despite Wheeler’s resistance to Weyl’s critique of his reading of Mach’s principle, Wheeler held Weyl’s dialogue in high regard and appears to have been influenced by it quite substantially. It is well documented that Wheeler had a deep admiration for Weyl. Evidence of this can be found in the address he prepared for the *Internationaler Hermann-Weyl-Kongress*—held in celebration of the centenary of Weyl’s birth—in which Wheeler writes (Wheeler, 1986): “If I had to come up with a single word to characterize Hermann Weyl, [...] it would be that old-fashioned word, so rarely heard in our day, ‘nobility.’ I use it here [...] also in the sense of showing exceptional vision.”²⁷ Concerning the issue at hand however, it appears that Wheeler was quite profoundly influenced by Weyl’s dialogue on Mach’s principle specifically. Wheeler’s book (along with Thorne and Misner), *Gravitation*—which represents the culmination of the knowledge accrued over the course of Wheeler’s teaching of general relativity at Princeton—specifically cites Weyl’s 1924 dialogue (which is described as “delightful”) along with works by Dennis Sciama as further reading (Misner et al., 1973, §21.12, p.543). Although as we saw, he had already located and read Weyl’s dialogue in April 1953, Wheeler was careful to keep a stained cutout from a letter he received from Weyl dated to May of that year in which Weyl details exactly where to find the dialogue in question (WR2, p.24).

It is interesting to remark here too that it is quite plausible that it was his reading of Weyl’s dialogue that inspired Wheeler to adopt the same format for a pivotal talk he gave in

²⁷See (Furlan, 2020) for some discussion of Weyl’s influence on Wheeler.

the autumn of 1953 in Tokyo. Wheeler’s Tokyo talk is cast as a dialogue between two heroic figures from Japanese history: “Saigo Takamori” and “Sugawara no Michizane”. Wheeler’s Tokyo talk, which was on the problems of elementary particle physics, and the associated “Tokyo program” (WR2, p.35), brought together many of the ideas that Wheeler had been toying with that year and previously, and represented a certain shift in Wheeler’s thought from focus on a particle based ontology (especially in the 1940s) to a field-first approach (Blum and Brill, 2020).²⁸ It is plausible that Weyl directly influenced Wheeler in this shift too, especially given that Weyl’s dialogue follows a very similar conceptual progression, coming to terms with the necessity of a field-first picture of nature, and thus moving away somewhat from the Machian view.

Wheeler’s Tokyo talk involves a brief discussion of Mach’s principle, during which Wheeler makes a new argument, not related to the Einstein closed-universe idea. Wheeler considers a simple AAD gravity model with an interaction potential representing inertia in addition to the Newtonian and analogous to the long-distance Liénard-Wiechert field of an accelerating charge in electrodynamics (Blum and Brill, 2020). Wheeler’s simple expression gives a *relative inertia + gravity* model characterised by the equation of motion:

$$\frac{Gm_1m_2}{r^2} - \sum_k \frac{Gm_1am_k}{c^2r_k} = 0, \quad (6.1)$$

which reduces to the normal Newtonian equation of motion provided that:

$$\frac{G}{c^2} \sum_k \frac{m_k}{r_k} = 1, \quad (6.2)$$

where the sum extends over all of the masses of the universe m_k which are at distances r_k from the mass m_1 . This curious equation (6.2) has long been associated with Mach’s principle, and has a winding history of being forgotten and rediscovered independently (sometimes with slightly different constants of proportionality), including by Schrödinger (1925).²⁹ As we will see shortly, Wheeler’s argument is remarkably similar to the derivation made by Dennis Sciama in his article “On the origin of inertia” (Sciama, 1953b) published earlier that same year, although Sciama was working with a simplified vector potential gravitational theory also based on some analogies with electromagnetism. Blum and Brill (2020) have discussed the striking similarity to Sciama’s paper, noting that:

²⁸See Blum and Brill (2020) for a deep dive into Wheeler’s Tokyo Programme as well as the English translation of his talk in Tokyo (which had formerly only be published in Japanese translation).

²⁹We will be returning to this topic in chapter 8 which includes a thorough overview of many of the derivations.

It is unclear whether Wheeler knew of Sciama’s argument and merely rephrased it in AAD terms, or whether he had found it independently in his attempts at constructing an AAD version of gravity, building on an AAD formulation of electrodynamics.

Since Sciama’s paper was not discussed in Wheeler’s talk or in the lead up to it in the notebooks, it seems most likely that we are witnessing a convergence of thought, however there is one source that we know Wheeler had read that may have influenced him to derive this expression, namely, Weyl’s dialogue. At the start of the second half of Weyl’s dialogue, Petrus recounts the “well known result of Thirring” (namely from [Thirring \(1918\)](#)) that a “rotating hollow sphere H (representing the celestial sphere)” produces forces analogous to centrifugal forces on a test body at its centre ([Weyl, 1924](#)). Petrus then explains that:³⁰

one can explain, in Machian terms, the centrifugal force and the flattening of the Earth as effects of the starry heavens rotating around the stationary Earth, if one assumes that the mean distance of the stars is as large as the gravitational radius of their total mass.

By the “gravitational radius” Weyl is referring to what we would call the Schwarzschild radius $r_s = \frac{2GM}{c^2}$. Petrus’s argument therefore implies that this cosmological Machian criterion might be satisfied provided that:³¹

$$c^2 \sim \frac{GM_u}{R_u}, \quad (6.3)$$

with M_u and R_u as the mass and radius of the universe respectively, which is an expression that is essentially equivalent to that derived by Wheeler, and which indeed Wheeler would continue to endorse later on. In fact, in the 1973 textbook *Gravitation*, Wheeler includes a retelling of Thirring’s argument concluding with the same result as Weyl, that the expression:³²

$$\sum_{\text{far away stars}} \frac{m_{\text{star}}}{r_{\text{star}}} \sim \frac{m_{\text{universe}}}{r_{\text{universe}}} \quad (6.4)$$

³⁰Translation of “Man wird danach in Machscher Weise die Zentrifugalkraft, die Abplattung der Erde als eine Wirkung des um die ruhende Erde sich drehenden Sternenhimmels erklären können, wenn man annimmt, daß die mittlere Entfernung der Sterne so groß ist wie der Gravitationsradius ihrer Gesamtmasse.”

³¹Note that [Schrödinger \(1925\)](#) derives a result of the same order the following year in a classical model using a similar method to Wheeler, and this is often characterised as an original discovery ([Schrödinger, 1995](#); [Assis, 1999](#)). However it should be noted that Schrödinger’s paper actually cites Weyl’s dialogue in the introduction, so it is possible that Schrödinger too caught wind of it from there.

³²Note that it is safe to assume that Wheeler would have written the parts of the textbook relevant to Mach’s principle given that Wheeler was much more involved with this topic than Misner or Thorne; moreover that section of the book strongly reflects Wheeler’s sensibilities on the issue.

“must be of order unity” (with units chosen such that $G = c = 1$), and adds a remark that this result may be consistent with his preference for closed universe models: “Just such a relation of approximate identity between the mass content of the universe and its radius at the phase of maximum expansion is a characteristic feature of the Friedman model and other simple models of a closed universe” (Misner et al., 1973, p.548, eq.21.160). Although it is plausible that Weyl influenced Wheeler in this regard, it is also perfectly possible that Wheeler arrived at it independently.³³

What is important to understand here with this last result is that the Thirring-Weyl type argument is quite different in nature from the Einstein-Wheeler argument for the closed universe. The logic of the argument is of a totally different nature even though both appear to proceed from a prior commitment to Mach’s principle. As we will see in the next section, Sciama (as mentioned), developed this argument much more clearly, but he was more inclined to regard it as an endorsement of a flat universe of critical density, rather than a spatially closed one.

As we have seen, many of the questions concerning Mach’s principle that Wheeler raised in his early relativity notebooks were vague and inconclusive, the arguments used to favour the closed universe model didn’t seem to hold up to scrutiny. Fast forwards several years and we see that the situation remains much the same. In his 7th relativity notebook, covering the period from 1959-1960, while sat on a train to Syracuse, Wheeler writes an interesting passage reflecting on a seminar titled “Comments on Mach’s principle” he had recently given which was attended by various notable figures including Roger Penrose (WR7, p.150). Wheeler recounts that there was “much discussion [following his talk of] whether retarded or simultaneous potentials” should be used, indicating that even this basic consideration has not been decisively resolved by 1959. Among the points listed requiring further examination, Wheeler includes: the question of the closure of the universe, the meaning of $1/r$ in the potential, whether the potential is *retarded* and what is the mass-energy term that should be taken as the source. Additionally, Wheeler reports that Penrose raised questions both concerning whether inertia at a point should be determined by the interior of the light cone or merely the light cone surface, and concerning the technicalities of temporal closure. Overall, Wheeler’s notes following his talk appear to show little evidence of conceptual progress on the issues since 1953. The idea of the closed cosmos, which had been initiated by Einstein in

³³Note for instance that Wheeler’s remark about the sum over the inertial influence of matter needing to converge based on the “presence of sum in acceleration analysis of inertia” is given in p.104 of his notebook and precedes his reading of Weyl’s dialogue. It is plausible that this kind of reasoning refers to a simple calculation similar to that which Wheeler presented in Tokyo. What can be said with certainty however is that this kind of argument did not originate with Sciama (1953b) but was already somewhat more widely recognised.

the context of a universe thought to be eternal, roughly homogeneous and non-expanding, continued to endure in association with Mach's principle, although the arguments in favour of this concept appear to have received little clarification.

6.2 Sciama's lost promise

6.2.1 Youthful speculations

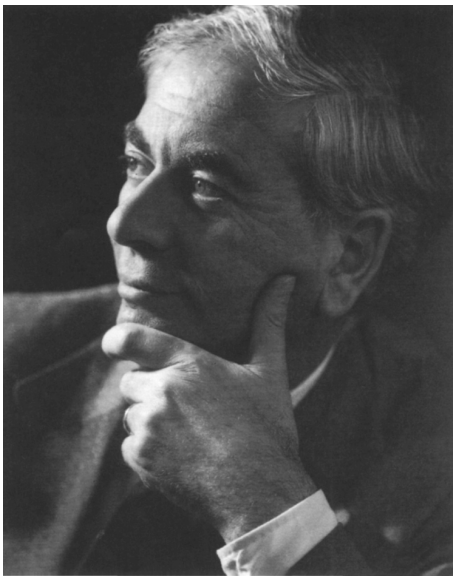


Figure 6.4: Dennis W. Sciama.

Dennis Sciama's initial and most lasting contribution to the literature on Mach's principle comes from his article "On the origin of inertia" ([Sciama, 1953b](#)), published in 1953, which is incidentally the same year that, as we saw, Wheeler developed many of his own views on the topic while teaching general relativity at Princeton. Sciama came at the issue from a somewhat different angle to Wheeler; he considered it a failure of general relativity that it sustained non-Machian solutions such as empty Minkowski space which lacks matter but in which a test-particle nonetheless has inertial properties, and thought therefore that an alternative theory should be developed where these would be eliminated by construction. In a brief correspondence

with Einstein spanning from December 1950 to January 1951, Sciama expresses some criticisms he had of general relativity and in particular the general covariance based approach which he sees as physically vacuous. In its place he advocates for a strong sense of invariance which he calls "form invariance" rather than "value invariance" (relating the former to the invariance we see in SR and the latter to that of GR which he criticises). This form invariance should concern only the conformal subgroup of the diffeomorphic group according to Sciama.³⁴ In 1955, two weeks before Einstein's death, Sciama would travel to Princeton to meet him. In an interview with Spencer Weart ([Weart and Sciama, 1978](#)), Sciama recounts that he started the conversation by telling Einstein: "I've come to talk about Mach's principle and I've come to defend your former self against your later self," to which he reports that Einstein responded: "Ho, ho, ho, that is gut, Ja!"

³⁴Dennis W. Sciama to Albert Einstein, 11 January 1951, EA 20-470, Albert Einstein Archives, Hebrew University of Jerusalem.

Sciama's 1953 paper is an edited version of the third chapter of his PhD thesis of the same name (Sciama, 1953a) supervised by Paul Dirac. In the second chapter of his thesis, while addressing the failure of general relativity to implement Mach's principle, Sciama comments on the Einstein argument for the closed universe in a passage discussing Einstein's introduction of the cosmological constant Λ (Sciama, 1953a, p.29):

The point of this modification was that the new equations have a solution corresponding to a closed space (Einstein universe), so that no difficulty arises with boundary conditions at infinity. This space is full of matter and Mach's principle is satisfied, so that Einstein originally thought that he had found a complete solution to the problem of inertia. However, De Sitter (1917) pointed out that the modified equations still had a solution corresponding to empty space (de Sitter universe), so that they are still not completely consistent with Mach's principle. Furthermore, the discovery a few years later of systematic red-shifts in the spectra of distant nebulae showed that the static Einstein universe was in conflict with observation. Einstein therefore gave up all hope of satisfying Mach's principle and withdrew the term, for which he now saw no justification.

Sciama struggled to create a rank-2 tensorial theory that would implement Mach's principle, although he ultimately regarded that this was necessary due to "kinematical considerations" (Sciama, 1953a, p.33). Concerning the intended rank-2 theory, Sciama remarks that "the use of such a potential leads to rather involved mathematics which tends to obscure the physical significance of the theory," he therefore opted to start out by constructing a vector-potential theory as a proof-of-concept model which works by explicit analogy with Maxwellian electromagnetism and captures the physically significant results of the intended theory. Unlike general relativity, Sciama's theory is inherently cosmological in scope and *attempts* to incorporate Mach's principle by construction in its very foundation by linking the strength of gravity directly to the large scale structure of the cosmos.

$$\Phi = - \int_V \frac{\rho}{r} dV, \quad \bar{A} = - \frac{1}{c} \int_V \frac{\bar{v}\rho}{r} dV. \quad (6.5)$$

Although, instead of the charge and current density of surrounding matter as sources of Φ and $\bar{A}$, the sources of these gravito-inertial potentials are now the mass ρ and momentum $\rho\bar{v}$ densities of the relevant surrounding matter. Since, unlike charge, mass is always positive, distant matter is no longer neutral and the integration must be performed over the entire observable universe V , where r is the distance of the matter density being integrated from the relevant point at which we are calculating the potentials. From this simple model, Sciama

was able to derive both inertial and gravitational effects simultaneously, however the correct centrifugal and Coriolis forces were only produced if the strength G of this gravito-inertial coupling is given by the expression:

$$U = c^2 \quad (6.6)$$

where $U = -G\Phi$ is the gravitational potential of the universe with respect to a test-particle. In particular, the Coriolis field is generated by the Gravito-magnetic field $\bar{A}$ of a rotating universe, and the centrifugal field is generated by a relativistic modification of Φ in a rotating frame (Sciama, 1953b).³⁵ Therefore, according to the model, inertia can only be explained entirely in terms of the gravitational influence of distant matter if this equation holds exactly. Unlike the results of Wheeler and Weyl based on Thirring's considerations as we saw previously, this is not an order-of-magnitude estimate but a precise condition. Assuming a smoothed out model of homogeneous and isotropic universe of density ρ expanding with a simple Hubble law $\mathbf{v} = \mathbf{r}/\tau$ where $\mathbf{v}$ is the velocity of matter at distance $\mathbf{r}$ and τ is a constant, and adding that "matter receding with velocity greater than that of light makes no contribution to the potential," Sciama was able to integrate Φ giving the condition $2\pi G\rho\tau^2 = 1$, which is conspicuously similar to the critical density condition in Friedmann models (which differs by a factor of 4/3).³⁶

Reflecting on the consequences of equation 6.6, Sciama (1953a, p.40) notes that it implies that the total energy (inertial and gravitational) of a particle at rest in the universe is zero, in particular, the energy that will be needed for a given particle of mass m_p to escape the gravitational well due to the rest of the matter in the cosmos ($m_p U$) is exactly equal to its rest-energy ($m_p c^2$), which was an attractive result for Sciama particularly since at this time he was working in the context of the Steady State model of cosmology, and this zero-energy condition would allow for matter creation without violating energy conservation. Incidentally, although there is no evidence that Sciama was aware of this at the time, the same condition had been proposed in 1939 by Pascal Jordan (Jordan, 1939), based precisely on this requirement that the total energy contribution of any given mass to the universe is zero, but entirely independently of considerations of Mach's principle.³⁷ At one point in

³⁵Note that Sciama's 'toy-model' is imperfect and borrows results from special relativity without being a rigorous relativistic field theory. A more thorough presentation of Sciama's model, as well as a discussion of its shortcomings will be presented in chapter 8.

³⁶Note that Sciama's simple model here is based upon basic assumptions that differ substantially from those underlying the Friedmann equation. We will see shortly, based on remarks by Bondi (1995) why there is a more robust case for a connection to the critical density condition.

³⁷Jordan had been led to consider variable- G cosmologies due to Dirac's observation of the famous "large number" coincidences (Dirac, 1938), rather than a study of Mach's principle. I thank Alexander Blum for alerting me to Jordan's paper in connection with Sciama's result.

Wheeler's notebooks he expressed Einstein's notion of a closed space as a result which is much more profound than what Mach's principle initially seems to suggest (WR1, p.104): "Mach principle, say it leads to something much deeper than showed on surface — idea of closed space." We might say that for Sciama the *deeper* result that Mach's principle leads to is the idea of the zero-energy universe; although it is clear that Sciama did not evangelise for his result as vigorously as Wheeler who had the authority of Einstein's writings to appeal to.

In the 1950s, Sciama was admired by his friends and colleagues for his openness and enthusiasm about the Steady State model and Mach's principle. Notably, he was responsible for inspiring his friend Roger Penrose to switch from pure mathematics to astrophysics and cosmology. In an interview conducted by the author, Penrose explains "he did decide that he thought I was somebody worth getting interested in physics. So I learned an awful lot of physics from him. [...] I didn't feel that I liked applied mathematics very much. Yes, but I did think Dennis, he excited me with regard to physics, and Mach's principle was certainly part of it." Penrose also recounts an anecdote that captures the kind of speculative discussions Sciama often pursued with him:³⁸

In order to try and interest me in these topics, he liked to talk to me. As he drove, he would decide he wanted to go to a Shakespeare plays in Stratford, and so he would suggest I come with him. And on the route there's a, a windy road with sort of cliffs on the side. Yes. And as you went round this windy road, and you get thrown to the side of the car as you go around, he would say, "That's the action of the fixed stars." So the inertia was fixed by the fixed stars, so he was taking the blame away from him. [...] And so we used to discuss, okay, well, suppose the stars were eliminated one by one, then what would happen to inertia? Well, he'd say, "Well, the inertia would be determined by the earth." Okay, well, let's eliminate the Earth, then it would be determined by the car. So we used to talk about that sort of thing, if we would take all of the electrons and the protons, and take them away, then inertia would get ill-defined when you had nothing left.

Many other fond memories of Sciama can be found in a collection of essays written in celebration of his 65th birthday (Ellis et al., 1993). In Malcolm MacCallum's contribution, he reports concerning Sciama's prejudice towards Mach's principle (p.231): "his own opinions were strongly enough held that we used to joke that if we wanted to stop research all we

³⁸Interview with Roger Penrose, 18 July 2023.

need do was say loudly in the tea-room that we did not believe in Mach's principle. But it was a very supportive and stimulating atmosphere for which I will always be grateful."

Despite his early vivaciousness, the latter part of his career seems to have involved a progressive demoralisation on those issues of early interest. Sciama underwent a shift from his status as a young pioneer advancing his own bold ideas to a mentor figure who is now primarily remembered for having been doctoral advisor to a long list of astrophysicists and cosmologists who subsequently became very prominent in their fields.³⁹ Having been an enthusiastic advocate of the Steady State cosmology in his early career through his associations with his mentors Hermann Bondi and Thomas Gold, Sciama was rather quick to accept the fall of the model once enough evidence had mounted against it, especially after one of his students, Martin Reece, had played a key role in interpreting radio-astronomical observations of the CMB that provided evidence against the model in the mid-1960s. Sciama later acknowledged that the demise of the Steady State theory was, in a certain sense, a disappointment, particularly for those physicists who were drawn to it for its conceptual boldness. In an interview with Spencer Weart in 1978, he remarked that he had often been struck by a contrast in attitudes toward the theory, and that he had tended to associate sympathy for Steady State with a more imaginative cast of mind ([Weart and Sciama, 1978](#)):

I often tried to correlate the psychology of people with whether they supported or decried the Steady State theory or were just neutral, I'm not sure I ever got very far doing that, I couldn't help thinking that on the whole the more imaginative people seemed to like the theory, not necessarily think it was true, but at least were sympathetic.

Despite the decline of the Steady State theory, for quite some time longer, Sciama continued to investigate whether Mach's principle could provide a guiding light towards the selection of cosmological models. Later in the interview with Weart, Sciama indicated that his gravitational-inertial energy balance expression ($U = c^2$) derived in 1953 might correspond to the critical density condition of the Einstein-de Sitter model ([Weart and Sciama, 1978](#)):⁴⁰

³⁹These include: George Ellis (1964), Stephen Hawking (1966), Brandon Carter (1967), Martin Rees (1967), Gary Gibbons (1973), James Binney (1975), John D. Barrow (1977), Philip Candelas (1977), David Deutsch (1978), Adrian Melott (1981) and Antony Valentini (1992). For more on Sciama's influence on a generation of relativists, see the collection [Ellis et al. \(1993\)](#) which was written in celebration of Sciama's 65th birthday.

⁴⁰Note that [de Swart \(2020\)](#) characterises this passage as evidence that Sciama "had a different preference [to Wheeler] but similar reasoning for preferring a universe with a specific density." However, as we have seen, the reasoning involved is altogether different, despite both arguments being based on considerations of Mach's principle.

You see, if you take these Newtonian analogues of Milne and McCrea, the Einstein-de Sitter model is the one where the total energy of the universe is zero, the kinetic energy and the negative gravitational potential energy just balancing. Well, if you think that kinetic energy manifesting inertia is due to gravitation, then you might intuit that the most Machian way of having one made by the other would be if there's equal amount of energy, which would give you uniquely the Einstein-de Sitter model, I still have a secret hope that that might turn out so, but it may well not.

This connection to the Einstein-de Sitter model is expressed more explicitly by Hermann Bondi in his brief talk in Tübingen (Bondi, 1995, p.475). Here he discusses the quantity $4\pi\rho G\tau^2$ which a Machian condition such as Sciama's eq. 6.6 fixes to be constant over the course of cosmological evolution:

If you say that this quantity, which of course is a pure number, has a particular value, that tells you something either about the mean density of the universe, about its time scale, or about the constant of gravitation (which can be regarded as the ratio of gravitational to inertial forces). You can read it as you wish, but in almost any way you read it, it suggests something which is very Machian: a link between the universe and a local quantity like the constant of gravitation. [...] It is constant in only two cosmologies that I know of: the Steady State theory and the Einstein-de Sitter model. For, since the time scale is τ effectively $R/\dot{R}$,

$$4\pi G\rho\tau^2 = \frac{4\pi G\rho R^2}{\dot{R}^2}.$$

In a Friedman universe with negligible pressure $\rho \sim R^{-3}$:

$$4\pi G\rho\tau^2 \sim \frac{1}{R\dot{R}^2}.$$

The only case where this is a constant is if R varies like $t^{2/3}$, which is the Einstein-de Sitter model.

Bondi and Samuel (1997) would later list the condition $4\pi\rho G\tau^2 \sim 1$ as one of 10 possible formulations of Mach's principle. We may note here that the quantity $4\pi\rho G\tau^2$ ought also to be held constant according to the argument which Wheeler endorsed drawing on the results of Thirring (1918), which would undermine Wheeler's claim in Misner et al. (1973, §21.12)

that this relation is compatible with “the Friedman model and other simple models of a closed universe.”⁴¹

6.2.2 Decline of Sciama’s Machian programme

Unfortunately, this line of reasoning was not developed any further in his career, and this had a lot to do with the subsequent research on Mach’s principle that Sciama’s colleagues carried out. In his 1953 paper, Sciama had promised a follow-up article, referred to as “II” which would implement the same physical ideas in a tensorial theory capable of reproducing the empirical successes of general relativity. The difficulty that Sciama faced with this task is made clear by the fact that the promised paper only arrived 16 years after the first, and moreover, failed to deliver the results that so many had appreciated in the original paper.

The second paper (Sciama et al., 1969) came out of a collaboration with Peter Waylen and Robert Gilman (a former student of Wheeler who had been influenced by Sciama’s 1953 article) in July of 1969. At this point, Sciama had just published a book titled *The Physical Foundations of General Relativity* (Sciama, 1969a)—in April of that year—in which he endorses the Machian status of general relativity and discusses how Mach’s principle and the equivalence principle played foundational roles in the development of the theory. It is not clear exactly when this happened, but by this point Sciama is no longer bothered by the objection he had had in 1950 to Einstein’s use of general covariance and seems to have undergone a process of reconciliation—or perhaps of compromise—with the growing community of relativists that he was involved in mentoring. The aim of Sciama et al. (1969) was to rewrite the Einstein field equations in integral form, and this required using much more sophisticated and conceptually challenging mathematical tools involving *Greens functions* which Sciama’s collaborators helped him navigate, a development which Sciama lamented given that it opposed his personal preference for technical simplicity and clarity of thought.⁴² Following the development of this toolkit, Gilman wrote a follow-up paper (Gilman, 1970) applying these methods to Mach’s principle and cosmological models in which he determined that “many (and probably all) nonempty relativistic Robertson-Walker models with $\Lambda = 0$ and $0 \leq p \leq \rho c^2$ are acceptable Machian cosmologies.” The original result ($U = c^2$) that had been

⁴¹Wheeler implicitly acknowledges in saying that the relation only holds for closed Friedman models “at the phase of maximum expansion,” however the reason why it should only hold here and not at other points in time is not given.

⁴²See this expressed in the interview with Weart (Weart and Sciama, 1978): “I’m afraid so, but I feel forced by the physics to be that complicated.” Note also that around this time, other researchers were independently developing similar approaches to the implementation of Mach’s principle based upon Greens functions, including: Al’Tsuler (1967); Lynden-Bell (1967).

present in Sciama 1953 paper was nowhere to be found, and it seemed that Mach’s principle could no longer be leveraged in a meaningful way for the selection of cosmological models.

By the mid 1970s, Sciama as well as his Machian colleague Derek Raine were saying similar things. At the Colloque International CNRS No. 220 (Ondes et radiations gravitationnelles) held at the *Institut Henri Poincaré* in Paris in June 1973, Sciama gave a talk reporting the latest results of his and colleagues’ work on the integral formulation of general relativity (Sciama, 1974). In the subsequent Q&A, the French physicist Olivier Costa de Beauregard, who clearly had some prior familiarity with Sciama’s work asked him:⁴³ “In this approach, do you arrive at a precise statement regarding the value of GM/c^2L ?” (where GM/c^2L refers to the quantity which as we have seen numerous sources have suggested should be of order unity on the grounds of Mach’s principle). Sciama responds:

Unfortunately not. All Robertson-Walker models turn out to be Machian. The reason is that the Green’s function adjusts itself whatever the value of the present mean density of matter in the universe, to give compatibility with M_1 and M_2

where M_1 and M_2 refer to two “Mach conditions” discussed in the paper. After this paper, Sciama no longer published on the topic of Mach’s principle. In the mid 1970s he appears to pivot towards a focus on dark matter, black holes and other topics in astronomy. Interestingly, it was just around this time, in 1974, that Sciama would be responsible for green-lighting the publication of a short paper by Julian Barbour in *Nature* (Barbour, 1974) which would greatly benefit Barbour’s career, enabling him to collaborate with Bruno Bertotti and subsequently become the world’s leading voice on ‘Mach’s principle’, a role which had arguably been held by Sciama before this.⁴⁴ Sciama did not attend the 1993 Tübingen conference on Mach’s principle (co-organised by Barbour), although his 1953 work was cited at various points by participants. Raine’s article in the conference proceedings mentions Sciama’s “toy model” of inertia and summarises the predicament in which the research programme culminated (Raine, 1995):

Mach’s Principle as originally conceived was supposed to supply a reason for the existence of the matter in the universe, in the quantity found, through the

⁴³Translation of: “Dans cette approche, aboutissez-vous a un énoncé précis concernant la valeur de GM/c^2L ?”

⁴⁴Note that in a follow-up paper, Barbour (1975) uses a simple classical model, similar to one 60 years prior by Reissner (1915), from which he derives a relation given as $GM/Rc^2 \sim 1$ (with M and R representing the mass and radius of the universe), which he associated with the “cosmic coincidences” of Dirac (1938). Later on however, Barbour (1995) would argue that general relativity is a “perfectly Machian theory” on the basis of his collaborative work with Bruno Bertotti (Barbour and Bertotti, 1982) in which the possible relative configuration of masses of classical mechanics is replaced with the “superspace” of possible Riemannian 3-geometries. In this context the condition $GM/Rc^2 \sim 1$ no longer has any relevance.

relation $G\rho\tau^2 = 1$ (i.e., an $\Omega = 1$ universe). In fact, we find that any FRW universe fulfills the conditions, so we need a different explanation [...] Most of the competing explanations (inflation, anthropy) also require the universe to be reasonably isotropic. This leaves precious little for Mach's Principle to explain! In this case Mach's Principle is simply true by accident.

That same year (1993), Sciama would publish *Modern Cosmology and the Dark Matter Problem* (Sciama, 1993), which was in part an update of a previous book *Modern Cosmology* from 1971 (Sciama, 1971). Despite transferring much of the material from the first book, Sciama entirely removed any mention of Mach's principle from his new publication.⁴⁵

While Gilman's results seemed to have dissuaded Sciama and Raine from further pursuing the Machian programme, interestingly, de Beauregard was not affected in the same way. In 1999 (the year of Sciama's death), in defiance of the pessimistic results of his British and American colleagues, de Beauregard wrote a short article on Mach's principle in which he endorsed Sciama's original $U = c^2$ result, connecting it both to Einstein's equivalence principle and the unification of mass and energy (de Beauregard, 2000):

Compare the formulas $W_g := Um_g$ and $W_i := c^2m_i$, the first expressing the gravitational potential energy of a test particle of passive gravitational mass m_g and the second the energy equivalent to its inertial mass m_i . The equality $m_g = m_i$ was inferred from a thought experiment by Galileo. Assuming the equality $W_g = W_i$ is an updated form of Mach's conjecture. The equality $U = c^2$ then follows. This means that inertial mass is induced via gravitational potential energy, expressing an equivalence of the two Einstein equivalences. In other words, assertion of the equivalence of mass and energy, and the equivalence of inertial and passive gravitational mass, are equivalent if Mach's principle is true.

De Beauregard's perspective on this issue is quite intriguing, however the paper is not widely cited and this view does not seem to have penetrated into mainstream cosmology or physics in general. Both de Beauregard (2000) and Woodward and Mahood (1999) (among others) endorse the view that Sciama's key contribution to the story concerning Mach's principle was to introduce the equivalence $U = c^2$. In Woodward and Mahood's paper it is claimed in addition to this that in the *linearised* (Maxwell-like) context: "Sciama shows that, up to factors of the order of unity, U must be equal to $-GM/R$, M and R are the mass and radius of the

⁴⁵For instance, chapter 8 of Sciama (1971) survives almost intact in section 3.2 of Sciama (1993) apart from the introductory section and the final section on "cosmological coincidences" (i.e. the Dirac large number coincidences) which were the two sections in which Mach's principle had been discussed.

observable universe, and the density of matter is about 10^{29} g/cm^3 —that is, about “critical” (or closure) density.” Since at the time, supernova data suggested that the density should be below the critical threshold, this was seen as a potential problem and Woodward and Mahood appeal to the results of [Gilman \(1970\)](#) and subsequent development by [Raine \(1981\)](#) to argue that general relativity “dictates that inertia is gravitationally induced irrespective of whether or not cosmic matter density is critical,” without realising that this result entirely undermines the initial result $U = c^2$ that they had started out by endorsing. Some others such as [Vigoureux et al. \(2003\)](#) on the other hand have continued to speculate along the lines of [Bondi \(1995\)](#), arguing that Sciama’s $U = c^2$ result leads to a strong condition on cosmology that can provide a concrete approach to interpreting and resolving various cosmological questions including the horizon problem, the homogeneity of the CMB and the issue of the universe’s flatness.

In recent times, the leading physicist who seems to be actively endorsing the view that Sciama’s $U = c^2$ is equivalent to the critical density condition is C.S. Unnikrishnan ([Unnikrishnan, 2022](#), p.216-7), who’s views concerning both special and general relativity appear to closely mirror those expressed in Bondi’s short talk at Tübingen.⁴⁶ However Unnikrishnan’s proposals have so far been largely ignored by the broader astrophysics and cosmology research communities. Attention to some of these ideas has also been brought by Alexander Unzicker who, among other things, points to the convergence between Mach’s principle and Dirac’s large number hypothesis ([Unzicker, 2009, 2008, 2007](#); [Unzicker and Preuss, 2015](#)).⁴⁷

6.2.3 Section summary

In summary, the second class of arguments from Mach’s principle we have seen here involves the derivation of a relation of the form $G\rho\tau^2 \sim 1$. Expressions of this sort are widespread in the literature and have been derived in many contexts. In the context of general relativity, a simple (yet methodologically questionable) derivation involves fitting the Lense-Thirring frame dragging of a rotating universe to inertial forces in a rotating frame, or in a gravito-electromagnetism analogue model it involves fitting gravito-magnetic effects of the rotating universe to the Coriolis force of classical mechanics. Expressions of this form were recognised

⁴⁶In his talk, Bondi discussed both the association between Mach’s principle and the critical density condition, as well as a critique of the universality of special relativity on the basis that “there is at every point of the universe a preferred velocity, namely, that from which the universe appears most isotropic” ([Bondi, 1995](#)), which we discussed already in the previous chapter. Both of these points are found to be core tenets of Unnikrishnan’s proposed paradigm of “cosmic relativity” ([Unnikrishnan, 2022](#)).

⁴⁷Unzicker’s work, including video presentations on Mach’s principle was an early influence on me, and planted the seeds of some of the topics explored in the thesis years before I began focussed research.

at least as far back as Weyl (1924), but also derived widely, including by Schrödinger, Barbour, Wheeler and many others.⁴⁸

In Sciama’s imperfect but physically illuminating ‘toy-model’, the relation comes out as $U = c^2$ which is associated with a zero-energy universe. For Sciama and Raine at least this was meant to uniquely select a critical density universe, however their efforts, along with Gilman and others to transfer Mach’s principle into the conceptual mould of general relativity wound up undermining this hope. Bondi (1995) has proposed that in a broader sense the Machian condition is that the quantity $4\pi G\rho\tau^2 \sim 1$ should be a constant over cosmological time, which is indeed a feature of classical Machian models in general. Bondi points out that among Robertson-Walker model this picks out the critical density, although he also added that experimental evidence did not favour it. These ideas continue to be considered by some researchers although they have not had a substantial impact on mainstream cosmology since the 1990s.

6.3 Response to Weyl

The difficulty with implementing anything like Sciama’s $U = c^2$ condition in relativistic cosmology ultimately comes down to the same problem that Weyl had already identified so many years before. The computability of $U = G \int \frac{\rho}{r} dV$ presupposes a pre-existing space over which we can integrate, i.e. that material particles have well defined spatio-temporal relations which precede the inertio-gravitational influences which they induce on one another. In other words, it necessitates that a concept of space—capable of equipping matter with relations of distance, velocity, relative size, and so on—can be disentangled from the space-time metric and geodesic principle of general relativity. If this is not possible, the “Machian programme” which we have explored throughout this thesis can no longer be considered viable. In order to have relations between spatio-temporally separated points one needs a pre-existing space-time structure that can realise those relations, it is therefore circular to propose that the space-time structure will be determined by those spatio-temporal relations.

Weyl has made this point in writings apart from the previously discussed dialogue; for instance in his *philosophy of mathematics and natural science*,⁴⁹ Weyl writes (Weyl, 1949, p.105):

without a world structure the concept of relative motion of several bodies has, as

⁴⁸See for instance Assis (1989); Treder (1972), or for a very early version of this relation in a slightly different form see Reissner (1915).

⁴⁹Originally published in 1927 as *Philosophie der Mathematik und Naturwissenschaft* Weyl (1927).

the postulate of general relativity shows, no more foundation than the concept of absolute motion of a single body. [...] Thus no solution of the problem is possible as long as in adherence to the tendencies of Huyghens and Mach one disregards the structure of the world.

By the “structure of the world,” Weyl means the same as what he elsewhere calls the “guiding field.” In this way, general relativity did not exactly represent a triumph of the relationalist philosophies of Leibniz, Huygens, Berkeley and Mach, but it more closely aligned with the philosophy of Euler, as Cassirer had also realised very early on ([Giovanelli, 2024](#)). The same point has also been made by [Stein \(1967\)](#):

It is often claimed that the general theory of relativity has demonstrated the correctness of Leibniz’s view [as compared to Newton’s]. This is a drastic oversimplification. It is no more true in the general theory than in Newtonian dynamics that the geometry of spacetime is determined by relations among bodies. If the general theory does in essence conform better to Leibniz’s views than classical mechanics does, this is not because it relegates ‘space’ to the ideal status ascribed to it by Leibniz, but rather because the space — or rather the spacetime structure — that Newton requires to be real, appears in the general theory with attributes that might allow Leibniz to accept it as real. The general theory does not deny the existence of something that corresponds to Newton’s ‘immobile being’; but it denies the rigid immobility of this ‘being,’ and represents it as interacting with the other constituents of physical reality.

Jürgen Ehlers also makes the same point in [Ehlers \(1995, p.466\)](#) “in general relativity, a velocity of one body relative to another, distant one can only be defined by means of the metric and/or the connection field between the bodies, and in so far as these fields are dynamic, relative velocity is no longer a kinematical concept”, citing Weyl’s dialogue and quoting Stein (as above) to support his point. Going back to the analysis of Mach’s principle we gave in [chapter 4](#), we can say that of the two hypotheses Mach offered up as the possible *cause* of inertial motion, general relativity embodies the *space as a medium* hypothesis rather than the one he is famous for according to which the physical properties of space are an “abbreviated reference” to the masses of the universe. In this respect it is important to recall that Mach explicitly said that he has no objection in principle to this sort of view ([Mach, 1893, p.547](#)): “I have no objections to Budde’s conception of space as a sort of medium (compare page 230), although I think that the properties of this medium should be demonstrable physically in

some other manner, and that they should not be assumed *ad hoc*.” This point has also been made by [Brown](#) (2005, p.159-160):

Conflating $g_{\mu\nu}$ with space-time itself, or an essential part of it, has also given rise to the widespread misapprehension that the existence of physical solutions of the vacuum field equations, including the Minkowski metric $\eta_{\mu\nu}$ with its preferred global inertial coordinate systems, is inconsistent with Mach’s principle. Once it is recognized that $g_{\mu\nu}$ is an autonomous field (or fields) in its own right, nothing in Mach’s philosophy implies that its dynamical behaviour, or very existence, must be determined by the presence of (other kinds of) matter.

At this point we should pause and ask ourselves what reason we have left to keep pursuing this tired idea. It has been over 150 years since Mach first advanced the proposal, and yet little genuine progress seems to have been made. Should we not, at some point, retire our efforts and concede that this has been a winding, deeply informative, but ultimately misguided venture? I have two reasons, developed in this thesis, to suggest that we ought nonetheless to continue to pursue Mach’s hypothesis:

1. Our analysis of relativity principles, the foundations of geometry and the methodological presuppositions underlying Mach’s principle in the first part of the thesis pose a serious challenge to GR’s methodology, in particular to the application of Riemannian geometry in physics. The view developed in this first half of the thesis will help us mount an objection to Weyl’s argument.
2. As we will see in the final two chapters, physics potentially has a lot to gain if Mach’s hypothesis is correct: the prospect is of a highly predictive and explanatory cosmological theory which makes sense of the equivalence principle rather than merely postulating it, and (as we will see in the next chapter) potentially provides us with an explanation for the existence of gravity.

In the remainder of this section we will discuss the first reason given above: how does our analysis of the relativity principle and the foundations of geometry allow us to respond to Weyl’s objection.

Over the course of this thesis we have discussed 4 distinct senses of the term “relativity,” and argued that the conflation of these has caused ceaseless misunderstanding.

- In the first place we have the Galilean relativity of classical mechanics, which is a convenient idealisation—motivated by the approximate homogeneity of the broader

environment—applied to certain limited subsystems of the universe which allows the application of certain conservation laws to those systems (see chapter 1).

- Secondly, we have the relativity of “special relativity theory,” associated with the symmetry of systems under the Lorentz group rather than the Galilean group. We argued that this form of relativity is not, as it is normally portrayed, a native property of “spacetime,” but instead, appears to be a result of the wave-nature of matter (see chapter 5).
- Thirdly, we have the general interchangeability of coordinate conventions which finds expression in the ‘general covariance principle’ that gave general relativity its name. On its own this principle is essentially physically vacuous, nonetheless, the application of this principle has in effect led to the physicalisation of the concept of space, i.e. the introduction of the metric as an independent agent in physics, since at each point the spatio-temporal properties of matter are required to be defined with respect to some omnipresent field due to the general covariance requirement.
- The fourth sense of relativity, which, we argue, is opposed to the third in its intentions, does not express the dependence of physical properties on a locally defined field, but rather expresses the dependence of physical properties upon the rest of the universe in its entirety. This is the relativity underlying Leibniz’s universal shift invariance as well as Mach’s principle, and is also discussed more broadly in Julian Barbour’s work. Like general covariance, and unlike Galilean and Lorentzian relativity, this last sense of relativity is given *a priori* (since it does not have to do with *real* motion) and therefore also physically vacuous, however, just like general covariance it will allow us to formulate conjectures concerning the physical dependence of phenomena upon one another.

It is this last sense of relativity—not general covariance—that will be most fruitful for us to investigate if we seek a theory of the universe as a whole, since it expresses the thought that there really *is* a universe, i.e. a coherent notion of the totality of the material world, to which universal laws should apply. Whereas the third sense of relativity asks that spatio-temporal properties be defined with respect to a local field, the fourth asks that they should be defined with respect to the rest of the universe in its entirety. Therefore an attempt to implement Mach’s hypothesis should disregard the third sense of relativity and only consider the fourth.

As we have seen in chapter 2, the consequence of the possibility that the entire material universe can be conceived as a whole, that is, lacking anything external to it, is that it must be

possible to represent it in a Euclidean, i.e. *homogeneous* space. This is because if it is possible to consider the universe in its entirety as a single system it must also be possible to consider a transformation in scale applied to that universe, as well as other transformations such as translations in space. As we have seen, only a homogeneous space will leave the universe invariant under all those transformations. It is this that, we propose, allows us to circumvent Weyl's objection.

In our defence of Delboeuf's conception of Euclidean geometry in chapter 2, we brought out a crucial distinction between two entirely separate, yet often conflated, concepts of space. On one hand, models of physics usually involve a *physical* space. This is a space equipped with some prescription such as the "law of inertia" (or a geodesic principle) which governs the motion of bodies in the absence of forces, but which makes this space *causally* responsible for the so-called 'inertial' motion. On the other hand, another conception of space is possible that exists only for the representation of *shape*, that is, for the representation of spatial relations between points. This second concept is of a *homogeneous* space in the sense that it is invariant under arbitrary *shape-preserving* transformations, but at the same time, it is from this *homogeneity* (which includes invariance under rescalings) that the axioms of Euclidean geometry are derived. In this sense, this space is Euclidean and rigid, and this is what allows us, in principle, to recover the notion of relative motion at a distance.⁵⁰

For Weyl, as for others, the *rigidity* of the Newtonian space of classical mechanics was inextricable from its physical content due to the law of inertia. This is why the realisation of the dynamical origins of inertia inevitably led Weyl and others to reject the possibility of relative motion at a distance. This conclusion, which renders Mach's hypothesis incoherent and at the same time necessarily turns space into a physical reality, was already implicitly contained in the dominant understanding of the foundations of geometry at the end of the 19th century. It was therefore necessary for us to go back to Delboeuf's neglected contribution, in order to defend the philosophical legitimacy of Mach's hypothesis and of the concept of relative motion in general. Equipped with our entirely *passive* concept of space, we could also define "matter" by contrast as anything that participates in *causation*. Our view of matter thus involves a rejection of Weyl's dichotomy between "force" and "guidance" (which is really a continuation of Euler's distinction between external and internal causes discussed in chapter 3). Once this dichotomy is rejected, the unqualified part of Weyl's principle C "*all events are causally determined by matter*" becomes analytic.

⁵⁰Note that there are still some problems that remain unresolved, in particular, it is not yet clear how the retarded action of gravitational influences will be taken into account given that the gravitational field distorts causal structure even if this is represented in a Euclidean space. This issue won't be addressed in this thesis but is left as a question for a potential postdoctoral project to address.

The question of what is causally responsible for determining the inertial motion now once again is left to a choice of *hypotheses*, as Mach had already recognised: either it is determined by some *medium*, or it is somehow due to the action of the *other bodies* of the universe. In both cases however, the cause according to us is material. Since general relativity, as we saw, necessarily makes “space-time” into something physically real and autonomous, it is suitable to interpret it as a realisation of the first sort of hypothesis. The space-time of general relativity is therefore a physical and substantial medium, it is a kind of “matter” and this of course avoids all manner of philosophical problems that arise from the effort made by some to interpret a dynamical entity as non-causal (see [Nerlich \(1994\)](#) for instance). Indeed, the recognition of this physical role of space in his theory led Einstein, in 1920, to return to the concept of the ether.⁵¹ Our difference from Weyl therefore is that we do not see ourselves as forced to accept the first class of hypothesis. The hypothesis of Mach remains a live option for us, and the physical concept of space is only strictly necessary if one is modelling a subsystem of the universe. The question concerning the validity of Mach’s hypothesis then comes down to this: *is the physical space that we use to model subsystems of the universe ultimately not just an abbreviated reference to the relations of that system to the rest of the masses in the universe?*

Summary

Over the course of the last two chapters we have seen how Einstein’s approach to the implementation of Mach’s hypothesis was ultimately self-undermining. On one hand, by interpreting Lorentzian effects as kinematical and thereby precluding the possibility that they may have a material cause, the “special theory of relativity” contradicts the philosophical roots of *Mach’s principle*. On the other hand, although it could be argued that the new *dynamical* space-time of the “general theory of relativity” salvages Mach’s principle (so long as “space-time” is considered as a physical medium which acts causally), this theory severely problematises the basic notion of relative motion at a distance itself, and thereby renders *Mach’s hypothesis* incoherent.

In this chapter we focussed especially on the engagement with Mach’s principle of John Wheeler and Dennis Sciama. Starting in the 1950s, Wheeler became the leading advocate of the idea that Mach’s principle implies that the universe should be spatially *closed*. We saw

⁵¹For instance: “the recognition of the fact that empty space in its physical relation is neither homogeneous nor isotropic compelling us to describe its state by ten functions (the gravitation potentials $g_{\mu\nu}$), has, I think, finally disposed of the view that space is physically empty. But therewith the conception of the ether has again acquired an intelligible content, although this content differs widely from that of the ether of the mechanical undulatory theory of light.” ([Einstein, 1920](#)).

that in addition to this he at various times flirted with the notion of temporal closure as well as closure in both space and time. Wheeler’s ideas here were based on his reading of Einstein who had made an appeal to spatial closure in order to eliminate *boundary conditions* from solutions of his gravitational field equations since he saw these as conflicting with the *Machian* requirement that the metric should be determined entirely by the universe’s matter content. Einstein had found coherence with the basic observation that a non-expanding universe of mean matter density $\rho > 0$ should be spatially closed. Wheeler added to this the claim that closure was required if the sum over the inertial influences of distant matter converges. In both cases, these arguments for a closed universe might better be construed as arguments for the universe’s temporal horizon.

The second kind of argument we explored involves the intriguing relation:

$$G \sum_i \frac{m_i}{r_i} \sim c^2, \quad (6.7)$$

which has been expressed widely in the literature on Mach’s principle. As we saw, [Weyl \(1924\)](#) reached a relation of this form based upon a cursory analysis of [Thirring \(1918\)](#). Wheeler also derives the same approximate relation for his 1953 talk in Tokyo based on a basic consideration of a simple AAD gravito-inertial model.⁵² [Sciama \(1953b,a\)](#) derived a particularly compelling version of this result in his doctoral work which takes the form:

$$U = c^2, \quad (6.8)$$

and is associated with the appealing idea that each particle may have a vanishing net contribution to the total energy of the universe which was independently suggested by [Jordan \(1939\)](#) for reasons unrelated to Mach’s principle. Sciama admitted that he had a “secret hope” that this relation would wind up giving him something like a requirement that the universe is at the critical density, however, over the course of his career he was unable to extract fruitful results from this initial idea, and ultimately abandoned Mach’s principle, much as he had earlier abandoned the Steady State model of cosmology. While certain speculations based upon Sciama’s equation (given above) persist, these have had little impact on mainstream cosmology in the last few decades. Nonetheless, the arguments that we have given in section 6.3 and throughout this thesis suggest that it would be premature to abandon this line of research.

We distinguished two kinds of passive transformations that may have relevance to physics: (1) arbitrary local transformations of coordinates, (2) rigid transformations and scale transformations of models of the universe as a whole. Whereas the first presupposes that physical

⁵²As we will see later on in chapter 8 expressions of this form were derived numerous other times throughout the 20th century, often independently of one another.

properties are defined locally with respect to some field, the second presupposes that physical properties are defined with respect to the rest of the matter in the universe as a whole. We argued that the proper implementation of the Machian programme ought to put aside considerations of general covariance and focus only on the second class of transformations.

Although for the most part, this research programme has not been pursued consciously, since the reasons we have given for the rejection of non-Euclidean geometry as well as general covariance have not been articulated prior to this thesis, progress has nonetheless been made by physicists working in the context of *classical* Machian models of gravity and inertia. These classical models will be our main focus for the rest of this thesis. In the next chapter we take a look specifically at one of the most concrete insights that this research programme has to offer: that the existence of the force of gravity may be *explained* as a necessary consequence of the relativity of inertia. We will explore this idea mainly from the philosophical side and discuss why this kind of result is appealing and emblematic of scientific progress.



NORTH STAR

Gravitation as a warping of space was a credible notion, though it gave not the least hint as to the nature or origin of gravitation; why the presence of matter should affect ‘space’ was left entirely unexplained.

E. A. Milne, 1935

Why does gravity exist?

Introduction

Although there is a great wealth of literature on Mach’s principle from physicists, philosophers and historians throughout the 20th century and more recently, one of the key ideas that the broader research programme has to offer seems to have been consistently neglected. This is what we have called *Reissner’s hypothesis* (hypothesis 4) which, as we will see, consists in the proposal that the relativity of inertia may *explain* why the force of gravity *exists*. In particular, the proposal is that a cosmological theory of the relativity of inertia will *necessarily* give rise to a mass-mass attraction between bodies that is of the correct strength to account for observed gravitational phenomena, without the need to independently postulate a gravitational interaction. If this claim about nature is correct, then formulating a theory that faithfully captures it would mark a significant advance in our understanding of the world.¹

¹For a bit of context, I originally came to this idea in January of 2023 based upon my interpretation of [Sciama \(1953b\)](#), and wrote about it in [Fay \(2024b\)](#). Later on, I found out that the same idea had already been

In this chapter we will discuss the historical origins of Reissner’s hypothesis which grew out of his dialectical engagement with Einstein’s work (section 7.1 and 7.2). Then we will connect the basic idea to the broader framework of thought that we have developed in this thesis in an effort to more fully articulate the meaning, motivation and significance of this idea (section 7.3). In particular, we adopt the cosmological stance that we developed in §1.1. Since we will not treat the mathematical details of Reissner’s theory in this chapter, the reader is welcome to read section 8.2 of the next chapter first.

7.1 Two converging hypotheses

In this section, we will discuss the two foundational ideas that guided Einstein initial investigations of gravitational theory: (1) Mach’s hypothesis concerning the relativity of inertia and (2) Einstein’s equivalence hypothesis concerning the unity of gravity and inertia. We will see how these ideas mutually reinforced one another and pointed towards an appealing possibility, which unfortunately was not realised in Einstein’s work.

7.1.1 Mach’s hypothesis

Although the hypothesis of a connection between relative inertia and gravity is often attributed to Mach, it does not in fact originate with him, as Assis (2014, p.250) has remarked. Rather, Mach was responsible for a more general hypothesis which we defined earlier in this thesis as hypothesis 2: “*The motion of bodies observed as rectilinear and uniform with reference to the fixed stars, referred to in classical mechanics as ‘inert’, might be caused directly by the action of these distant stars upon those bodies.*” This is what we have called “Mach’s hypothesis.” However, Mach was by no means the only person to have suggested something along these lines. We have seen in chapter 3 that already since Berkeley, the notion that the inertial frame should be fixed to the stars rather than to ‘absolute space’ was a recognised proposal, although there was always some ambiguity over whether this was simply an epistemological claim about how astronomers fix the inertial frame in practice, or a proposal of an alternative hypothesis about the cause of inertial motion. As we saw, Mach himself was ambiguous enough that secondary literature about “Mach’s principle” has been divided about which side he sat on. Nonetheless, we argued in chapter 4, against Norton, that Mach most definitely did

expressed by Reissner (1915) over a century before, moreover, Reissner’s model arguably realised this idea in a more direct way than Sciamia’s (we will discuss the difference in chapter 8 especially). I therefore decided to name this idea *Reissner’s hypothesis*; the prospect of its realisation in a physical theory has been the guiding light and primary motivation for the thesis as a whole.

propose that we seek an alternative physical law to account for the material origin of inertia. Some other authors since Mach were more explicit about this however, and of particular interest to us are the brothers Benedict and Immanuel Friedlaender, Wenzel Hofmann and August Föppl, each of which we will discuss briefly.²

Wenzel Hofmann’s contribution to this story was a booklet published in 1904 titled “Critical Examination of the Two Fundamental Concepts of Mechanics: Motion and Inertia” (Hofmann, 1904), which Einstein would later describe as a “witty little book” (Einstein, 1996, p.220). Very little seems to be known about Hofmann beyond the inscription on the cover of the book which identifies him as a “k. k. Professor,” i.e. kaiserlich-königlich (imperial-royal), meaning that he was an official appointment under the imperial–royal administration of the Austro-Hungarian Empire, and that he appears to have been Viennese. Hofmann independently developed the same standpoint as Mach concerning the law of inertia, but went a little further in proposing a modification of classical inertia by replacing the absolute kinetic energy lagrangian $L = \frac{1}{2}mv^2$, which makes reference to absolute space insofar as v describes velocity in an inertial frame, by a expression for the relative kinetic energy between pairs of masses M and m :

$$L = kMmf(r)v^2, \quad (7.1)$$

where v is now the relative velocity between M and m , and $f(r)$ enables account to be taken of the “possible—indeed perhaps even probable—influence of the distance r between the two masses” (Hofmann, 1904, p.30), k is the inertial coupling strength. As we will see later on, the same expression would be adopted by Reissner in his models. Unlike Reissner, Hofmann does not endeavour to go through the complex and rather involved process of mathematically deriving the consequences of his theory. However he does discuss its consequences in words in quite some length.

August Otto Föppl was a German engineer and professor of technical mechanics who made foundational contributions to elasticity, structural mechanics, and graphical statics. He is mainly known for the Föppl–von Kármán equations describing large plate deflections and for his influential 1894 textbook on Maxwell’s electromagnetic theory, which helped disseminate Maxwell’s ideas and according to Holton (1988) may have impacted Einstein’s early work on special relativity. As Norton (1995, p.27) notes, he was the only physicist at this time (before Einstein) to engage with Mach’s question who was of significant standing. His engagement with the issue consists in a paper written in 1904, titled “On absolute and relative motion” (Föppl et al., 1904). Here he attributes the identification of the issue

²For each of these figures, see some further discussions in Julian Barbour’s notes to the partial translations of their texts in Barbour and Pfister (1995), see also further commentary in Renn (2008) who has also provided translations of Föppl et al. (1904) and Friedlaender and Friedlaender (1896).

discussed to [Mach \(1901\)](#) and also cites Poincaré’s *Science and Hypothesis*, as endorsing the relativist viewpoint. Much like Hofmann and Mach, Föppl assumes inertia to be due to mass-interactions and comes to the conclusion that a properly relativised account of inertia will lead to some “velocity-dependent forces”. Föppl emphasises multiple times in the text that the velocity forces “have nothing to do with gravitational forces, [and may] obey an entirely different law with respect to mutual distance than the latter.” However he does note towards the end of the text that “it would be very difficult to disentangle from astronomical observations the portion attributable on the one hand to gravitational forces and on the other to velocity forces.”



Figure 7.2: August Föppl.

Neither Mach, Hofmann nor Föppl therefore explicitly endorsed some kind of unification of relative inertia with gravitation; indeed Föppl was keen to stress that this was not his intention (perhaps such speculation would be seen as damaging to his reputation). The idea of combining these two, which is of particular interest for this thesis, first appeared in print, prior to Hofmann and Föppl’s works, in 1896 in a short book written jointly by Benedict and Immanuel Friedlaender titled “*Absolute oder relative Bewegung?*” ([Friedlaender and Friedlaender, 1896](#)). Indeed, it seems that until Einstein, the Friedlaender brothers are the only ones to explicitly make this connection. In the section written by Immanuel a remark is made concerning the connection of this programme to the unification of gravity and inertia:

However, it seems to me that the correct form of the law of inertia will only then have been found when relative inertia as an effect of masses on each other and gravitation, which is also an effect of masses on each other, have been derived on the basis of a unified law.

In a footnote, Friedlaender adds that the relativity of inertia could perhaps be implemented by applying an analogue of Weber’s law for electromagnetism to gravitation and inertia:³

³Weber’s law for electromagnetism is a relational antecedent to Maxwell’s electrodynamics founded by Wilhelm Eduard Weber, a colleague of Gauss; see [Assis \(1994\)](#) for further context. See also [Assis \(2014, 1995\)](#) for a discussion of the various gravity models which implement an electromagnetic analogy similar to Weber’s law, including for instance: [Schrödinger \(1925\)](#); [Brown \(1955\)](#); [Edwards \(1974\)](#); [Assis \(1989\)](#). The models of Schrödinger and Assis will be discussed in section 8.1.1 of this paper.

For this it would be very desirable to resolve the question of whether Weber's law applies to gravity, as well as the question concerning gravity's speed of propagation.

Although this suggestion has foreshadowed numerous attempts to model gravity and inertia in this way, which we will explore in detail in the next chapter (§8.1), this idea was not developed any further than this by the Friedlaender brothers.⁴

7.1.2 The equivalence hypothesis

Immanuel Friedlaender's idea of a unified law underlying both inertia and gravitation would not have been conceivable were it not for the remarkable fact, observed since Galileo, that all bodies in free-fall regardless of their composition are subjected to the same acceleration in a gravitational field; a property which gravity shares with the inertial or "fictitious" forces of classical mechanics. In 1907, Einstein saw in this idea a vague possibility of the extension of the relativity principle to accelerated reference frames (Einstein, 1907a). According to Einstein, the assumption of "the complete physical equivalence of a gravitational field and a corresponding acceleration of the reference system [...] extends the principle of relativity to the uniformly accelerated translational motion of the reference system." Although it is now well understood that Einstein was wrong on this, and that in fact this idea enables the relativization of the gravity-inertia split and not the relativization of motion (Janssen, 2012), Einstein nonetheless placed great weight in this insight which eventually grew into the various forms of the "equivalence principle," which we will here be referring to as the *equivalence hypothesis* (since it arises as a speculative generalisation of an empirical result and it not deduced on *a priori* grounds).

Hypothesis 3 *The equivalence hypothesis: The observed local equivalence of a gravitational field and an accelerated reference frame suggests that gravity and inertia ought to be derived together as twin aspects of a unified law.*⁵

⁴Immanuel would go on to have a distinguished career as a geologist, founding a volcanology institute in Naples, whereas Benedict would go on to have an interesting career as, among other things, a sexologist who co-founded several homosexual advocacy groups which emphasised the *masculinity* of male-male sexuality, before his death by suicide in 1908 (Blasius and Phelan, 1997).

⁵The present formulation of the equivalence hypothesis is inspired by what Lehmkuhl (2021) calls the "Einstein equivalence principle". See Lehmkuhl (2014, 2021) for more context on Einstein's various formulations of the equivalence principle and their role in motivating the unification of gravity and inertia.



Figure 7.3: Albert Einstein.

In 1912, Einstein began to consider his equivalence hypothesis in conjunction with Mach’s hypothesis (Einstein, 1912). However, again Einstein arrives at the recognition of Mach’s idea from an arguably misleading angle. In the previous year, Einstein had deduced that it is a consequence of his equivalence hypothesis that the speed of light must vary spatially in the presence of large masses (Einstein, 1911); He also mentions that the inertial mass of a body should slightly increase in a gravitational field, in Einstein and Grossmann (1913) this is given as⁶:

$$m = \frac{\mu}{c} \quad (7.2)$$

where $m = m(r)$ is the inertial mass, μ “denotes a constant that is characteristic of the mass point and independent of the gravitational potential”, and $c = c(r)$ is the variable speed of light in a gravitational field, given in terms of an invariant speed of light c_0 :

$$c = c_0 \left(1 + \frac{\Phi}{c^2} \right) \quad (7.3)$$

where $\Phi = \Phi(r) = -\frac{MG}{r}$ is the Newtonian gravitational potential around a central mass M (Einstein, 1911). The result of all this is that in the presence of a gravitational field, the inertial mass m increases, therefore, in 1912 Einstein speculates (Einstein, 1912):

This suggests that the entire inertia of a mass point is an effect of the presence of all other masses, which is based on a kind of interaction with the latter. [...] This is exactly the same point of view that E. Mach advanced in his astute investigations on this subject.

In later publications Einstein continued to uphold the idea that his equivalence hypothesis, along with Mach’s hypothesis, were central to his speculations concerning gravity. Indeed, in a discussion following the lecture version of his 1913 paper, “On the Present State of the Problem of Gravitation,” he is recorded as stating (Einstein, 1996, p.228):

the fact that the identity of the inertial and the gravitational mass has proved correct with such a remarkable accuracy seems to me to be one of the most im-

⁶Notation has been adjusted for consistency with the rest of this dissertation.

portant pointers for the development of the theory. The need to find a more profound explanation for that identity, and besides that also the view concerning the relativity of inertia advanced by Mach, was actually the motive that compelled me to devote myself to the problem of gravitation.

The above passage came as a response to a question from an engineer named Hans Reissner. And as we will see, Reissner's interactions with Einstein's ideas seem to have had a significant impression on him, since shortly after this, he would write a paper endeavouring to prove just this sort of "explanation" for the identity of gravitational and inertial mass.

7.2 On Reissner's hypothesis

Hans Reissner (1874–1967) was a pioneering German aeronautical engineer and amateur physicist, known in part for having designed and built the world's first successful all-metal aircraft, the *Reissner Canard* while holding a professorship at the University of Aachen. To theoretical physicists, he is primarily known today for being the first person to have derived the *Reissner-Nordström metric* for a spherically symmetric, stationary, charged body in Einstein's theory of general relativity (Reissner, 1916). Before this however, Reissner had been attentively following Einstein's reasoning in the course of the development of the general theory, and in 1914 and 1915 he published a pair of little-known papers (Reissner, 1914, 1915) addressing the possibility of the fulfillment of Mach's hypothesis concerning the material origin of inertia.

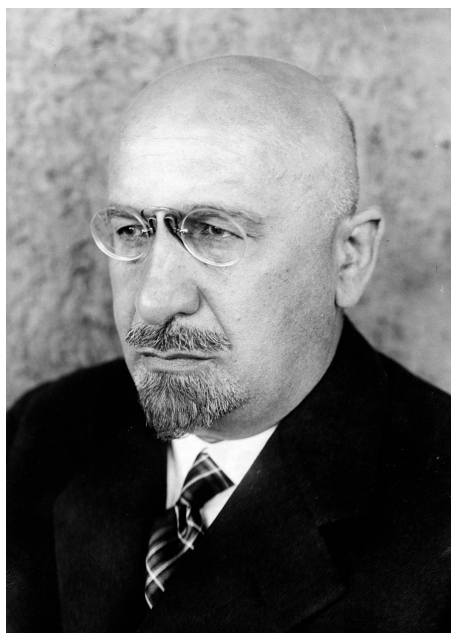


Figure 7.4: Hans Reissner.

Whereas, as we saw in section 7.1.1, Einstein believed he had discovered an extension of the principle of relativity in his equivalence hypothesis. In his first paper, Reissner correctly points out that these are "essentially different requirements" (Reissner, 1914). Concerning the relativity principle he writes:

It is to be required that not only absolute speed but also any absolute motion whatever, in particular acceleration, must be undetectable. This requirement was already formulated by Mach, but it has not yet been carried out.

Reissner's 1914 paper is essentially a mathematical reconstruction of Wenzel Hofmann's theory; indeed Reissner uses the same relative kinetic energy expression as Hofmann (eq.7.1), but notes that since $f(r)$ "must either tend asymptotically to a constant at infinity or simply be a constant", Reissner decides for now to regard it as a constant. Since Reissner did not cite Hofmann's booklet, it is not clear that he had read it, however the fact that the same expression for the relative kinetic energy is given suggests that it is likely that he did. Moreover, Einstein explicitly referred to Hofmann's booklet in the very same 1913 talk on "The present state of the problem of gravitation" (Einstein, 1996, p.220) which Reissner in fact cites in Reissner (1915) (additionally as we saw above he was an active participant in the Q&A):

Let me also make reference to a witty little book by the Viennese mathematician W. Hofmann that independently advances this same point of view. I shall call the conception sketched here "the hypothesis of the relativity of inertia."

Julian Barbour has proposed a more charitable interpretation however, that "Einstein's brief reference to Hofmann gives no indication that Hofmann had given a precise mathematical formulation that went beyond anything that can be found in Mach, so there is no reason to suppose Reissner felt it was necessary to obtain a copy of Hofmann's obscure and not properly cited booklet" (Barbour and Pfister, 1995, p.145).

In his 1915 paper, "*On the Possibility of Deriving Gravity as the Direct Consequence of the Relativity of Inertia*," Reissner breaks with the prevailing tendency, exemplified by Hofmann and Föppl, to treat gravity and inertia as independent phenomena. Instead, he returns to Einstein's equivalence hypothesis, having come to the unexpected realization that it contained hidden fruits. Referring back to his 1914 paper, Reissner writes (Reissner, 1915):

In the essay in question, I stated and for the first time quantitatively formulated the idea that the relativity of acceleration can only be implemented if the centrifugal forces of a rotating body correspond to centripetal forces of all other masses [...] However, my knowledge of the equality of inertial and gravitational masses had not been included as necessary, since my approach involved separate kinetic and potential energy functions. Mr. Einstein's equivalence hypothesis which asserts the mechanical and optical identity of an acceleration field with a field of constant gravity seems to imply the deeper meaning that gravity is also a resistance to acceleration. [...] it is precisely this idea that makes it possible to fulfill Hertz's ideal requirement of representing gravity as an inertial force.⁷

⁷Reissner's reference to "Hertz's ideal requirement of representing gravity as an inertial force" here refers to Heinrich Hertz's posthumously published *Principles of mechanics presented in a new form* (Hertz, 1899), in

In other words, despite having previously dismissed Einstein's equivalence hypothesis as an inappropriate approach to generalizing the relativity principle, to his surprise, Reissner now found that the equivalence hypothesis enables exactly this, though not in the way that Einstein had suggested. The equivalence hypothesis suggests that gravity and inertia should be unified as two aspects of a single law, in such a way that the two laws need not be postulated independently of one another:

If I am successful, gravity would be understood as a direct and necessary consequence of the relativity of acceleration, the identity of the gravitational and inertial masses would be shown to be self-evident and the gravitational field would not only be equivalent to an accelerated space, as Einstein proposes, but gravity itself would be identified as a resistance to relative acceleration.

Reissner's hypothesis hearkens back to Immanuel Friedlaender's comments from 1896, however, by exploring this idea more explicitly, Reissner raises the possibility that a relativized theory of inertia would not only unify inertia with gravity but *explain* gravity as a necessary consequence of relativized inertia.

The mechanism by means of which gravity would arise according to Reissner involves the internal motion of the constituent particles within massive bodies: in the absence of absolute space, the relative momenta of particles are coupled in order to provide a relational basis for the motion deemed by classical mechanics as "inertial". However, Reissner postulates that massive particles do not only move uniformly through space but also vibrate back and forth. Reissner shows that, taking the contribution to the kinetic energy as inversely proportional to distance $f(r) = 1/r$, and provided certain appropriate assumptions (see §8.2.1 for details), the additional relative motion due to vibration induces an inter-particle attraction that can be interpreted as gravitation (see §8.2.1). This particular idea of getting gravity from vibratory motion in a relative-inertia theory would be rediscovered independently by both [Barbour \(1975\)](#) and [Cook \(1976\)](#) six decades later. However, other models (such as [Sciama \(1953b\)](#)) also may be seen to embody Reissner's hypothesis in a weaker sense (see chapter 8 for further discussion).⁸ For this reason, we define *Reissner's hypothesis* independently of the particular mechanism which is being postulated:

which Hertz endorses the unification of all mechanical phenomena under a single law.

⁸Note that for most of these other models that do not involve the mechanism proposed by Reissner, Barbour and Cook, the unification of inertia and gravity is usually portrayed the other way around, i.e. as an explanation of inertia in terms of gravity, rather than the way we favour here, i.e. as an explanation of gravity in terms of relative inertia.

Hypothesis 4 *Reissner's hypothesis:* *Gravity arises entirely as a necessary consequence of the relativity of inertia.*

In the final section of this short chapter, we present a philosophical framework for interpreting Reissner's proposal, bringing it into contact with the broader framework of thinking that we have been developing throughout this thesis.

7.3 Interpretation and explanation

In chapter 1, we discussed how science proceeds through a two-stage method. On the one hand, starting from facts, we seek to generalise more and more to find a small number of general facts which capture the many particular facts initially perceived. On the other hand, we construct ideal systems which transform these general facts into hypotheses or postulates, taken as absolutely valid, which form the starting point of a purely ideal construction. Within this construction, the foundational hypotheses are no longer subjected to empirical scrutiny, instead they become definitions which are *constitutive* of the objects in this system, i.e. these objects are identified and categorised in accordance with the definitions. The problem with this method however, is that the hypotheses should only really be taken as true by definition if they are given entirely *a priori*, that is if they completely lack any empirical content and are justified purely as part of the *method* of science. For if they are motivated empirically, it is not possible to assert that they can be universally true. A self-conscious scientific method ought always therefore to leave room for the possibility that something given to us by experience may also be refuted by experience.

Indeed empirical investigations can only tell us about *contingent* facts: things that may be true here or there, now or then, but not things which are true everywhere and at all times. The reason for this comes from a basic examination of the principle of sufficient reason: if we take the universe to be the totality of things to which we are causally connected, then any fact which is universally true cannot be justified *empirically* since we will not encounter that fact in experience which supplies us only with *changes*, causal actions of the world on our bodies which are inherently *contingent* in nature. Against this, it is often claimed that universal truths such as $1 + 1 = 2$ are confirmed ubiquitously on an everyday basis by experience, for instance if one goes about 'counting apples.' However, what one is confirming here is not that $1 + 1 = 2$ for this statement is true independently of the properties of apples, instead one confirms that individual apples do in fact conform adequately to our notion of a stable unified object whose boundaries are well defined and can be taken to instantiate the number "1" in our conceptual model of the given situation.

All this implies that ideally, we should strip our definitional frameworks in science of any elements deemed to be contingent. Many physicists have felt this instinctively and so they have advocated that ‘fundamental’ physics should not accommodate any dimensionless ratios that are not given as pure numbers, or in other words that are not explicable from purely structural (non-empirical) considerations.⁹ This problem is not only true of dimensionless numbers however, but also applies to other arbitrary elements of a physical theory. In classical mechanics, the concepts of *space*, *time* and *inertia* have a particularly ambiguous status, sometimes they seem to be part of our definitional framework, yet at other times they appear to have actual empirical content.

The central aim of the thesis up to here has been to bring attention to this problem and thereby properly distinguish the empirical from the *a priori* element in these concepts. Any element in our theoretical construction that are given *a posteriori* cannot be regarded as universal, but must be represented as an empirical object which may vary spatio-temporally. As we saw in chapter 2, on one hand Euclidean geometry has an element that is given entirely *a priori* due to the relativity of spatio-temporal quantities including *size* or “magnitudes.” This Euclidean geometry is part of the *method* of science and lacks any empirical content. However, if this geometry is taken as something which actually constrains the shapes of natural solids, such that these actual, real bodies are taken to have a real relation to that space at the particular location of the given bodies, this space is endowed with physical properties.¹⁰ Implicit in the system of classical mechanics, is a hypothesis which involves exactly this, which we will call the classical *scale-inertia* hypothesis. According to this idea, as solid bodies move through geometric space, their *size* is kept constant with respect to this space by a relation *to this space*. In other words, if solid bodies keep roughly the same dimensions with respect to one another it is not because their metrical properties are determined with respect to one another, rather, it is because all bodies have their metrical properties determined by a common relation to space.

If this had been articulated in the 18th century, Euler and others might have said that this scale-inertia is due to causes inherent in the body itself, thus the maintenance of size as it moves about and ages is due to properties of the body itself. Just as with *inertia*, we reject this notion since there is nothing about a body taken in isolation from all surroundings that would tell us about absolute size. In particular, if a body existed in an otherwise empty universe

⁹For instance, consider the work of Eddington, Dirac, Wheeler, Einstein, among others.

¹⁰Note that the hypothesis of Mach is founded on the idea that it may be possible to do away with this physical space in physics altogether such that the physical properties of bodies will be defined with respect to one another instead. This is why, as we saw in chapter 6, Mach’s idea is inherently in tension with the principle of general covariance which implies the necessity of defining a bodies spatiotemporal properties with respect to a locally present “metrical field”.

and we supposed that it linearly increase in size but kept the relations among parts intact, it would not be possible (even in principle) to detect that a change was taking place, therefore, as Leibniz would say, there is no difference at all. The law that a body—in the absence of other forces—maintains size as it ages or moves in space must be rooted in relations that this body has to something *external* to it. And again, in classical mechanics the external entity with respect to which a body maintains size, is *absolute space*, and this space, according to our arguments must therefore—in this system—be regarded as something *physically real*.¹¹ Thus bodies do not without reason increase or decrease in size as they travel through space.

Similarly, with regards to time, although it is perfectly legitimate to postulate a homogeneous universal time for purely methodological purposes (i.e. for coordination), classical mechanics also implicitly involves the hypothesis of what we will call classical *duration-inertia*. Just like with scale, clocks are not relationally determined in classical mechanics but are kept running in step with one another due to their common relationship to absolute time. In addition to these two, classical mechanics also presupposes *inertia*, that (in the absence of other causes) bodies maintain their direction and velocity as they travel through space. Whereas inertia has an *a priori* element insofar as it serves simply to define *force*, the set of reference frames taken as “inertial” are not knowable *a priori* and so cannot legitimately form part of our definitional framework; rather, these should be taken as a property of a real *inertial field* which may be variable in space and time, and cannot, insofar as it is given empirically, be taken as perfectly homogeneous.

In each of these cases, since there is a part that is given *a posteriori*, we should represent it by a field $\phi(\vec{x}, t)$ that may vary in space and time (where ‘space and time’ here are taken in the sense of homogeneous forms used for coordination). The field responsible for scale-inertia may be represented by the scalar variable $\phi_\xi(\vec{x}, t)$ and the field responsible for duration-inertia may be represented by the scalar variable $\phi_\tau(\vec{x}, t)$. The field responsible for inertia (i.e. inertial motion) on the other hand needs to be vectorial since it should include directional information, we can represent it by $\vec{\phi}_v(\vec{x}, t)$.¹²

Still in the context of classical mechanics, we find a similar situation in the case of the law of gravity, in particular, there is no problem *a priori* with postulating an attraction between bodies proportional to $m_i m_j / r_{ij}^2$, however, provided that m_i , m_j and r_{ij} are determined independently (and not defined solely by this relation), the particular magnitude of this attraction will represent an empirical result. It is therefore not justifiable to postulate that the

¹¹for a more elaborate discussion recall §3.2.3, as well as chapter 4 more generally.

¹²In all cases we give the fields as functions of x and t , which are the homogeneous space and time coordinates that allow us to represent spatio-temporal variation, but do not necessarily correspond to measure space and time coordinates (ξ, τ)

magnitude G of this attraction, which is determined empirically, should be universal. We are forced therefore to also represent this value as a quantity which may vary spatio-temporally: $G = G(\vec{x}, t)$. In classical mechanics, in all of these cases ϕ_ξ , ϕ_t , $\vec{\phi}_v$ and G are each taken, by hypothesis, to be universal and unchanging; yet this is not justified, and indeed can not possibly be justified empirically. Can we do better?

There are three ideas now that will allow us to make progress here. First of all, (1) we know that since solid bodies and clocks are *materially constituted*, inertia will not be independent of duration-inertia and scale-inertia.¹³ Second, (2) the *equivalence hypothesis* of Einstein suggests that gravity is intimately connected to inertia, and by extension the *metrical* behaviours of bodies associated with the notions we have introduced of scale-inertia and duration-inertia, which implies that all four of the fields we have introduced should be connected. Thirdly, (3) the other hypothesis that may guide us here is of course Mach's suggestion that the inertial motion of a given massive body might be defined with respect to all the other bodies of the universe. In combination, these three suggest that all of these variables $\phi_\xi(\vec{x}, t)$, $\phi_t(\vec{x}, t)$, $\vec{\phi}_v(\vec{x}, t)$ and $G(\vec{x}, t)$ might be related to the gravitational potential of the universe, or to be more specific, they might be determined by the relations that some point $(\vec{x}, t)$ has to the rest of the mass content of the universe. The directionality of inertia requires that this potential should be at least vectorial, i.e. the fundamental interaction potential is not scalar but either a 3-vector, a 4-vector or some higher rank potential; moreover it may be more appropriate to call it the *inertial potential* rather than the gravitational potential, although this remains a question of taste.

Now Reissner's hypothesis consists in the idea that by providing a relational account of $\vec{\phi}_v(\vec{x}, t)$ we will automatically explain the existence of gravity. This means that our account of relative inertia will at the same time produce an attraction between masses which will imply that $G(\vec{x}, t)$ is non-zero and takes a particular value. Moreover, it will determine the specific values that this field takes without us having to postulate an additional field to account for the observed values of $G(\vec{x}, t)$. This is not the case in classical mechanics, since the values of $G(\vec{x}, t)$ are postulated independently of inertial and metrical properties. The appeal of this idea should be quite obvious, for if the hypothesis is correct, it would allow us to eliminate multiple unexplained fields: $\vec{\phi}_v(\vec{x}, t)$, $G(\vec{x}, t)$, and possibly $\phi_\xi(\vec{x}, t)$ and $\phi_t(\vec{x}, t)$ from our ontology,¹⁴ and explain all of their effects in terms only of the distribution of mass

¹³Indeed if material bodies are represented as systems of point particles and their chrono-metrical properties are outcomes of the dynamical laws of interaction of those point particles, then, provided that the interaction potentials of those particles refer only to relative quantities, it should only be necessary to ensure that inertia is relationally determined, and the relational determination of scale and duration should follow. We will come back to this in §8.3.4.

¹⁴Here the word "ontology" is being used to refer to the list of independent material (i.e. causal) entities that

and momentum throughout the universe which is already known to exist. It would in this way represent the ultimate culmination of a historical process which has been centuries in the making, the final resolution of the central flaws that were inherent in classical mechanics, which prevented this theory from being properly *universal*. That is, the arbitrary elements of the theory that appear *explicitly* in the classical doctrines of *inertia*, *universal gravitation*, and *implicitly* in the unspoken doctrines of *scale-inertia* and *duration-inertia*.

Many of the greatest and most lasting achievements of science consist in *unifications* of classes of phenomena formerly thought to be disconnected from one another. Very often, these unifications are associated with a *reduction* in the number of natural constants that our account of nature needs to postulate. Examples of these include: Boltzmann's law, Wien's law, Ohm's law, among many others, though we will not discuss these here. In Maxwell's electromagnetism, the recognition that light is an electromagnetic wave is captured by the relation $c = \frac{1}{\sqrt{\epsilon_0 \mu_0}}$ between the speed of light c and the permittivity ϵ_0 and permeability μ_0 constants of free space (a numerical coincidence which would have appeared baffling in the absence of a theoretical explanation). With regards to gravitation, important advances were made by Kepler, whose laws of planetary motion place strict constraints on admissible planetary orbits. This laid the groundwork for Newton's revolutionary unification of earthly gravity with planetary motion in his notion of universal gravitation which can be expressed in modern notation by:¹⁵

$$\Phi = G \sum_i \frac{m_i}{r_i}, \quad (7.4)$$

where $\vec{g} = -\vec{\nabla}\Phi$ is the local acceleration due to the masses m_i at distances r_i , G is the gravitational constant and the summation is carried over all the relevant bodies i . In this way, the constant of acceleration $g_{\text{earth}} = 9.81 \text{ m/s}^2$ operative on the surface of earth is *explained* in terms of a more general law. It is not necessary to postulate independent constants of nature g_p for each new planetary surface p which we may encounter, instead a single constant G is postulated as *universal* and all of the *contingency* is placed, not in laws, but in the quantities $m_i(\vec{x}, t)$ and $r_i(\vec{x}, t)$ which may vary spatio-temporally. This was a remarkable and bold induction which might have been proven wrong in countless instances by experiment; its endurance can only be explained by the fact that it captured something essentially *true* about the relations of phenomena in the nature world.

we postulate should exist in order to account for observed phenomena (an arguably unfortunate but common usage in contemporary philosophy of science).

¹⁵Note that in Newton's time the law of gravity was not expressed in such terms, indeed the gravitational constant G was in fact introduced much later, however it was already implicit and deducible from the doctrine that Newton laid out.

Although this does represent a very substantial achievement, our previous analysis shows that we should still remain unsatisfied by the postulation of a gravitational constant G whose universality has not been justified. As we have mentioned a few times already, remarks by [Dirac \(1938\)](#) suggest that $G(\vec{x}, t)$ may vary over cosmological epochs, in particular, it should decrease with the time scale τ . As we will see, theories that in some way embody Reissner's hypothesis eliminate the natural constant G by reducing it to a relation to the sum of all the masses in the universe i :

$$G \sim \frac{c^2}{\sum_i \frac{m_i}{r_i}}, \quad (7.5)$$

which is consonant with an epochal decrease in its magnitude. This is done by unifying gravity with relative inertia in such a way that gravity may be understood as a side effect of the universal relative-inertia interaction, where the strength δ of the inertial coupling is naturally fixed to the masses of the universe as a whole by some basic symmetry considerations. If this is true of nature, i.e. if $G(\vec{x}, t)$ is indeed constrained by this relation and gravity is indeed a consequence of relative inertia, this would represent a genuine and lasting insight into nature that would be immune to subsequent theory change. It is therefore of great importance that we do our best to determine whether or not this is the case.¹⁶

Summary

This chapter introduced and motivated what we have called *Reissner's hypothesis*: the proposal that a sufficiently developed, cosmological account of the relativity of inertia would not merely coexist with gravitation, but would *necessarily* give rise to it. If correct, gravity would not need to be postulated as an independent interaction; rather, it would emerge as a dynamical consequence of a relational inertial law. After explaining how this idea came to guide the thesis as a whole, we traced its historical origins through the convergence of two lines of thought that shaped Einstein's early investigations of gravitation: Mach's hypothesis concerning the material origin of inertia, and Einstein's equivalence hypothesis concerning the unity of inertia and gravitation. While Mach, Hofmann, and Föppl each entertained the possibility of a relational account of inertia, and the Friedlaender brothers explicitly suggested that inertia and gravitation might ultimately be derived from a unified law, it was Hans Reissner who—through his dialectical engagement with Einstein—first articulated the stronger claim that gravity itself might be *explained* as a necessary consequence of the relativity of inertia. By critically engaging with Einstein's work, Reissner argued that the equivalence hypothesis

¹⁶See §8.3.1 for further discussion of how these Machian theories differ from general relativity.

points not to an extension of the relativity principle, but to a reinterpretation of gravity as a resistance to relative acceleration, thereby rendering the equality of inertial and gravitational mass self-evident rather than postulated.

In the final section, Reissner’s hypothesis was interpreted within the broader philosophical framework developed throughout the thesis. We argued that any element of a theory that is introduced on empirical grounds cannot legitimately be treated as universally fixed, and should instead be represented as a field that may vary in space and time. Classical mechanics, on this view, illicitly treats inertia, gravitation, and even the metrical stability of rods and clocks as universal. Guided by Mach’s hypothesis and the equivalence hypothesis, we suggested that inertial motion, metrical behaviour, and gravitation should all be understood as relational effects determined by the distribution of matter in the universe. Reissner’s hypothesis captures the most radical consequence of this programme: that by providing a fully relational account of inertia, one simultaneously explains why gravity exists at all, and why its coupling strength takes a particular value, thereby eliminating an otherwise unexplained universal constant. In this sense, the hypothesis represents a natural continuation of the unificatory pattern by which physics has historically made its most durable advances.

In the next chapter, we will examine various “classical” relative-inertia theories which to greater or lesser extents appear to implement Reissner’s hypothesis. All of the models we discuss will involve some kind of unification of gravity and inertia captured in the relation 7.5. These models take advantage of the flat space-time of classical mechanics which allows for a coherent notion of relative motion at a distance, and eliminate the physical role of this space-time by ensuring that dynamics only depend upon the *relative* situations and motions of bodies. Whereas many of the efforts to implement Mach’s hypothesis in the latter half of the 20th Century (including the Brans-Dicke theory or the SWG approach discussed in chapter 6) try to do so while remaining faithful to post-classical physics, these efforts overlook what we have called Reissner’s hypothesis and thereby obscure what we take to be the most promising theoretical result that this research programme has to offer. If Reissner’s hypothesis is correct, it would be desirable to construct a theory which builds it into its foundation, rather than try in some artificial way to build it into the post-SR, post-GR framework of contemporary physics. As we have seen in chapters 5 and 6, considerations of relativity and of Mach’s principle were central to the motivations behind Einstein’s two “relativity” theories, and yet in spite of their remarkable achievements, these theories did not realise their original intentions. It is desirable that we seek a framework in which these motives might be brought to fruition.



RUSTLING LEAVES

With firm-knit bones on the well-founded,
enduring earth.

Goethe, *Grenzen der Menschheit*

Is zitterbewegung the source of static interactions?

Introduction

In this final chapter we take a closer and more technical look at various ‘classical’ gravinertial models which were proposed over the course of the 20th century. We will discuss the challenges they face, the differences between them and their physical and philosophical implications.¹ In Section 8.1 we discuss models that unify gravity and inertia by analogy with electromagnetism. In such models, the inertial and gravitational interactions are represented by a scalar and a vector potential respectively, which, in the case of Sciama (1953b) are combined into a relativistic four-vector. In general however, the unification of these two is not entirely motivated on theoretical grounds therefore it is not clear that these models properly instantiate Reissner’s hypothesis. In this section we also discuss the historically important issue of the anisotropy of inertia mass, which for over a century has been a point of controversy for these kinds of theories, as well as some of those covered in the subsequent section.

¹Throughout this chapter we will use the term *gravinertial* to refer to theories unifying inertia and gravity under a single interaction. More broadly in the literature the terms “gravito-inertial” and “inertio-gravitational” have also been used for these purposes.

In section 8.2, we will discuss the models of [Reissner \(1915\)](#), [Barbour \(1975\)](#) and [Cook \(1976\)](#) which propose a specific mechanism by means of which gravity would arise due to relative inertia. In these theories, the scalar (gravitational) potential is generated as a result of the rapid oscillatory motion of particles whose masses interact fundamentally only through a velocity-dependent potential motivated by the need to relativise inertia. As a result there is no fundamental scalar gravitational interaction, however these theories rely on the supposition that all particles with “rest-mass” are necessarily in a state of oscillation. [Barbour \(1975\)](#) suggested that a realist interpretation of the *zitterbewegung* of fermions may provide just this kind of oscillation, which takes place at velocity c , and [Cook \(1976\)](#) showed that this leads to a gravitational force of the correct order of magnitude, with an *emergent* analogy with electromagnetism.

We conclude the thesis with a more speculative and open-ended examination of possible directions for future research. Taking as a guiding ideal the criterion proposed by [Barbour \(1975\)](#)—according to which an adequate theory should reveal intimate connections between some of the most universal features of nature, namely gravity, inertia, special relativity, and quantum mechanics—we ask to what extent the models discussed in this chapter realise this ambition. In this spirit, we address four related themes. First, in §8.3.1 we clarify the essential respects in which this class of theories departs from general relativity. Second, in §8.3.2 we show how Sciama’s model points toward an intimate connection with special relativity. Third, in §8.3.3 we examine how Richard Cook’s model suggests a further link to quantum mechanics, and consider whether his result supports the broader conjecture that so-called “static” interactions may, in general, emerge from fundamentally velocity-dependent interaction potentials. Finally, in section 8.3.4, we return to the problem of the relativity of magnitudes—introduced from a methodological perspective in chapter 2 and central to our disagreement with the prevailing consensus on the foundations of geometry. Building on our earlier discussion of what we termed scale-inertia (Section 7.3), we reconsider this notion from the standpoint of the physical ideas under discussion and explore its implications for cosmology.

Most of the models we discuss presuppose instantaneous action-at-a-distance. While this is certainly a weakness of these theories, it is plausible that their results could translate to the retarded action case, however we will not be treating this question in detail in this chapter.

Classical inertia

Before we begin our analysis and comparison of the various gravinertial models, it will be helpful to recall the form of the Lagrangian for a system of particles subject to the law of

inertia and the law of gravity in classical mechanics. It is given by the difference between the kinetic ($T_i = \frac{1}{2}m_i v_i^2$) and potential ($U_i = -\sum_{j \neq i} G \frac{m_i m_j}{r_{ij}}$) energies:

$$L = \sum_i \frac{1}{2} m_i v_i^2 + G \sum_{i < j} \frac{m_i m_j}{r_{ij}}, \quad (8.1)$$

where G is the Newtonian gravitational constant. Whereas the gravitational potential is defined relationally between particles, the kinetic energy term, which is responsible for the inertia of matter in classical mechanics, is defined in terms of the particles's absolute velocities, i.e. their velocity with respect to absolute space. We can also write the classical Lagrangian for a pair of particles as follows:

$$L_{ij} = \frac{1}{2} m_i v_i^2 + \frac{1}{2} m_j v_j^2 + G \frac{m_i m_j}{r_{ij}}, \quad (8.2)$$

Most of the models discussed follow straightforwardly from a postulated Lagrange function, therefore we will focus on comparing the different Lagrangians across models. Concerning the models of Sciama and Cook however, we adopt somewhat different methods since these models were not presented using the Lagrangian formalism.

In various places throughout this section, we have chosen to modify the notation used in the original papers so as to ensure it is standardised across models. In general, we use G to denote the Newtonian gravitational constant and δ to denote a quantity representing the ration of inertial to gravitational coupling strength. Since many of these models feature a variable inertial mass m_i , in general we denotes the invariant masses as μ_i , whereas the inertial masses are given as μ_i . The distance vector between two masses μ_i and μ_j is given by $\mathbf{r}_{ij} = \mathbf{r}_j - \mathbf{r}_i$, whereas $r_{ij} = |\mathbf{r}_j - \mathbf{r}_i|$ is the magnitude of this distance. $\dot{r}_{ij}$ denotes the rate of change in r_{ij} whereas $v_{ij} = |\dot{\mathbf{r}}_i - \dot{\mathbf{r}}_j|$ is the relative velocity of a pair of particles in a given frame of reference.²

²It should briefly be mentioned that we have omitted a discussion of the contribution of the British physicist and philosopher of science Guy Burniston Brown (Brown, 1955, 1982) for the sake of space, although it would have been perfectly appropriate to include it. Brown's theory is very similar to the electromagnetic analogue models of Schrödinger and others. Among other things, as was indicated briefly in chapter 4, I credit Brown for having made the point that "Mach's principle" is actually a hypothesis, some 32 year before I made this the central point of my paper (Fay, 2024a). In fact, I only became aware of Brown's remark two days after my paper was published. Some discussion of Brown's theory, as well as that of Edwards (1974) which I have not yet had time to examine closely, can be found in Assis (1999).

8.1 Electromagnetic analogue models

8.1.1 The Weber potential

The models we examine in this section involve a simple modification of equation 8.2 to ensure that relative velocities only are present. The first to present a mathematical model of this kind was in fact [Reissner \(1914\)](#)—which we will cover in the lead-up to our discussion of [Reissner \(1915\)](#) in section 8.2—however no constraint on the proportion of inertial to gravitational coupling was given. By contrast, as we will see, the models in this section fit the ratio of gravitational to inertial coupling such that it give the correct prediction for the Perihelion shift of mercury, allowing for a derivation of the classic Machian relation $U \sim c^2$. Consequently, the perihelion shift is also interpreted as a specifically *Machian* effect.

Schrödinger’s model Schrödinger’s contribution to the literature on Machian physics models was submitted for publication to *Annalen der Physik* a decade after the publication of Reissner’s papers, in the summer of 1925 (just a few months before he would derive the equation that bears his name). Schrödinger’s paper is in many ways similar to [Reissner \(1914\)](#), indeed, in a note written following its original publication, Schrödinger profusely apologises to Reissner for having unconsciously plagiarised Reissner’s 1914 paper, which he would have read at the time, and calls both of Reissner’s papers “very interesting”.³

In his paper, Schrödinger derives inertia and gravity—like [Reissner \(1914\)](#)—from two independent terms:⁴



Figure 8.2: Erwin Schrödinger.

$$W_{ij} = \delta \frac{\mu_i \mu_j}{r_{ij} c^2} \dot{r}_{ij}^2 - \frac{\mu_i \mu_j}{r_{ij}}, \quad (8.3)$$

³See [Barbour and Pfister \(1995, p.156\)](#) and [Assis \(2014, p.483\)](#) for discussions of Schrödinger’s apology to Reissner. Note also that this is very similar to the assumptions of [Hofmann \(1904\)](#).

⁴In Schrödinger’s original paper the gravitational constant G is left out and Schrödinger uses G to denote the inertial constant which we denote with δ . Like Reissner, Schrödinger also uses μ to denote the invariant masses, however we have replaced there with m . Schrödinger’s Lagrangian is given in his paper as $W_{ij} = \delta \frac{\mu \mu'}{r} \dot{r}^2 - \frac{\mu \mu'}{r}$, however for the sake of clarity and consistency with our discussion of other models we have chosen to write it as equation 8.3.

where δ is an inertial constant to be determined. Although Schrödinger did not mention this fact, his potential function is formally identical to Weber’s potential for electromagnetism, so that Schrödinger’s model can be seen as the first real fulfillment of Immanuel Friedlaender’s comment from 1896 in which it was suggested that the application of Weber’s law to gravity may enable the Machian unification of gravity and inertia.⁵

One might think that an expression for the gravitational constant G may be derived by simply fitting the velocity-dependent term in Schrödinger’s Lagrangian to the classical term for inertia. However it is not so simple, since the dependence on $\dot{r}_{ij}$ rather than v_{ij} of the first term on the right hand side of eq. 8.3 means we cannot simply compare it to the classical Lagrangian (eq. 8.2). It was therefore necessary for Schrödinger to integrate his function in order to calculate G which makes the paper as a whole rather complicated. To summarise the basic results we will focus only on the determination of expressions for δ and G .

On the basis of an analogy with Weberian electromagnetism, it is reasonable to assume that δ is of order 1, and thereby an approximate relation of the form $U \sim c^2$, would be calculable. However, Schrödinger obtains a more precise value for δ by fitting the model to observed results for the perihelion shift of Mercury, which gives $\delta = 3$.

Utilising some basic simplifying assumptions, Schrödinger is able to integrate his function and ultimately gets an expression for the Newtonian gravitational constant which he calls a “remarkable relation”:

$$G = \frac{c^2}{2\Phi_U}, \quad (8.4)$$

where $\Phi_U = \int_0^R 4\pi r \rho dr = 2\pi G R^2 \rho$ such that $U = G\Phi_U$ is the total static gravitational potential of the universe according to Schrödinger’s basic assumptions made for the purposes of integration. Put differently, this implies that $U = c^2/2$; which differs from Sciama’s expression—which we first encountered in chapter 6—by a factor of 2.

Following from this, Schrödinger discusses the implications that this result has for cosmology. Since the gravitational potential of our solar system and indeed our galaxy appears to be vanishingly small in comparison to the total mass needed for the correct prediction of Mercury’s perihelion, it follows that (Schrödinger, 1995):

only an entirely vanishing fraction of the inertial effects observed on the earth and in the planetary system arises from the interaction with the masses of our Milky Way system.

⁵See section 7.1 where we discuss this proposal by Friedlaender and Friedlaender (1896).

This is a remarkable and bold result given that at this time very little was known experimentally about the true size of the universe. In fact, it was only the year before, in 1924, that Hubble demonstrated that there were galaxies other than our own milky way; before this many would have identified the universe with our galaxy. As we remarked in chapter 6, however, it is plausible that part of the motivation for Schrödinger to derive a result such as this in a classical context may stem from his reading of Weyl’s dialogue that was published the previous year (Weyl, 1924), in which Weyl briefly discusses similar consequences based on a cursory examination of the Lense-Thirring effect. Indeed Schrödinger cites Weyl’s dialogue at the start of his paper and was also known to be good friends with Weyl.

Assis’s model In André Assis’s original article “On Mach’s principle” of 1989, Schrödinger’s Weberian model is independently reinvented, 64 years after Schrödinger’s article, however, in Assis’s case the analogy with Weber’s law is explicitly recognised (Assis, 1989). Indeed Assis would go on to advocate in favour of the application of Weber’s law to both gravity and electromagnetism on the basis of its relational character, writing several books on the history of Weberian electromagnetism (Assis, 2021a,b,c,d, 2024).

Assis’s Lagrangian and results are essentially exactly the same as those of Schrödinger since the same Lagrangian is used, although, since Assis is explicitly applying the analogy with electromagnetism, he uses Weber’s original unit conventions.⁶ Just as with Schrödinger’s theory, Assis’s inertial coupling constant $\xi = 6$ is derived by fitting the predicted perihelion advance to that obtained by using general relativity, although the analogy with electromagnetism suggests that it should be ~ 1 .

In addition to rediscovering Schrödinger’s results, Assis also shows that the model produces classical inertial forces. By considering an isotropic universe of homogeneous density ρ in a frame of reference relative to which the universe as a whole has a translational acceleration $\mathbf{a}_U$ and is rotating with an angular velocity $\boldsymbol{\omega}_U(t)$. The total force on a body in this



Figure 8.3: André Assis demonstrating a ‘Newton’s bucket’ type experiment.

⁶In Assis’s paper, the Weber potential is given as $U_{ij} = \frac{k}{r_{ij}} \left(1 - \frac{\xi}{2c^2} \dot{r}_{ij}^2 \right)$, with $k = H_g \mu_i \mu_j$ and $\xi = 6$. With respect to the notation we have chosen to use in this paper, $\xi = 2\delta$ and $H_g = G$

frame is then shown to be accelerating according to the expression ([Assis, 1989, 2014](#)):

$$\mathbf{a}_1 = \mathbf{a}_U - \boldsymbol{\omega}_U \times (\boldsymbol{\omega}_U \times \mathbf{r}_1) + 2\boldsymbol{\omega}_U \times \mathbf{v}_1 + \frac{d\boldsymbol{\omega}_U}{dt} \times \mathbf{r}_1 \quad (8.5)$$

Which are the predicted terms for the linear, centrifugal, Coriolis and Euler accelerations respectively. Thus the Weberian model of Schrödinger and Assis correctly predicts classical inertial forces without reference to absolute space.

8.1.2 Mass anisotropy

The model of Schrödinger and Assis, as well as those of Reissner and Barbour which we will examine later, give rise to a tensorial and anisotropic effective inertia mass m_{ij} . In particular, it was thought that this would imply that the inertial mass of objects should depend on the *direction* in which they are measured. This is essentially due to the dependence of the lagrangian on the derivative of relative positions $\dot{r}_{ij}$ rather than relative velocity $\mathbf{v}_{ij}$.

Accurate experiments carried out in the 1960s by [Hughes et al. \(1960\)](#); [Drever \(1961\)](#) gave a high precision null-result for the anisotropy, which ultimately motivated Julian Barbour to abandon this kind of class of theories. However, in defence of these Machian models, [Dicke \(1961\)](#) has argued that:

because of the universal character of the inertial anisotropy, being present in the same way for all particles (or fields), the spatial anisotropy is unobservable locally.

Interestingly, a similar idea was already proposed in Reissner’s paper of 1915: “the fact that classical mechanics, with mass as a constant scalar quantity, serves so well must be seen as an indication that we find ourselves in a region of space with a sufficiently symmetrical mass distribution—unless it should turn out that the tensorial and variable nature of inertial mass appears to us as scalar and unchanging due to the variable properties of our measuring instruments” ([Reissner, 1915](#)). However at this time Reissner himself had not decided one way or the other and left the question open. For some more clarity on the issue, we quote from [Woodward and Mahood \(1999\)](#) who paraphrase Dicke’s argument as follows:

Briefly, Dicke shows that any initially assumed tensorial mass can be absorbed into the spacetime metric by a conformal transformation if the mass anisotropy is universal. That is, if all types of matter respond the same way to the anisotropy, then

$$m_{ij} = m f_{ij} \quad (8.6)$$

where m_{ij} is the tensor mass, m the scalar mass and f_{ij} a universal dimensionless metric tensor field. Though the tensor field may have anisotropies at extended scales, it is locally Minkowskian in sufficiently small regions of spacetime. It follows that inertial reaction forces are not direction dependent, as is observed. Dicke’s point is that the Hughes-Drever experiments are actually tests of the local isotropy of spacetime (rather than tests of supposed tensorial mass), and the results confirm to exquisite accuracy the assumptions of GRT [the general theory of relativity].

In correspondence, Richard Cook (see §8.2.3 for discussion of Cook’s model) has indicated that he endorses Dicke’s view, whereas Julian Barbour does not recognise Dicke’s argument. In fact, drawing in particular from the experiments of James E. Faller, Barbour considered that the anisotropy problem had experimentally disproven the kinds of Machian theories discussed presently, and subsequently directed his attention towards his collaborative work with Bruno Bertotti ([Barbour and Bertotti, 1982](#)), and later on the development of his theory of *shape dynamics* ([Mercati, 2018](#)), whose main connection to the pair of papers discussed in section 8.2.2 is the continuation of the concept of the *relative configuration space*. Given that there is still disagreement concerning how the issue of mass anisotropy should be interpreted it we will withhold judgement concerning which position ought to be taken and note that this is a potential area of further research. The mass anisotropy problem was also a motivation for Treder’s model which we will move on to in the next subsection.⁷

8.1.3 Treder’s Riemann potential

Another little-known approach, distinct from those of Schrödinger and Assis, is Hans-Jürgen Treder’s “inertia-free mechanics”. Treder was a prominent theoretical physicist in the German Democratic Republic, best known for his contributions to gravitational physics, though in his later years he also gained recognition for his work in the history and philosophy of science. Despite their significance, Treder’s papers remained largely unknown to colleagues outside the Eastern Bloc. By the time of Germany’s reunification, Treder had largely retired, and the continued absence of English translations further limited the dissemination of his ideas.

⁷Notice that there is an analogy here between the null-result of inertial mass anisotropy and the much older null-result of ether drift (and other) experiments which we discussed briefly in chapter 5. In both cases, appeals have been made to the covariance of our measurement apparatus to explain the absence of locally observed effects, and likewise in both cases the experiments seem to have somewhat divided the community.

His most important work on the subject of Mach's principle was his monograph "*Die Relativität der Trägheit*" (Treder, 1972). The essential difference from the theories of Schrödinger and Assis is that Treder uses the *Riemann* potential rather than the Weber potential:

$$V_{ij} = \delta \frac{\mu_i \mu_j}{c^2 r_{ij}} \mathbf{v}_{ij}^2 - \frac{\mu_i \mu_j}{r_{ij}}, \quad (8.7)$$

which is identical to the Weber potential apart from the velocity term $\mathbf{v}_{ij}^2$ which replaces the r_{ij}^2 of Schrödinger and Assis. This potential was, like the Weber potential, originally introduced by Riemann (1876a) in his theory of electromagnetism. The difference is that $\mathbf{v}_{ij} = \mathbf{v}_i - \mathbf{v}_j$ encodes the relative velocity between particles i and j rather than the rate of change of their relative distances $\dot{r}_{ij}$. This choice comes with some advantages as well as drawbacks.



Figure 8.4: Hans-Jürgen Treder.

On the one hand, Treder's potential does not lead to the anisotropy of inertial mass. The inertial mass can be calculated quite straightforwardly from a comparison between equation 8.7 and the classical kinetic energy Lagrangian for a single particle. In particular, if we let $V_i = \sum_j V_{ij}$ and assume that the particles j are not moving amongst themselves (such that they have the same state of motion with respect to i), then $\mathbf{v}_i = \mathbf{v}_{ij}$ for all j so that we get:

$$V_i = \frac{\delta \varphi_i}{c^2} \mu_i \mathbf{v}_i^2 - \mu_i \varphi_i, \quad \varphi_i = \sum_{j \neq i} \frac{\mu_j}{r_{ij}} \quad (8.8)$$

Comparing this to the single particle Lagrangian of classical mechanics, we can see that the inertial masses m_i will be given in terms of the invariant masses μ_i by the relation:

$$m_i = \frac{2\delta G \varphi_i}{c^2} \mu_i. \quad (8.9)$$

Now, it is also possible to calculate an expression for the gravitational constant by splitting the second term $-\mu_i \varphi_i$ into (1) a contribution from distant matter $\varphi_{\text{distant bodies}}$ and (2) a contribution from a single local mass M in the foreground φ_M . The result of the calculation is that we get a gravitational 'constant' (in reality it is variable), given by:

$$G_i = \frac{c^2}{2\delta \varphi_i}, \quad (8.10)$$

where G_i is the strength of the gravitational coupling at the location of particle i . Just like in the models of Schrödinger and Assis, Treder calculates the inertial constant by fitting it to the observed perihelion shift of Mercury, which gives: $\delta = 3/2$.

The major drawback of Treder’s approach is that by using relative velocities rather than the rate of change of relative distances, Treder’s theory is no longer rotation invariant since the Lagrangian loses its invariance under rotations of the reference system. This is because in a rotating frame of reference, the difference in velocities $\mathbf{v}_{ij} = \mathbf{v}_i - \mathbf{v}_j$ is not generally constant, for instance if i is taken to be at the origin and the distance $\mathbf{r}_{ij}$ is perpendicular to the axis of rotation, We will have $\mathbf{v}_i \rightarrow \mathbf{v}_i$ whereas $\mathbf{v}_j \rightarrow \mathbf{v}_j + \boldsymbol{\omega} \times \mathbf{r}_{ij}$, such that the relative velocity transforms as:

$$\mathbf{v}_{ij} \rightarrow \mathbf{v}_{ij} - \boldsymbol{\omega} \times \mathbf{r}_{ij} \quad (8.11)$$

This issue may potentially be resolved by introducing a term of the form $-\boldsymbol{\omega} \times \mathbf{r}_{ij}$ into the Lagrangian, in a manner analogous to Lynden-Bell (1995). Lynden-Bell did not work with the Riemann potential in his papers, however the correct modification of Treder’s Lagrangian has recently been carried out by Braun (2024) who has shown that the modified Riemann potential enables calculation of the correct classical inertial forces (eq. 8.5).

Returning briefly to the issue of mass anisotropy, it is interesting to remark that Treder has also argued, in a paper from 1973, that general relativity itself should also predict mass anisotropy (Treder, 1973) and is therefore experimentally disproven! Of course, if Dicke’s objection to the observability of anisotropies is correct then general relativity is secure, but if so, this should also be the case for the Machian models of Schrödinger, Assis, Reissner and others.

8.1.4 Sciama’s Maxwellian analogy

Whereas all of the models considered so far operate within the framework of classical mechanics, i.e. they involve instantaneous action at a distance between masses, Sciama’s model (Sciama, 1953b) is not entirely classical as it borrows results and concepts from special relativity and Maxwellian electromagnetism, and is presented *prima facie* as a field theory. Despite this, the so called “toy-model,” is not a consistent relativistic theory, which did not matter to Sciama since it was only constructed for illustrative purposes and was meant to precede a complete theory which Sciama thought would have to involve a rank-2 tensor potential like general relativity.

Sciama gives his potentials in terms of integrals of a matter density ρ divided by the distance r from the point we are calculating the potentials at. The integration is implicitly

carried out over a three dimensional hypersurface V corresponding to the past light cone (i.e. the observable universe) of the point in question (although this is not actually specified in the equations given it is mentioned in the text). The potentials provided are:

$$\Phi = - \int_V \frac{\rho}{r} dV, \quad \mathbf{A} = - \int_V \frac{\mathbf{v}\rho}{cr} dV. \quad (8.12)$$

By idealising the universe as perfectly homogeneous with a radius approximated as $c\tau$, Sciama gets:

$$\Phi = -2\pi\rho c^2\tau^2. \quad (8.13)$$

If our test-mass now moves with a small non-relativistic velocity $\mathbf{v}$ with respect to this universe, the vector potential will be given by:

$$\mathbf{A} = \frac{\Phi}{c} \mathbf{v}. \quad (8.14)$$

The analogues of Maxwell's electric and magnetic fields are given by:

$$\mathbf{E} = -\nabla\Phi - \frac{1}{c} \frac{\partial \mathbf{A}}{\partial t}, \quad \mathbf{H} = \nabla \times \mathbf{A}, \quad (8.15)$$

and inertial motion of the particle is then recovered in the condition that $\mathbf{E} = 0$, since this is the motion that a particle would have to exhibit in order to feel no inertio-gravitational force. In the present case of a perfectly homogeneous universe, this condition applies when $\frac{\partial \mathbf{v}}{\partial t} = 0$, i.e. when the test particle is not accelerating with respect to the universe.

1. Inhomogeneous universe: gravity By considering a mass M superposed upon this otherwise homogeneous model universe and at rest relative to it, Sciama finds that the fields are given by:

$$\mathbf{E} = -\frac{M}{r^2} \hat{\mathbf{r}} + \frac{\phi + \Phi}{c^2} \frac{\partial \mathbf{v}}{\partial t} \quad (8.16)$$

where $\phi = M/r$ is the potential generated by the test particle. Setting $\mathbf{E} = 0$ for inertial motion now gives:

$$\frac{\partial \mathbf{v}}{\partial t} = \frac{c^2}{\phi + \Phi} \frac{M}{r^2} \hat{\mathbf{r}} \quad (8.17)$$

Which reduces precisely to Newton's law of gravitation provided that $\frac{-c^2}{\phi + \Phi}$ is just the gravitational constant G . Therefore we will have achieved the proper unification of inertia and gravity provided that:

$$G = -\frac{c^2}{\Phi_U} \quad (8.18)$$

where $\Phi_U = \phi + \Phi = \int_V \frac{\rho}{r} dV$ integrated over the whole universe. We can also let $U = -G\Phi_U$ and express Sciama's result as $U = c^2$, which we have already discussed in chapter 6, where U is the gravitational potential of the entire universe taken at a given point.

2. Uniform rotation So far, Sciama's model has shown that it is possible, in a slightly inhomogeneous, non-rotating universe to interpret Newton's laws of gravity and inertia as arising from the balance between the effects of (1) a local inhomogeneity in the mass density and (2) the resistance to acceleration provided by the rest of the universe. However two shortcomings remain:

1. There so far has not been any justification for introducing the two potentials Φ and $\mathbf{A}$ as a unified entity, therefore Sciama might as well have chosen different coupling constants in front of each potential in which case the derived relation for the gravitational constant $G = -c^2/\Phi_U$ could in principle be modified to pick up any desired constant of proportionality, which would make this equation meaningless.
2. Sciama has not shown that his theory will actually generalise to non-inertial frames of reference.

These questions appear to be at least partially answered when we analyse Sciama's treatment of the centrifugal force in a uniformly rotating frame. Although, as we shall see, the method he uses is *ad hoc*, and directly undermines Mach's principle, it is nonetheless highly suggestive since it indicates that a proper relativistic extension of his model might be able to resolve these issues adequately.

Sciama starts out by giving the scalar and vector potentials (Φ and $\mathbf{A} = (A_x, A_y, A_z)$ respectively) in a non-rotating frame as:

$$A_x = A_y = A_z = 0; \quad \Phi = -1$$

where units are chosen such that $G = c = 1$. Now Sciama argues that in a frame rotating around the z axis with angular velocity ω , the potentials will transform as:

$$\begin{aligned} A_x &= \omega y \\ A_y &= -\omega x \\ A_z &= 0 \\ \Phi &= -(1 + \omega^2 r^2)^{1/2} \end{aligned}$$

where $r^2 = x^2 + y^2$. Whereas the modifications of the $\mathbf{A}$ fields follow naturally from Sciama's definition of this potential as $\mathbf{A} = - \int_V \frac{\mathbf{v}\rho}{cr} dV$, the modification given of the scalar potential

Φ does not. Instead, Sciama explains that this is due to “the transformation properties of four-vectors”, citing a paper by Nathan Rosen ([Rosen, 1947](#)) as justification. While it is true that if $A_\mu = (\Phi, \mathbf{A})$ is taken to be a four-vector in Minkowski space then the given transformations applied to the components of $\mathbf{A}$ imply that Φ must also transform as shown above, there are two issues with using this result: (1) Sciama’s model is only allegedly concerned with the classical limit so there is no justification for the use of results particular to special relativity, and more importantly, (2) the transformation of Φ does not follow from the integral expression given for Φ in equation 8.12, therefore the modification of Φ should be interpreted as an effect of the absolute Minkowski space that has discretely been smuggled in, not of the masses of the universe. This undermines Sciama’s Machian intentions altogether.

Nonetheless, it is conceivable that a relativistic modification of Sciama’s Maxwellian analogy would include gamma factors in the integrals for Φ and $\mathbf{A}$ —for instance by integrating over four-momentum rather than $(\rho, \rho\mathbf{v})$ —and thereby generate an appropriate modification of the scalar potential.⁸

The gravilectric field generated by the rotating universe according to Sciama’s transformed potentials is:

$$\mathbf{E} = \Phi_U \frac{\omega^2 \mathbf{r}/c^2}{(1 + \omega^2 r^2)^{1/2}} + \frac{\Phi_U}{c^2} \mathbf{a} \quad (8.19)$$

where $\mathbf{a}$ is the acceleration of the universe with respect to our frame of reference. Setting Sciama’s condition for inertial motion, i.e. $\mathbf{E} = 0$, we get:

$$\mathbf{a} = -\frac{\omega^2 \mathbf{r}/c^2}{(1 + \omega^2 r^2)^{1/2}} \quad (8.20)$$

which for non-relativistic speeds reduces to the classical centrifugal acceleration $\mathbf{a} = -\omega^2 \mathbf{r}$. In this case the sign is negative since we have actually calculated the acceleration of the universe with respect to our test particle, which will be a centripetal acceleration.

3. Discussion We recall that in the models of Schrödinger, Assis and Treder, the ratio of the inertial to gravitational coupling was postulated to be of order 1 but then fitted precisely to produce the perihelion precession predicted by general relativity. By contrast, in Sciama’s model, although the ratio in coupling strengths between the inertial $\mathbf{A}$ and gravitational Φ potentials is initially introduced only by analogy with Maxwell’s equations, the particular ratio between them is corroborated by the treatment of $A_\mu = (\Phi, \mathbf{A})$ as a relativistic four-vector, and further corroborated by Sciama’s treatment of the centrifugal and Coriolis forces:

⁸We will not be trying to do this here. See [Fay \(2025\)](#) for a recent attempt along these lines.

if Sciama had started out with a different ratio in coupling strengths, he would not have been able to derive Coriolis and centrifugal forces in the correct proportions.

This suggests that a fully relativistic extension of Sciama's theory may fulfil Reissner's hypothesis, that is, it would predict the existence of gravity as a necessary consequence of the relativisation of inertia. Note however that such a theory would have to be conceptually quite different from ordinary relativistic field theories, since the potentials are meant to *generate* the inertial structure of Minkowski spacetime, rather than merely being subject to this spacetime acting as a background.

8.2 Internal motion models

Now that we have covered the electromagnetic analogue models we move on to the other class of 20th century Machian models, which are arguably purer instantiations of Reissner's hypothesis. As we will see, these models do not involve the postulation of two separate potentials, instead, both the *apparently* scalar interaction and the velocity-dependent interaction emerge from a single 3-vector potential for inertia, as we will see shortly.

8.2.1 Reissner's models

Before discussing Reissner's internal motion model (Reissner, 1915), we will briefly cover Reissner's first model (Reissner, 1914) for context.

1. Reissner (1914) In order to carry out the generalisation of the relativity principle, in accordance with Mach's ideas, Reissner replaces the classical kinetic energy term ($T_i = \frac{1}{2}m_i v_i^2$) with one defined entirely in terms of the changes of relative distance between masses:⁹

$$T_i = \sum_{j \neq i} \delta \mu_i \mu_j \dot{r}_{ij}^2 f(r_{ij}). \quad (8.21)$$

Reissner is able to determine that $f(r)$ must tend to a constant asymptotically and so sees no reason (for now) not to regard it as a constant. By applying the condition that Newton's equations of motion should hold with good accuracy for a large number of masses, Reissner derives the following condition on his inertial coupling constant:

$$\delta = \frac{3}{\sum_i \mu_i} \quad (8.22)$$

⁹This is effectively the same kinetic energy that Hofmann used, as we saw in the previous chapter (eq. 7.1).

So that his Lagrangian $L = T + U$, where U is the gravitational potential, becomes:

$$L = \frac{3}{\sum_i \mu_i} \sum_{i < j} \mu_i \mu_j \dot{r}_{ij}^2 + G \sum_{i < j} \frac{\mu_i \mu_j}{r_{ij}}, \quad (8.23)$$

As we can see however, although Reissner has implemented Mach's hypothesis in this first paper, inertia and gravity are treated independently of one another, so that there is no unification of inertia and gravity in this model.

2. Reissner (1915) The main difference in starting point of Reissner's second paper from that of the first is that he eliminates the need to consider an additional gravitational potential function, and instead derives both inertia and gravitation from a single relative kinetic energy function T with a different choice of $f(r)$; referring to his previous paper, [Reissner \(1915\)](#) writes:

at that time I saw no reason not to equate $f(r)$ to a constant and assumed a separate force function for gravitation. Here, however, the function $f(r)$ of the mutual distance should be used in such a way that no additional force function is required for gravitation.

Reissner introduces two postulates for the origin of inertial and gravitational forces respectively:

Postulate 1 *The inertial force of mechanics can be represented as the resistance to translational accelerations of a mass relative to all other masses in space.*

Postulate 2 *Weight, or Newtonian gravitation, can be represented as an inertial force arising from the rotation of mass particles relative to each other.*

To achieve this, Reissner then proposes:

$$T = \frac{1}{2} \sum_{i > j} \frac{\mu_i \mu_j}{r_{ij}} \dot{r}_{ij}^2 \quad (8.24)$$

for the total relative kinetic energy of a system of many particles. Reissner's Lagrangian $L = T$ now only has a single term, so there is no need for either an inertial or gravitational coupling constant, instead, the gravitational coupling is determined by identifying the predicted mass-mass attraction of the model with Newtonian gravity.

Since gravity arises in Reissner's model due to motion internal to a body, he is forced to assume that:

The structure of gravitating matter may be imagined such that, in every volume element, rotating particles are distributed without preference for any particular axis of rotation.

Given this assumption, Reissner works out that his model produces a attraction between masses given by:

$$K_r = \frac{1}{3} \mu_i \mu_j \frac{k_i^2 \omega_i^2 + k_j^2 \omega_j^2}{r_{ij}^2}. \quad (8.25)$$

where k_i is the radius of gyration and ω_i the angular velocity of the body i . Gravity will then have been *explained* as a phenomenon of relative inertia provided that K_r can be identified with the Newtonian gravitational force $G \frac{m_i m_j}{r^2}$ where m_i and m_j are the *inertial* masses. In both of Reissner's models, the inertial masses are anisotropic:¹⁰

$$m_i^* = \sum_{j \neq i} \frac{\mu_i \mu_j}{r_{ij}^3} (\mathbf{r}_{ij} \otimes \mathbf{r}_{ij}), \quad (8.26)$$

however, if the potential function $\sum \frac{\mu}{r}$ is assumed to change gradually enough, the inertial mass can be approximated as:

$$m_i = \frac{1}{3} \phi_i \mu_i, \quad \phi_i = \sum_{j \neq i} \frac{\mu_j}{r_{ij}}. \quad (8.27)$$

In this way, Reissner derives the following expression for the gravitational constant:

$$G = 3 \frac{k_i^2 \omega_i^2 + k_j^2 \omega_j^2}{\phi_i \phi_j}, \quad (8.28)$$

where the term $\phi_i \phi_j$ has units of mass/distance, since the units of m_i is mass, however the invariant quantity μ_i has different units.

8.2.2 Barbour's models

In one of the most striking coincidences in the history of Machian theories, the two models of Reissner would be reinvented exactly 60 years after their initial publication in a pair of papers by Julian Barbour ([Barbour, 1974, 1975](#)) which correspond almost exactly to the two papers by Reissner of 1914 and 1915 respectively, despite Barbour not having come across Reissner's papers at the time.

¹⁰Here the asterisk in m_i^* indicates that the term is tensorial.

One of Barbour’s more significant contributions in his first paper was to introduce the concept of the *relative configuration space* (RCS) of the universe, which—for a universe consisting of a system of N particles in Euclidean space—consists of “all the distinct relative configurations of these N particles”. The generalised concept of the RCS would go on to be an important feature in all of Barbour’s later works and in the field of *shape dynamics* in general.

Much like Reissner, Barbour initially treats gravity and inertia separately by proposing different potentials for each.

In the second paper (Barbour, 1975), Barbour then makes essentially the same move as Reissner had made in his second paper 60 years before: he modifies his inertial potential from the first paper by introducing a $1/r_{ij}$ dependence:

$$\tilde{\Gamma} = \left(\sum_{i>j} \frac{m_i m_j}{r_{ij}} r_{ij}^2 \right)^{1/2} \quad (8.29)$$

and proposes that whereas inertia arises “due to the relative motion of masses as a whole”, gravity arises “due to the additional relative motion generated by internal motion”, which corresponds exactly to the two postulates of Reissner (1915) respectively. Although Barbour’s Lagrangian is the square root of that used by Reissner in his second paper, the resulting theory is equivalent, in the sense that both actions yield the same extremal trajectories.

Barbour obtains his results by considering a “highly idealised model” in two dimensions where matter is modelled as being constituted of rings of mass rotating with a velocity of internal motion given as c . An attractive force between pairs of particles is derived which is identified with Newtonian gravity, giving the expression:

$$G \sim \frac{Rc^2}{M} \quad (8.30)$$

for the gravitational constant. Where R is the radius of the universe and M its total mass. Barbour notes that this expression resembles one of the famous “cosmic coincidences”—that had been brought to attention by Dirac (1938) and was discussed in some detail by Bondi (1961)—provided that the velocity of rotation c is the speed of light. This had not been considered in Reissner’s paper of 1915, in which the velocity of internal rotation was left as $k\omega$.

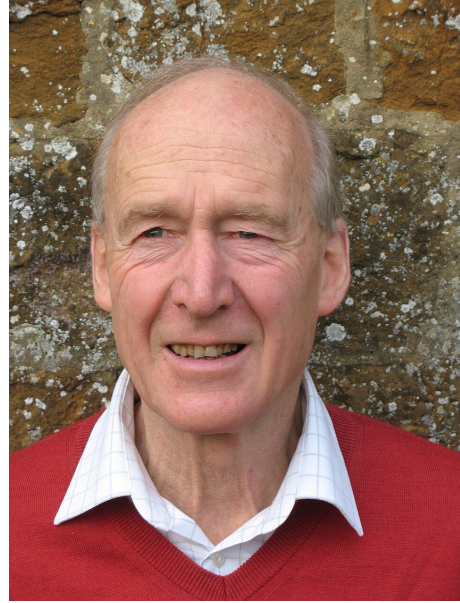


Figure 8.5: Julian Barbour.

The possibility of deriving gravity as a necessary consequence of relative inertia in Barbour’s theory then hinges on this intriguing hypothesis about the nature of matter, that:

the rest-mass energy of the particles is associated with an incessant internal motion at velocity c (all energy then being kinetic in origin) or alternatively with the *zitterbewegung* of elementary particles, which occurs at c .

The ‘*zitterbewegung*’ refers to the rapid oscillatory motion of Dirac fermions, which we discuss in more detail shortly. Barbour’s proposal in turn suggests a link with both quantum mechanics and special relativity, which he acknowledges explicitly:

The ideal would therefore be a theory based on a generalization of (8.29) in which special relativity and quantum mechanics play an integral part, providing a natural framework for the necessary internal motion. In such a theory, the four universals of Nature—gravity, inertia, special relativity and quantum mechanics—would clearly be linked in an intimate and inseparable manner.

What is all the more remarkable about Barbour’s proposal here is that the very next year, an American physicist named Richard Cook would rediscover the same idea entirely independently of Barbour and explicitly develop the suggested connection with quantum mechanics by deriving gravity as an effect of the “*zitterbewegung*” of elementary particles as we will now see.

8.2.3 Cook’s *zitterbewegung*

Richard J. Cook is an American physicist whose most well recognised contribution was to show how quantum jumps in a single trapped atom could be directly observed (Cook and Kimble, 1985), a result that later proved useful for the development of highly accurate atomic clocks. In 1976, Cook was working at a US Air Force Academy in New Mexico when he published a highly interesting paper rediscovering the mechanism of Reissner and Barbour under a slightly different guise (Cook, 1976). Remarkably, although Cook’s paper was published just one year after the publication of Barbour’s 1975 paper, and indeed in the same journal, Cook was not aware until recently of either Barbour or Reissner’s papers. Cook was aware however of Dennis Sciama’s 1969 book *The physical foundations of general relativity* (Sciama, 1969b), in which Sciama leverages his analogy with electromagnetism to derive an expression for the mutual inertial force between two masses m_i and m_j in relative acceleration:

$$\mathbf{F}_{ij} = \frac{Gm_i m_j \mathbf{a}_{ij}}{c^2 r}. \quad (8.31)$$

Since Cook did not know of Sciama's 1953 paper, he did not know of Sciama's inclusion of the scalar potential Φ and never thought it necessary to introduce this potential independently. We can consider this obliviousness on Cook's part as quite fortuitous however, since had he known of Sciama's original paper he would likely not have been prompted to derive gravity without introducing Φ into the theory.

Cook calls eq. 8.32 the Einstein-Sciama force, and generalises it to include contributions for the whole of the universe:

$$\mathbf{F} = \frac{Gm}{c^2} \int \frac{\rho(\mathbf{r})\mathbf{a}(\mathbf{r})}{|\mathbf{r}_m - \mathbf{r}|} d^3r = 1. \quad (8.32)$$

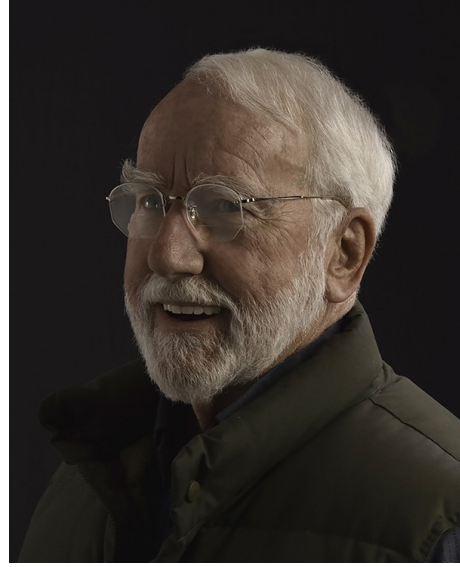


Figure 8.6: Richard J. Cook.

Cook then remarks that this force will be identifiable with the inertial force under linear acceleration of the reference frame provided that the matter distribution is such that:

$$G \int \frac{\rho(\mathbf{r})}{|\mathbf{r}_m - \mathbf{r}|} d^3r = c^2, \quad (8.33)$$

which is just Sciama's result $G\Phi = c^2$. Cook then remarks that this equation is in good agreement with experimental values.

Cook then seeks to derive gravitational effects out of this inertial force law, in a manner essentially analogous to the models of Reissner and Barbour, in particular Cook seeks to show (Cook, 1976):

that the acceleration of particles within matter gives rise to an Einstein-Sciama force outside of matter, that is similar in many respects to the Newtonian force of gravity.

Which is analogous to postulate 2 of Reissner. In order to do this, Cook considers a particle m_1 , located at $\mathbf{r}_1$ and confined within a sphere of radius ϵ around the origin which is vibrating with a velocity v_1 assumed to be random and statistically isotropic". After some manipulation, Cook arrives at the following expression for the time-averaged force $\langle F^i \rangle$ on a particle at some distance $r_2 \gg \epsilon > r_1$ away:

$$\langle F^i \rangle = -\frac{Gm_1m_2r_2^i}{r_2^3} \frac{\langle v_1^2 \rangle}{3c^2} \quad (8.34)$$

where, $\mathbf{r}_2 = (r_2^i, r_2^j, r_2^k)$ such that $r_2^i = |\mathbf{r}_2| = r_2$ when i is taken to be the coordinate in the direction of $\hat{\mathbf{r}}$. The factor of 3 in the denominator comes out because of the isotropy of the particle's average velocity. At this point it is worth noting that the result is entirely analogous to the force calculated by both [Reissner \(1995\)](#) and [Barbour \(1975\)](#), however since Cook has already used Sciama's result that $G = -c^2/\Phi$, it is not as easy to identify this result with Newtonian gravity, since even if we suppose that $\langle v_1^2 \rangle = c^2$, Cook's expression will only correspond to $1/3$ of the force required for Newtonian gravity.

However, Cook then goes on to show that the difficulty is eliminated when quantum mechanics is used to calculate the gravitational force. Although we will not show Cook's calculation here, his method essentially involves replacing the classical numbers a_1^i and $r_1^j a_1^i$ by expectation values of the corresponding Dirac operators of a generic spin- $\frac{1}{2}$ particle ([Cook, 1976](#)). The operator associated with the squared of the velocity is $v^2 = v^i v^i = c^2 \alpha_i \alpha_i$, however the anticommutation relation $\alpha_i \alpha_j + \alpha_j \alpha_i = 2\delta_{ij}$ implies that $\alpha_i \alpha_i = \delta_{ii} = 3$. This gives us the surprising result that $v^2 = 3c^2$, acknowledged by Cook in a remark that "Paradoxical properties of the Dirac velocity operator, such as $v^2 = 3c^2$, are known to be a result of the zitterbewegung or rapid oscillatory motion of the Dirac electron." The result of this is that we recover a gravitational force given by:

$$F^i = -\frac{Gm_1 m_2 r_2^i}{r_2^3}, \quad (8.35)$$

and since "matter may be regarded as composed of neutrons, protons and electrons", Cook now feels justified in "identifying the inertial-gravitational force with the Newtonian force of gravity" ([Cook, 1976](#)).

Apart from being briefly cited by [Barbour and Pfister \(1995, p.142\)](#) in the notes to Barbour's translation of Hans Reissner's papers, Cook's paper has received very little attention, even though Cook appeared to be on his way to fulfilling exactly the condition that Barbour had given for an "ideal" theory the previous year, that "the four universals of Nature—gravity, inertia, special relativity and quantum mechanics—would clearly be linked in an intimate and inseparable manner" ([Barbour, 1975](#)). Barbour's own turn away from these Machian theories at this time was, as we saw, due to his becoming persuaded that they were inconsistent with null-results in measurements of the anisotropy of inertial mass. Cook himself went on to have a career in laser and atomic physics and did not develop the idea further. In correspondence, Cook has mentioned that until now he was not even aware of the models of Reissner and Barbour, and that the only person who had taken his idea seriously in the 1970s was Edwin Jaynes, who is well known for his information-theoretic approach to statistical mechanics.¹¹

¹¹See [Jaynes \(1957a,b\)](#) for Jaynes's papers on this topic, which are extremely widely cited.

8.3 Promising roads ahead

Since the models discussed above were largely developed independently of one another, there has so far not been a coherent research programme bringing all of these ideas together. Julian Barbour, as we have said, turned away from these types of theories due to his concerns about the anisotropy of inertia which were raised to him by Bruno Bertotti. André Assis, on the other hand, has put forwards a coherent research programme proposing a return to *Weber-style* action-at-a-distance for both gravity and electromagnetism. Assis's theory, which is known as *Relational Mechanics* (Assis, 1999), emphasises in particular the models which we examined in section 8.1 but does not recognise the RBC mechanism which we discussed in section 8.2. Additionally, Assis's work does not emphasise what we called *Reissner's hypothesis*, that these theories give us an *explanation* for the existence of gravity, which we have put forwards as the primary *physical* result of the theories discussed.

In this final section of the thesis, we bring together what we take to be the most promising ideas emerging from the preceding analysis. Inevitably, this part of the thesis is more speculative in character than what we have seen so far, and it should be read with that in mind. The considerations advanced here are not intended as definitive conclusions, nor as fully worked-out theoretical proposals, and in some cases they go beyond any suggestions that have already been made in published literature. Rather, they are offered as exploratory suggestions, meant to sketch a possible framework within which the preceding results could be unified and extended, while leaving many details open. As we have seen, after presenting his second relative-distance Machian model in 1975, Barbour articulates a clear statement of what he takes to be the ideal theoretical goal:

The ideal would therefore be a theory based on a generalization of [eq. 8.29] in which special relativity and quantum mechanics play an integral part, providing a natural framework for the necessary internal motion. In such a theory, the four universals of Nature—gravity, inertia, special relativity and quantum mechanics—would clearly be linked in an intimate and inseparable manner.

Although Barbour makes this remark without knowledge of the other models discussed in this chapter, we shall see that several of their results lend independent support to the plausibility of this research programme.

We split the section into four parts: we start out in §8.3.1 with a brief discussion of how the models discussed in this chapter differ essentially from general relativity. In §8.3.2 we discuss the connection of these theories to special relativity. In §8.3.3 we discuss its connection to quantum mechanics, especially in light of Cook's model. Additionally, Given the

analogy between Sciama’s model and Maxwell’s electrodynamics, we explore whether the RBC mechanism may apply not only to gravity, but also as an explanation of the electrostatic interaction in terms of a purely velocity-dependent potential. In this regard, we derive an important null-result and consider its interpretation. Finally, in §8.3.4, we return to the question of chapter 2, that is, of the relativity of scale, and consider this in the light of the models discussed. This naturally leads us back into a discussion of the connection of all of these ideas to cosmology.

8.3.1 Difference from general relativity

As we saw in section 7.1.1, Einstein’s own intention with his theory was to “find a more profound explanation” for the equivalence of gravitational and inertial mass (Einstein, 1996). However, as has been emphasised especially by Sciama (1953b) and Cook (1976), the principle of equivalence is not given an explanation in this theory since, as Cook has put it:

General relativity is certainly not capable of providing a more fundamental understanding of the principle of equivalence, for no theory can explain its postulates, except by argument in a circle.

The same issue is recognised by Sciama (1953b) who insists that in his own model, unlike general relativity, “[t]he principle of equivalence is a consequence of the theory, not an initial axiom.” This issue is related to the fact that Einstein’s theory does not relate the gravitational constant G to the amount of matter in the universe, that as we have seen was a feature of all the models we have discussed in this chapter, which consistently gave an expression of the form $U \sim c^2$ (although in Reissner’s model this was less clear due to the choice of units). An easy way to understand the necessity of this result is to observe that if inertia has its origin in the masses of the universe, then the strength of the inertial coupling, which we will now denote γ , to each of those masses must be inversely proportional to the total amount of mass. Therefore, with regards to inertia the quantity of mass m_i assigned to a given particle i only has meaning in relation to all the other quantities m_j where $j \neq i$, but has no absolute significance, since if we multiply all of our masses k by a common parameter α : $m_k \rightarrow \alpha m_k$, there will be no observable difference as γ will transform in inverse proportion $\gamma \rightarrow \gamma/\alpha$. Now, in the theories we have considered here, whether on account of an analogy with electromagnetism or on account of the RBC mechanism, the inertial coupling γ is posited to be *universally* proportional to the gravitational coupling strength G (although the universality of this proportion is only satisfactorily made *necessary* in models involving the RBC mechanism). This means that a mass-scaling as we have just considered, will leave gravitational

phenomenology similarly invariant, and the proportionality between inertial and gravitational mass will be universally secured. In these theories therefore, the gravitational pull of some particular nearby body i is not an expression of the absolute amount of mass m_i contained in the body, rather it is an expression of how that particular aggregation of mass *stands-out* with respect to the rest of the matter in the universe, i.e. it is an expression of the degree of local inhomogeneity in the distribution of mass in the universe. If the sum over all inertial influences was infinite: $\sum_i \frac{m_i}{r_i} \rightarrow \infty$, then the contribution of each particular mass to the whole would become negligible and gravitational phenomena would also disappear: $G \rightarrow 0$.¹²

All this is not true of general relativity, since there is nothing in this theory that requires that inertia has its origin in the influences of other masses, therefore, although certain gravito-magnetic (Lense-Thirring) effects exist, and indeed involve the same coupling strength G as the standard Newtonian gravitational effects, these effects are purely expression of a body's absolute mass. If we take an FLRW cosmos for instance and apply a mass scaling as considered above: $\rho \rightarrow \alpha\rho$, the dynamics are *not* invariant and we get a modification of the geometry. We can also understand the difference here in the fact that in GR, nothing inherent about the theory fixes the value of G ; instead its value is obtained empirically, and there is no reason given as to why it should not have been different. As we have argued in chapter 7, unexplained quantities such as this should not feature in laws deemed *universal*, since this would violate the *principle of sufficient reason*, however even if one is unconvinced by this argument, it is perfectly fair to ask on what grounds we assume that the value of G measured in the neighbourhood of here and now should apply in all of space and for all time.

8.3.2 Connection to special relativity

The models of Assis, Schrödinger and Treder, both: (1) implement Mach's hypothesis that inertia has its origin in the influence of masses and (2) assume a fixed proportionality between the strength of gravitational and inertial coupling, whose precise value was obtained by supposing that the perihelion shift of mercury is due to a balance of these two influences. However, this is not an *explanation* of the proportion in coupling, it is again (like the value of G in the Einstein Field Equations), an empirical fit. Sciama's model, by contrast, indicates the

¹²We can illustrate this with a thought experiment: In a cosmos governed by such laws, supposing that over time the $\sum_i \frac{m_i}{r_i}$ increases in an unbounded manner as causal influences accumulate from more and more distant matter, we would expect gravity to weaken so that eventually in the very distant future, planets and stars would expand and break apart, no longer held together by gravitational forces.

possibility of an explanation here, and his method in this regard is more similar to Einstein's. In general relativity, the strength of Lense-Thirring type effects is linked to the strength of gravitational effects because the source of both is the stress-energy-momentum tensor $T_{\mu\nu}$. In essence, the proportion in the coupling strengths of these two classes of phenomena is fixed by the formal-mathematical constraints that are placed on general relativity due to the Minkowski signature of 'space-time' and its associated speed-limit c . Likewise, Sciama is operating with similar assumptions by constructing a vectorial theory using Maxwell's equations, for which of course, the ratio in coupling strength of the scalar and vector potentials is fixed by Lorentz-invariance. Interestingly, Sciama was influenced to do this by Hermann Weyl, who had written an insightful paper in 1944 titled: "How far can one get with a linear field theory of gravitation in flat space-time?" (Weyl, 1944) where (among other things)¹³ he showed that, in the words of Sciama (1953a, p.34):

The only linear second-order differential tensor equations for a field of this type [i.e. with a four-vector potential] that imply the conservation of source are Maxwell's equations.

Therefore, the particular ratio in coupling strengths that Sciama chose were related to implicit considerations of Lorentz-invariance, and so his result $U = c^2$ should be understood as rooted in these same considerations. Of course, since there is no such Lorentz invariance constraint in the classical frameworks assumed by Assis, Schrödinger and Treder, there is no corresponding theoretical justification for the particular values of γ that were derived.

As we saw also, Sciama's theory leads to further attractive results. The proportionality between the coupling strengths for Φ and $\mathbf{A}$ is corroborated by Sciama's treatment of the centrifugal force and Coriolis forces. In particular, it is this proportionality which enables Coriolis and centrifugal forces to appear in the correct proportions (recall that the Coriolis force is due to the field $\mathbf{A}$ whereas the centrifugal force is due to a relativistic modification of Φ). Secondly, the model also leads naturally to the attractive consequence that any given particle contributes a net zero energy to the universe since its rest-energy is balanced by its gravitational potential: $mc^2 - m \int_V \frac{\rho}{r} dV = 0$.

¹³Interestingly, towards the end of the paper, Weyl (1944) makes some remarks in connection with Mach's principle and the vast discrepancy in between the strengths of the gravitational and electromagnetic couplings: "one knows that the ratio of the electric and gravitational radii of an electron, $(e^2/m) : \kappa m = \frac{e^2}{\kappa m^2}$, is a pure number of the order of magnitude 10^{40} . This circumstance and Mach's old idea that the plane of the Foucault pendulum is carried around by the stars in their daily revolution, point to a construction in which the gravitational force is bound to the totality of masses in the universe. Our present theory, Maxwell + Einstein, with its inorganic juxtaposition of electromagnetism and gravitation, cannot be the last word." This remark links Mach's principle to Dirac's "large number" coincidences, a connection which, as we have seen, has been made many times throughout the 20th century.

8.3.3 Connection to quantum mechanics: emergent statics?

So far, the results we have seen suggest that there may be a harmonious connection between gravity, relational inertia and special relativity. If we take Kroupa’s view (see chapter 5) that special relativity follows naturally from the “wave-nature of matter” and that, had the sequence of discoveries unfolded differently, special relativity might have been derived as a theorem of quantum mechanics, we are already acknowledging a certain harmony between special relativity and quantum mechanics. However, Cook’s result suggests there may be a deeper connection to quantum mechanics which was wholly unexpected. The connection we discuss here is quite speculative of course since, for one, the associated realist interpretation of quantum zitterbewegung is not generally accepted; nonetheless, this connection was deemed worthy of investigation by both Barbour and Cook, moreover a fairly substantial and well cited literature does take the realistic interpretation of zitterbewegung quite seriously.¹⁴

In all three models—those of Reissner, Barbour, and Cook—gravitational attraction arises from the oscillatory motion of particles coupled through a relative-inertia potential. While this mechanism may appear somewhat obscure in the formulations of Reissner and Barbour, it becomes particularly transparent in Cook’s derivation. Consider two particles p_1 and p_2 . The oscillatory motion of p_1 induces an oscillating inertial force on the nearby particle p_2 . Crucially, this induced force is not symmetric over a full oscillation cycle. When p_1 approaches p_2 , the induced force on p_2 is repulsive, whereas when p_1 recedes from p_2 , the force becomes attractive. However, because the separation between the particles varies during the oscillation, these two phases do not contribute equally. During the repulsive phase, p_1 is, on average, slightly farther from p_2 than during the attractive phase. Since the magnitude of the induced inertial force is inversely proportional to the inter-particle distance, the attractive part of the cycle is therefore weighted more strongly than the repulsive part.

This asymmetry can be visualised by representing the force on p_2 as a weighted sinusoidal function, $F(t) = \alpha(t) \sin(t)$, where the weight $\alpha(t) = \frac{r}{r + \sin(t)}$ encodes the inverse dependence of the force on the instantaneous separation between the particles. The oscillation remains sinusoidal in form, but its amplitude is modulated so that phases corresponding to smaller separation are enhanced relative to those at larger separation. As a result of this amplitude modulation, the time integral of $F(t)$ over one oscillation period does not vanish: the attractive contribution dominates slightly over the repulsive one. Over many cycles this imbalance accumulates, producing a net acceleration of p_2 toward p_1 . In this way, an effective

¹⁴See for instance: Huang (1952); Barut and Bracken (1981); Barut and Zanghi (1984); Salesi and Recami (1994); Rodrigues Jr et al. (1993); Hestenes (1990, 2008); Catillon et al. (2008); Rivas (2008). The original zitterbewegung concept was introduced by Schrödinger (1930) in his discussion of the Dirac electron, however it should be noted that neither Schrödinger or Dirac endorsed a realistic interpretation of this phenomenon.

attractive interaction—formally analogous to gravity—emerges from purely oscillatory dynamics. Interestingly the effect is totally independent of the radius or form of the oscillation but, provided that the motion is statistically isotropic, depends only on its average velocity.

Cook’s starting point, as we have seen, begins with a consideration of what he called the “Einstein-Sciama force” between a pair of mutually accelerating particles which he takes from Sciama (1969b). Since the so-called “Einstein-Sciama” force can be derived from Sciama’s original paper Sciama (1953b), one question that naturally arises is whether Sciama’s model can be modified to eliminate consideration of the scalar potential Φ , i.e. by defining $\mathbf{E} = -\frac{1}{c} \frac{\partial \mathbf{A}}{\partial t}$, and recover both gravity and inertia through a consideration of the zitterbewegung of elementary particles. Since Sciama’s model is formally analogous to electromagnetism (indeed the proportionality between the coupling strengths of Φ and $\mathbf{A}$ —which allowed Sciama to derive $U = c^2$ —is the same as that in Maxwell’s theory), a further question that naturally arises is whether we can also do the same with electromagnetism. Can we derive electrostatic attraction from a theory in which particles only couple with an electrodynamical, velocity dependent potential? We might go even further, and ask: is it possible that there are no static potentials in general, and interactions that seem as such are really due to oscillatory motion in disguise? This might help us avoid the so called ‘self-energy divergence’ problem that a point particle that generates a static field should have infinite self-energy. We can expect a result such as this to disappear if the interaction potential depends upon the relative velocity of particles since a point particle is necessarily stationary with respect to itself.

As we will see, this does not in fact work, provided that we stay within the general framework of Maxwellian electrodynamics it is not possible to produce an emergent net scalar field due to the vibration of a source. We derive this null result below and briefly discuss whether or not this should lead us to abandon the idea of emergent static interactions due to zitterbewegung.

Let us consider the simple case of a modified version of Maxwell’s equations for electromagnetism, in which the electric field $\mathbf{E}$ depends only upon the magnetic vector potential $\mathbf{A}$, but takes no contribution from the scalar potential Φ . In the radiation gauge:

$$\mathbf{E} = -\frac{\partial \mathbf{A}}{\partial t}, \quad \mathbf{A}(\mathbf{r}, t) = \frac{\mu_0}{4\pi} \int \frac{\mathbf{J}(\mathbf{r}', t_r)}{|\mathbf{r} - \mathbf{r}'|} d^3r', \quad (8.36)$$

where $t_r = t - \frac{|\mathbf{r} - \mathbf{r}'|}{c}$ is the retarded time. The current density $\mathbf{J}$ is taken to describe a localized oscillating point charge at $\mathbf{r}_1(t)$:

$$\mathbf{J}(\mathbf{r}', t) = q_1 \dot{\mathbf{r}}_1(t) \delta^3(\mathbf{r}' - \mathbf{r}_1(t)).$$

We now evaluate the electric field at a distant point $\mathbf{r}$, where $r = |\mathbf{r}| \gg |\mathbf{r}_1|$. In this regime, we expand the retarded Green's function in a multipole approximation:

$$\frac{1}{|\mathbf{r} - \mathbf{r}_1|} \approx \frac{1}{r} + \frac{\mathbf{r}_1 \cdot \mathbf{r}}{r^3} = \frac{1}{r} + \frac{r_1^j r^j}{r^3}, \quad (8.37)$$

where repeated indices are summed over. Substituting into the expression for the vector potential:

$$\mathbf{A}(\mathbf{r}, t) \approx \frac{\mu_0 q_1}{4\pi} \left(\frac{\dot{\mathbf{r}}_1(t_r)}{r} + \frac{r_1^j(t_r) r^j}{r^3} \dot{\mathbf{r}}_1(t_r) \right). \quad (8.38)$$

Taking the time derivative yields the electric field:

$$\mathbf{E}(\mathbf{r}, t) \approx -\frac{\mu_0 q_1}{4\pi} \left(\frac{\ddot{\mathbf{r}}_1(t_r)}{r} + \frac{r_1^j(t_r) r^j}{r^3} \ddot{\mathbf{r}}_1(t_r) + \frac{\dot{r}_1^j(t_r) r^j}{r^3} \dot{\mathbf{r}}_1(t_r) \right). \quad (8.39)$$

We now consider the time-averaged electric field $\langle \mathbf{E} \rangle$ at the distant observation point. Suppose the charge q_1 executes bounded oscillatory motion within a small region of radius ϵ centred at the origin. Then its mean acceleration vanishes over time:

$$\langle \ddot{r}_1^i(t) \rangle = 0,$$

so the first term in (8.39) will disappear. The second and third term however each yield a nonzero contribution when averaged. In component form:

$$\langle E^i(\mathbf{r}) \rangle = -\frac{\mu_0 q_1}{4\pi r^3} r^j (\langle r_1^j \ddot{r}_1^i \rangle + \langle \dot{r}_1^j \dot{r}_1^i \rangle). \quad (8.40)$$

However, when we compute the time-average for one of these terms, we find unfortunately that:

$$\langle r_1^j \ddot{r}_1^i \rangle = \lim_{\tau \rightarrow \infty} \frac{1}{\tau} \int_0^\tau r_1^j(t) \ddot{r}_1^i(t) dt \quad (8.41)$$

$$= \lim_{\tau \rightarrow \infty} \left[\frac{r_1^j \dot{r}_1^i}{\tau} \Big|_0^\tau - \frac{1}{\tau} \int_0^\tau \dot{r}_1^j \dot{r}_1^i dt \right] \quad (8.42)$$

$$= -\langle \dot{r}_1^j \dot{r}_1^i \rangle, \quad (8.43)$$

where we assumed that $r_1^j \dot{r}_1^i$ remains bounded for all time, we find unfortunately that it cancels out the effect of the other, so that:

$$\langle E^i(\mathbf{r}) \rangle = -\frac{\mu_0 q_1}{4\pi r^3} r^j (\langle \dot{r}_1^j \dot{r}_1^i \rangle - \langle \dot{r}_1^j \dot{r}_1^i \rangle) = 0. \quad (8.44)$$

Therefore, according to Maxwell’s equations a vibrating charge does not produce any additional net electrostatic-like attraction. Interestingly, the two effects $\langle \dot{r}_1^j \ddot{r}_1^i \rangle$ and $\langle \ddot{r}_1^j \dot{r}_1^i \rangle$ that we found are both of the right order of magnitude to produce exactly the electrostatic field provided that we use Cook’s result that $\langle \dot{r}_1^j \ddot{r}_1^i \rangle = 3c^2$. However, Cook’s calculation for gravitation did not involve both terms. The reason for this is that Cook was using an expression for the Einstein-Sciama force which bypasses the step we took that involved taking the time derivative of the field $\mathbf{A}$. Cook’s model therefore did not take the derivative of the $1/|\mathbf{r} - \mathbf{r}_1|$ dependence, which in our calculation produces a second term that neutralises the first.¹⁵

Now, although we may stop here and conclude that it is a mistake to try to make electrostatic interactions arise in this way, further considerations do naturally come to mind that cause us to reconsider the idea. In Cook’s model, gravitation arises from relative inertia when particles are assumed to always be moving with a relative speed $\dot{r}$ that is effectively c in the direction of their separation vector $\mathbf{r}$. Since this c is the speed limit of ‘relativistic’ physics, and keeping in mind the result that the magnitude of the effect is independent of the radius of vibration of the particle, it stands to reason that any further bulk vibration of our charged particle—which is already moving at the speed limit—ought not to produce any additional pseudo-electrostatic attraction, since if it did, this would imply that the relative motion of the particles along the axis of their distance surpasses the speed of light. The pair of particles is already moving with a relative velocity which is at its maximum. This suggests that an RBC type mechanism might indeed be the *real* cause of electrostatics, and might explain—in terms of a more fundamental microphysical theory—why Maxwell’s equations apply, and indeed why any further vibration causing an oscillation in $\mathbf{A}$ ought not lead to any additional emergent static force. If this is the case, this would not only give us a deeper understanding of electromagnetism, but could also motivate why, in Sciama’s model, the scalar potential Φ should always be posited alongside the inertial interaction potential $\mathbf{A}$, beyond considerations of Lorentz-invariance. This being said, the idea still remains highly speculative.

¹⁵I thank Pooya Farokhi for spotting that two terms that are produced cancel each other out (I had initially failed to carry out the chain rule correctly for eq. 8.39 and had erroneously reproduced Cook’s result). In correspondence, Pooya has also given an intuitive explanation for why such a result indeed ought not to arise in principle: “The best intuition is to think about antennas. There, we have essentially an oscillating dipole (to first order approximation), with a zero net charge that makes the scalar potential part vanish, compatibly with your principle. Now we have oscillating electromagnetic propagation, but no net non-zero field. In fact, we shouldn’t, otherwise, our radios wouldn’t just detect those oscillations, but would move over time with an average non-zero acceleration! Detection of electromagnetic waves (which is the $\vec{A}$ part) is all about putting a circuit that detects oscillating currents, with no net field” (Pooya Farokhi to Jonathan Fay, 6th May 2025).

8.3.4 Scale inertia and heuristics for cosmology

In chapter 2, we discussed how if we are permitted to consider the entire material universe as a single object, then the relativity of magnitudes implies that this universe should be represented as inhabiting a Euclidean space. In chapter 7, we discussed how, in analogy with the problem of the origin of inertia, a similar question may just as well arise which we call the problem of *scale-inertia*; that is, the problem of finding the material cause of the maintenance of the size of bodies in relation to one another. As we will come back to shortly, the problem of scale-inertia is intimately connected to the problem of inertia on account of the dynamical constitution of matter.

In chapter 3, we discussed Euler’s idea that the inertia of bodies is due to “internal causes,” that is causes inherent in the nature of the body itself, and not related to external bodies or external causes. An analogous view can be imagined, according to which the maintenance of a body’s size (in the absence of disturbing forces) is determined by causes inherent within the body and independent of its context. Just as we rejected Euler’s doctrine of internal causes concerning inertia, we can point out in the case of scale-inertia that a body’s size has no meaning in and of itself, in the absence of an external standard of scale. The *principle of sufficient reason* then requires that the cause of a body’s maintenance of size w.r.t. its surroundings is likewise to be found in some relation to this environment, whether this be some all-pervading field, or perhaps some relationship to the masses of the universe.

From here we may speculate, in a theory that disposes with the physical autonomy of the space-time metric, we can articulate a Mach-inspired hypothesis:

Hypothesis 5 *Relational scale-inertia hypothesis:* *The preservation of the size of bodies, observed as constant in proportion other bodies of the universe, might be caused directly by the action of these other bodies’ masses upon the former.*

Moreover, we can consider a further hypothesis inspired by Reissner’s hypothesis, which proposes that the relational determination of scale is mediated through the gravitational field, and indeed that the metrical properties of gravity arise as a side-effect of the necessity of this relational determination of scale.

In this regard, it is useful to consider the following thought experiment: consider all of the particles of the universe, denoted i , each is described as positioned in a Euclidean space with coordinates $(\mathbf{x}_i, t_i)$. Suppose that the distances between all the particles is multiplied by some arbitrary factor α : $\mathbf{x}_i \rightarrow \alpha\mathbf{x}_i$. In the absence of an external determination of scale, it is necessary that all of the length scales associated with the particles, or indeed with the dynamics which bind them in fixed relations should scale in the same proportion. In other

words, a scaling of all lengths should not cause widespread destruction of matter, unlike for instance, the much more diabolical thought experiment of *adding an electron to every atom*.

Now, if we suppose that this effect on length scales is mediated for a particle j via the sum: $\Phi_j \propto \sum_{i \neq j} \frac{\mu_i}{r_{ij}}$, then we should expect Φ_j to transform as: $\Phi_j \rightarrow \alpha^{-1} \Phi_j$, which tells us that locally, length scales should be inversely proportional to Φ_j , and indeed that they should be proportional to the local strength of gravitational coupling G_j , which as we have seen, is also inversely proportional to Φ_j in this class of theories, and decreases over cosmic time. This raises questions of whether cosmological redshift may simply be a result of shrinking length-scales over cosmic history. After all, in view of the notion of space we developed in chapter 2, the idea of the expansion of space is self-contradictory since ‘expansion’ is a spatial concept and therefore implicitly presupposes a passive space concept with respect to which the given thing expands. In light of this, the natural interpretation of ‘expansion’ according to the ideas developed in this thesis is that bodies are shrinking in size over the course of cosmic time. Additionally, if the cause of this expansion is indeed, as we have suggested, the increase in Φ_j , then the stability of the cosmos appears to be a natural consequence. In particular, there would be no reason for the ‘expansion’ to *reverse* or to *accelerate* as is expected in $\Lambda = 0$ FLRW models with $\Omega > 1$ and $\Omega < 1$ respectively. All this is of course mere speculation insofar as we lack a concrete theory.

I will end with some brief remarks that follow from the results we discussed in chapter 5. The universality of Lorentz transformations reveal two useful truths: (1) All length scales are dynamically constituted, i.e. the Euclidean rigid-body is a myth. (2) The universality of the effect means that the different dynamical laws which govern the constitution of bodies do not operate independently. According to the interpretation we gave, this implies the existence of a *universal medium*; this universality is also what enables a kinematical interpretation of Lorentzian effects (along the lines of Janssen (2009)) although we have argued that considerations of the cosmos & Machian considerations render this view problematic. Either way, this universal dependence on a common medium is exactly what enables the possibility of what Reichenbach called “universal forces,” i.e. causal influences that lend themselves to a geometrical interpretation. For reasons discussed throughout this thesis, we reject the geometrical interpretation, however, either way, this universality (of this field/space-time geometry) is what makes it possible for a universal action to be performed on rods and clocks of arbitrary constitution, and thereby enables just the sort of scale-invariance we have spoken of. In the context of both the *special* or *general* relativity theories,¹⁶ scale-invariance is auto-

¹⁶As we have said, both of these theories are in our view poorly named, and we suggest instead that they be renamed the *theory of the universal medium* and the *theory of the dynamical universal medium* respectively.

matically achieved provided that $\eta_{\mu\nu}$ or $g_{\mu\nu}$ are considered as physically real (material) fields whose dimensions also transform accordingly: $g_{ik} \rightarrow \alpha g_{ik}$. If we wish, along the lines of the hypotheses explored in this thesis, to achieve these results in the absence of the physical autonomy of the metrical field, we must suppose that this talk of fields can be rethought as an abbreviated reference to talk of relations between bodies. But if the rigid body, as we have said is a fiction, and its extension is dynamically constituted, we must fall back on the last refuge of the Euclidean rigid body: the point particle, perhaps not as an ontological claim but at least as a methodological necessity.¹⁷ In a world of point particles, scale-invariance ought to be automatically achieved once the dynamics of these particles has been relativised. In this regard, we should remember that elementary fermions are equipped with a natural length scale: the *zitter* radius $r_z = \frac{\hbar}{2mc}$. A realistic interpretation of the *zitterbewegung* is therefore favourable if we would like to understand all length scales as dynamically determined; and why should the particle move at the speed c ? Well, why not?

How did we get here?

To put this in context, it will be helpful to recall how we have got to this point. In chapters 1 and 2 we laid down the philosophical stance that we adopted throughout this thesis, which proposed that it is valuable and legitimate to consider the universe as a whole as a single object, and that this object is distinguished by the absence of any material reality with respect to which it can be referred. In chapters 3 and 4, we discussed how classical mechanics suffers from an illegitimate distinction between *internal* and *external* causes. To overcome this, it is necessary to formulate a theory in which the cause of motion—deemed ‘inertial’ in classical mechanics—is located in a body’s relation to external matter. From this perspective, the hypothesis underlying classical mechanics appears questionable since it implies the existence of a distinguished kind of *matter*—the inertial field—which is taken to be perfectly homogeneous. This is questionable since perfect mathematical symmetry is something that belongs to *thought*, and not empirical reality.

The problem therefore motivates alternative hypotheses about the cause of inertial motion, and the two main classes of these were given by Mach: that this motion is determined by some material *medium* in which bodies are embedded (hypothesis 1), or that it is determined

¹⁷I have always felt some discomfort about ‘ontology’ and ‘metaphysics’; it seems to me that the question of whether some particular thing *exists*, here or there, is a question that can be given a determinate answer, but whether the material world as a whole *exists* is not a question that can be meaningfully asked. This motivates an understanding of metaphysics as a system that seeks to clarify the relationship of our thoughts to reality, rather than an inventory of entities to which existence is attributed.

by some influence of the masses of all the other bodies in the universe (hypothesis 2). Einstein's theory of general relativity was intended to embody this second hypothesis, however, as we saw, it inadvertently embodied the first, although this has been made unclear due to the *formal* (not empirical) departure that this theory makes from the clear-cut space–matter distinction that we have defended in this thesis: in this theory, gravity and dynamical inertia are absorbed into space-time geometry, which has caused much difficulty for its interpretation. We argued that the key methodological error in this regard was already made in special relativity since the kinematical interpretation of this theory obscured its causal structure, making it inappropriate to apply it to the cosmos as a whole.

In the light of this, we determined that it is worthwhile to reconsider theories that embody Mach's preferred hypothesis, and in this context we found, in chapter 7, that in combination with the equivalence of inertial and gravitational mass, the notion that inertial motion is due to interactions between masses suggests that gravity may be *explained* as a side-effect of relative inertia (hypothesis 4), an idea which we attribute to Hans Reissner but was later rediscovered by Julian Barbour (although not phrased in these terms) and Richard Cook. In chapter 8 we saw that although other models, which are associated with André Assis's theory of relational mechanics, also lead to a constraint on the gravitational constant of the form $U \sim c^2$, we argued that the neatest derivation of this expression comes from the models involving the RBC mechanism, in which gravity arises due to *internal motion* of particles. When the internal motion is modelled using a realistic interpretation of zitterbewegung in quantum mechanics, this leads to a proportionality between gravito-magnetic (i.e. Lense–Thirring related) and gravito-electric (i.e. static gravitational) coupling strengths that is equivalent to that in Sciama's theory and by extension to the linearised regime of general relativity. Naturally, this is the same proportionality (c^2) as that between electrostatic and electrodynamic coupling strengths, which suggests the possibility that in this case too the static interaction may arise due to the quantum mechanical internal motion of particles associated with their rest-energy. These various connections, along with the association of the implied expression $U = c^2$ with certain appealing cosmological results—such as the idea that any given mass makes a net zero energy contribution to the universe, and the possible connection to cosmic flatness—point toward the possibility of a harmonious theory that would elegantly bring together the most universal aspects of nature: cosmology, inertia, gravity, electromagnetism, special relativity and quantum mechanics. This deserves renewed attention.

CONCLUSION

Key results

This thesis has investigated the philosophical foundations of the debate over the relativity of motion, with a sustained focus on the boundary between methodological commitment and empirical content in physical theorising. The unifying argument has been that a great deal of confusion in the foundations of modern physics stems from a failure to maintain this boundary clearly, and that drawing it carefully—which requires what I have called the *cosmological stance*—opens a coherent and under-explored research programme with genuine theoretical results. The most significant of these results concerns what I have called *Reissner's hypothesis*: the proposal that gravity arises necessarily out of a relative inertia theory, and so need not be postulated as an independent fundamental interaction.

The cosmological stance, introduced and defended in Chapter 1, treats the universe as a single coherent system about which only analytic, a priori statements can legitimately be made. Empirical statements concern contingent matters of fact and so cannot be universal in scope without violating the principle of sufficient reason. From this distinction follows a research programme: to identify what in our most general principles is genuinely empirical, to recognise such empirical content as contingent and so subject to potential spatio-temporal variation, and to seek deeper explanations for any apparently arbitrary elements that remain in the laws of nature. The cosmological stance was shown to be in tension with the abandonment of a fixed background structure in general relativity, and to motivate the recovery of energy conservation as the regulative image in physics of the law of cause and effect.

In Chapter 2, the recovery of Joseph Delboeuf as a significant figure in nineteenth-century debates on the foundations of geometry led to a defence of Euclidean apriorism at the cosmological level on the basis of the relativity of magnitudes. Bertrand Russell's relative-angles objection to Delboeuf's position—which has continued to be endorsed in the secondary literature—was shown to be fallacious. Delboeuf's argument was defended both in its historical context and in the present day, on the condition established in Chapter 1: that it is

possible to consider the universe as a whole as a single object, and that analytic relativity principles such as the relativity of scale are therefore relevant to the modelling of cosmology.

In Chapters 3 and 4, two components of the classical law of inertia were distinguished: a methodological component constitutive of the framework of classical mechanics, and an empirical component which is a hypothesis about the physical origin of preferred inertial frames and which therefore demands a causal explanation. Mach's critique of absolute space was shown to rest not on a merely empiricist objection but on a principled revision of the concept of causation—causal holism—according to which inertial motion is caused by a homogeneous environment, rather than being a default behaviour in the absence of causes. A further distinction was introduced between *Mach's principle*, a methodological doctrine that affirms relationalism, and *Mach's hypothesis*, the substantive empirical claim that inertia has its origin in mass-mass interactions. These distinctions structure the analysis of the relativity theories in the chapters that follow.

In Chapters 5 and 6, it was argued that both relativity theories, despite having been explicitly motivated by Machian and relational considerations, in fact failed to realise Mach's hypothesis. Special relativity, on its kinematical interpretation, reinstates a class of causally inexplicable default behaviours that Mach's principle aims to eliminate, and illegitimately applies what Wallace calls the *cosmological assumption* to a theory whose proper domain is dynamically isolated subsystems. General relativity in turn rendered spacetime dynamical and undermined the coherent notion of unmediated relative motion at a distance on which Mach's hypothesis depends, leaving Einstein's Machian programme in a somewhat ironic predicament. The Einstein–Wheeler argument for spatial closure was shown to rest on outdated assumptions tied to a non-expanding cosmology, and would have been better construed as an argument for the universe's temporal horizon. Sciama's doctoral derivation of the relation $U = c^2$ was recovered from the Cambridge archives; this relation, which entails that each particle's gravitational potential energy exactly cancels its rest-mass energy, deserves more serious attention than it has received since the decline of the Steady-State model. The historically informed approach of these two chapters suggests that the more productive question is not what spacetime *is* but what the theories *assert* about nature—a question made tractable by the distinction between empirical content and methodological commitment developed in the first half of the thesis.

The central theoretical contribution lies in Chapters 7 and 8, where Reissner's hypothesis was recovered, articulated, and assessed against twentieth-century classical Machian models. The hypothesis was traced to Hans Reissner's dialectical engagement with Einstein in the early years of general relativity, and connected to the broader unificatory ambition that has

motivated the thesis throughout. Among the models examined, the internal-motion models of Reissner, Barbour and Cook were shown to implement Reissner's hypothesis more directly than the electromagnetic-analogue approaches of Schrödinger, Treder, Assis and Sciama: the latter unify gravitational and inertial potentials by formal analogy, but supply no mechanism by which an effective scalar gravitation emerges from velocity-dependent inertial couplings. Cook's derivation of Newtonian gravity from the *zitterbewegung* of Dirac fermions, in which properties of the Dirac velocity operator supply precisely the numerical factors required, opens a conceptual bridge between gravity, inertia, special relativity and quantum mechanics. The unificatory programme that emerges—bringing these together with cosmology in an intimate and inseparable manner—was developed in a more speculative register at the close of the final chapter, and constitutes the most theoretically ambitious result of the thesis.

Future work

Several concrete open questions emerge from the results of this thesis. In connection with Chapter 1, the cosmological stance developed here—including its commitment to the denial of *necessary a posteriori*, or even *universal a posteriori*, propositions—has not yet been compared with the contemporary literature on metaphysics and the philosophy of language, where it is bound to find opponents.

In connection with Chapter 2, the proposal to re-establish the priority of Euclidean geometry will not be received favourably by the philosophy of mathematics literature in its current state. The Delboeufian view recovered here has yet to be brought into engagement with contemporary positions; future work will need to develop and defend the broader philosophy of mathematics that the chapter sketched, and to assess whether the argument from the relativity of magnitudes can be reconciled with the set-theoretic foundations on which modern mathematics rests.

In connection with Chapters 3, 4 and 5, further work is needed to compare the Machian critique of kinematics with broader accounts of causation in the philosophy of physics literature. The proposal drawn from Kroupa and Schmidt that Lorentz invariance reflects the wave-nature of matter rather than the structure of spacetime remains programmatic. A rigorous derivation of Lorentz covariance from a relational, matter-based framework would strengthen this line of argument considerably; it remains to be seen whether physicists will want to take up this view.

In connection with Chapter 6, a more complete account of Mach's principle in general relativity will have to engage with other contributions to this literature, including the Brans–

Dicke theory.

In connection with Chapters 7 and 8, the most pressing task is the extension of the Reissner–Barbour–Cook models to retarded action. All three models presuppose instantaneous action-at-a-distance, which is their most significant physical weakness, and it remains to be shown whether the emergence of effective Newtonian gravity can survive in a retarded formulation. Whether a realist interpretation of fermionic *zitterbewegung* can be embedded within a relational formulation of quantum mechanics is an open question with direct bearing on whether the unificatory ambition of the programme—connecting gravity, inertia, special relativity and quantum mechanics within a single relational framework—can ultimately be met. It also remains to be shown that the metrical effects of gravitation can be recovered in a point-particle framework, and that the model can be extended to cosmology in such a way that the suggested association with the critical density condition is recovered, alongside other features of the cosmos such as cosmological redshift. The work presented in this thesis is a first step in each of these directions.

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