Between Laws and Models: Some Philosophical Morals of Lagrangian Mechanics

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Saturday 28 August 2004: For Philosophy of Physics Handbook's FTP Site

Dear colleagues: This is not a draft of my Handbook article, so much as a preliminary effort for it! This pedagogic exposition of philosophical aspects of Lagrangian mechanics will be followed by a similar exposition for Hamiltonian mechanics; and *then*, I will extract from these pieces an article that is shorter, less pedagogic—and so more appropriate for the Handbook. Meanwhile, comments are welcome, but of course not expected!

Abstract

I extract some philosophical morals from some aspects of Lagrangian mechanics. (A companion paper will present similar morals from Hamiltonian mechanics and Hamilton-Jacobi theory.) One main moral concerns methodology: Lagrangian mechanics provides a level of description of phenomena which has been largely ignored by philosophers, since it falls between their accustomed levels—"laws of nature" and "models". Another main moral concerns ontology: the ontology of Lagrangian mechanics is both more subtle and more problematic than philosophers often realize.

The treatment of Lagrangian mechanics provides an introduction to the subject for philosophers, and is technically elementary. In particular, it is confined to systems with a finite number of degrees of freedom, and for the most part eschews modern geometry.

Newton's fundamental discovery, the one which he considered necessary to keep secret and published only in the form of an anagram, consists of the following: *Data aequatione quotcunque fluentes quantitae involvente fluxiones invenire et vice versa.* In contemporary mathematical language, this means: "It is useful to solve differential equations".

V. Arnold, *Geometrical Methods in the Theory of Ordinary Differential Equations*, Preface

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1 Introduction

1.1 Against the matter-in-motion picture

Lagrangian mechanics is one of the three great schemes of analytical mechanics, which forms a major part of classical mechanics. The other two schemes are Hamiltonian mechanics and Hamilton-Jacobi theory; (sometimes, Hamilton-Jacobi theory is considered a part of Hamiltonian mechanics). This paper is only about Lagrangian mechanics. But its companion paper will give a similar discussion—with similar morals—of Hamiltonian mechanics and Hamilton-Jacobi theory. So I shall begin by celebrating analytical mechanics as a whole, and arguing that all of it, i.e. all three schemes, deserves much more philosophical attention than it currently gets.

Analytical mechanics is one main part, and one of the glories, of classical mechanics. Its development is one of the triumphs, of both mathematics and physics, in the last three hundred years. It runs from the early discoveries of Maupertuis and d'Alembert, through those of Euler, Lagrange, Hamilton and Jacobi, and the application of their theories to continuous systems (as in classical field theory), to the work of Poincaré and his twentieth-century successors in chaos theory, and catastrophe theory. This development reveals the enormous depth and power of just a handful of ideas, such as configuration space, "least action", phase space and the Legendre transformation. Besides, these ideas, and the theories of analytical mechanics that use them, underpin in various ways the twentieth-century theories, quantum theory and relativity, that overthrew classical mechanics. In short: a triumph—and a rich legacy.

But in recent decades, analytical mechanics, indeed all of classical mechanics, has been largely ignored by philosophers of science. They have focussed instead on the interpretative problems of quantum theory and relativity. There are of course good reasons for this: among them, the enormous influence of the new theories on analytic philosophy of science, their undeniably radical innovations (e.g. indeterminacy, dynamical spacetime) and philosophers' understandable desire to address the issues raised by currently accepted physical theories.

But I fear there is also a worse reason: worse because it is false. Namely, philosophers think of classical mechanics as unproblematic. Indeed, there are two errors here, corresponding to what I will call the 'matter-in-motion picture' and the 'particles-in-motion picture'. Here I will discuss the first, leaving the second to Section 2.2.2. According to the matter-in-motion picture, classical mechanics pictures the world as made out of bodies (conceived either as swarms of a vast number of tiny particles separated by void, or as made of continuous space-filling 'stuff'), that move through a vacuum in Euclidean space and interact by forces such as gravity, with their motions determined by a single deterministic law, viz. Newton's second law.

Nowadays this picture is sufficiently part of "common sense" to seem unproblematic. Or at least, it is part of the common sense of the "educated layperson", with memories of high school treatments of falling balls and inclined planes! Certainly a great deal

of work in contemporary analytic metaphysics of science uses this picture of classical mechanics. Writings on such topics as laws of nature and physical properties often appeal to the matter-in-motion picture, as a source of examples or counterexamples for various metaphysical theses. For example, in debates about such theses as whether laws could be 'oaken', or Lewis' doctrine of Humean supervenience, the speculative examples often concern particles, conceived in an essentially classical way, and how they might interact with one another when they collide.[2]

So I agree that classical mechanics suggests the matter-in-motion picture; or to be more specific, the elementary approach to classical mechanics, familiar from high school, suggests it. But a moment's thought shows that the matter-in-motion picture is problematic. For whether we conceive bodies as swarms of tiny particles, or as made of continuous 'stuff', there are troublesome questions about how bodies interact.

For example, if they are swarms of particles, how can two bodies interact? And how can we explain the impenetrability of solids, or the difference between liquids and solids? Indeed, how should we take individual particles to interact? A force between them, across the intervening void ("action-at-a-distance"), seems mysterious: indeed it seemed so, not only to critics of Newton's theory of gravity, but to Newton himself.[3]

On the other hand, if individual particles interact only on contact (as the seventeenth century corpuscularians proposed), we face the same sorts of question as confront the alternative conception of bodies as made of continuous "stuff". For example, how should we conceive the boundary of a continuous body (including as a special case, a tiny particle), in such a way that there can be contact, and so interaction, between two such bodies? And even if we have a satisfactory account of boundaries and contact, there are questions about how to understand bodies' interaction. Considering for simplicity two continuous spheres that collide and touch, either at a point, or if deformed, over a finite region: how exactly does each sphere exert a force on the other so as to impede the other's motion (and limit its own further deformation)? Does each particle (i.e. point-sized bit of matter) somehow exert a force on "nearby" particles, including perhaps those in the other sphere?

These are hard, deep questions about the foundations of classical mechanics. They were pursued and debated, not only by the giants of seventeenth century natural philosophy; but also by their successors, the giants of classical mechanics from 1700 to 1900, including the heroes of analytical mechanics: Euler, Lagrange, Hamilton and Jacobi. But despite the supreme empirical success that classical mechanics had achieved by 1900, these and similar questions remained controversial—and were recognized as scientifically significant. So much so that in 1900 Hilbert proposed the rigorous axiomatization of mechanics (and probability) as the sixth of his famous list of open problems.

[2]Indeed, I think much contemporary philosophy is unduly wedded to the idea that the ontology ("world-picture") of any physical theory must be close to this matter-in-motion picture: consider for example discussions of "naturalism", or of the contrast between causes and reasons. But I shall duck out of trying to substantiate these accusations, since they are irrelevant to my aims.

[3]For discussion and references, cf. e.g. Torretti 1999, p. 78.

But soon afterwards research in the foundations of classical mechanics was overshadowed by the quantum and relativity revolutions. It was only after 1950 that it flourished again, pursued as much by engineers and mathematicians, as by physicists. (It formed part of a multi-faceted renaissance in classical mechanics; other aspects included celestial mechanics, much stimulated by spaceflight, and "chaos theory", stimulated by numerical analysis on computers.) And it remains a very active research area.

But not for philosophers! That is: most philosophers will recognize my list of questions as ones with which the seventeenth century natural philosophers struggled. But with the one exception of the mystery of action-at-a-distance, our philosophical culture tends to ignore the questions, and (even more so) the fact that they are still a focus of scientific research. There are at least two reasons, both of them obvious and understandable, for this.

First (as I said above), the quantum and relativity revolutions have led philosophers of science to concentrate on those theories' interpretative problems.

Secondly, there is a humdrum pedagogical reason, relating to the educational curriculum's inevitable limitations. In the elementary mechanics that most of us learn in high school, these questions are in effect suppressed. The problems discussed are selected so that they can be solved successfully, while ignoring the microscopic constitution of bodies, and the details of contact between them. For example, the bodies are often assumed to be small and rigid enough to be treated as point-particles, as in elementary treatments of planetary motion; (where, as I conceded, the main mystery one faces is the one acknowledged by philosophers—action-at-a-distance). And for some simple problems about extended bodies, like a block sliding down a plane, one can manage by adopting a broadly "instrumentalist" approach, in which for example, one just assumes that each body is rigid (whatever its microscopic constitution), contact is unproblematic, and all the forces on a body (including friction exerted at a boundary) act on the body's centre of mass. One then determines the motion of each body by determining the vector sum of all forces on it. (Pedagogy apart, I shall return to the limitations of this approach to mechanics at the start of Section 3.) In short, one never faces the questions above.

Of course, philosophers often augment high school mechanics with some seventeenth-century mechanics, through studying natural philosophers such as Descartes, Hobbes and Leibniz. But again curricular limitations impinge. Most philosophers' acquaintance with mechanics ends there. For around 1700, natural philosophy divided into physics and philosophy, so that few philosophers know about how mechanics developed after 1700, and how it addressed these foundational questions. In particular, as regards the eighteenth century: philosophers read Berkeley, Hume and Kant, but not such figures as Euler and Lagrange, whose monumental achievements in developing analytical mechanics, and in addressing such questions, changed the subject out of all recognition: a transformation continued in the nineteenth century by figures like Hamilton and Jacobi.

In addition to these two reasons, there may well be others. For example, we now

know that quantum theory underpins the empirical success of classical mechanics in the macroscopic world. (Somehow! There are many open questions, both philosophical and technical, about the details: the quantum measurement problem, and the physics of decoherence, are active research areas.) From this, some philosophers, especially those with more instrumentalist inclinations, will conclude that we need not worry too much about foundational questions about classical mechanics.

These reasons are obvious and understandable. But their effect—the belief that the matter-in-motion picture is unproblematic (apart perhaps from action-at-a-distance)—is unfortunate. It is not just that good foundational questions about classical mechanics get ignored. There is also a loss to our understanding of modern philosophy of science's origins. For it was not only the quantum and relativity theories that had an influence on philosophy of science; (as I mentioned). Before they arose, the lively debate over these foundational questions also strongly influenced philosophy of science. For example, Duhem's instrumentalism was largely a response to the intractability of these questions.

To be sure, today's specialist philosophers of physics know perfectly well that the matter-in-motion picture is problematic. Even in what seem the most unproblematic cases, there can be both a wealth of intricate mathematical structure, and plenty of philosophical issues to pursue. The familiar case of Newtonian point-particles interacting only by gravity affords good examples. Most philosophers would say that surely the mathematics and physics of this case is completely understood, and the only problematic aspect is gravity's acting at a distance. But the mathematics and physics *is* subtle.

For example, Painlevé conjectured in 1898 that there are collision-free singularities: that is, roughly speaking, that a system of Newtonian point-particles interacting only by gravity could all accelerate to spatial infinity within a finite time-interval (the energy being supplied by their infinite gravitational potential wells), so that a solution to the equations of motion did not exist thereafter. Eventually (in 1992) Xia proved that five particles could indeed do this.[4]

As to philosophy, relationist criticisms of the absolute Euclidean space postulated by Newton, and endorsed by the matter-in-motion picture, continue to be a live issue (Belot 2000). And instead of the usual definition of determinism in terms of instantaneous states across all of space determining the future, alternative definitions in terms of the states on open spacetime regions have been proposed and investigated (Schmidt 1997, 1998).

So much by way of exposing the errors of the matter-in-motion picture; (for more discussion, cf. my (2004: Section 2, 2004a)). I now turn to the specific theme of this paper and its companion: the philosophical morals of analytical mechanics.

[4]For more details, cf. e.g. Diacu and Holmes' splendid history (1996; Chapter 3).

1.2 Prospectus

I will draw four such philosophical morals: (the morals will be the same in this paper and its companion). Two concern methodology, and two concern ontology; and in each pair, there will be a main moral, and a minor one. No doubt there are other morals: after all, analytical mechanics is a vast subject, inviting philosophical exploration. I emphasize these four partly because they crop up throughout analytical mechanics; and partly because they all (especially the two main morals) arise from a common idea.

Namely: by considering the set of all possible states of the systems one is concerned with, and making appropriate mathematical constructions on it, one can formulate a general scheme for representing these systems by a characteristic family of differential equations—hence the quotation from Arnold which forms this paper's motto. As we shall see, such a scheme has several merits which greatly extend the class of problems one can solve; and even when one cannot solve the problem, the scheme often secures significant information about it. Though the details of the schemes of course vary between Lagrangian, Hamiltonian and Hamilton-Jacobi mechanics, they have several merits, and morals, in common.[5]

These schemes lie at a level of generality between two others on which philosophers have concentrated: the very general level of "laws of nature" or "the laws" of classical mechanics; and the level of a "model" (which in some philosophers' usage is so specific as to be tied to a single physical problem or phenomenon). Hence my title.

Lagrangian mechanics is a large subject, and I cannot fully expound even its more elementary parts. On the other hand, it is largely unfamiliar to philosophers, to whom I want to advertise the importance of its details. So I must compromise: I shall describe some central ideas—with a minimum of formalism, but with enough detail to bring out my morals. But in a few places (including some whole Subsections!), I indulge in expounding details that are not used later on, and so can be skipped: I will announce those indulgences with a *Warning* in italics.

But fortunately, my morals will be in many ways straightforward, and I will only need elementary pieces of formalism to illustrate them. There will be nothing arcane or recherché here.[6] (I suppose the reason these straightforward morals have apparently

[5] Of course, the ideas of a space of states (state-space), and general schemes for representing and solving problems, also occur in other physical theories, quantum as well as classical. I believe similar morals can often be drawn there. One salient example is catastrophe theory, a framework that grew out of analytical mechanics, but is in various ways much more general; I discuss its morals in Butterfield (2004b).

[6] Many fine books contain the pieces of formalism I cite (and vastly more!). I will mainly follow and refer to just two readable sources: Goldstein et al. (2002), a new edition of a well-known text, with the merit of containing significant additions and corrections to previous editions; and Lanczos (1986), an attractively meditative text emphasising conceptual aspects. Among more complete and authoritative books, I recommend: Arnold's magisterial (1989), the beautifully careful and complete textbooks of Desloge (1982) and Johns (2005), and Papastavridis' monumental and passionate (2002). All these books also cover Hamiltonian mechanics and Hamilton-Jacobi theory.

been neglected is, again, the matter-in-motion picture: i.e. the widespread belief that the ontology of classical mechanics is unproblematic, and that every problem is in principle solved by the deterministic laws—so why bother to look at the details of analytical mechanics?)

To indicate the road ahead, I shall also state the morals first, in Section 2. Then I illustrate them in two further Sections. In Section 3, I describe the basic ideas of analytical mechanics, using the principle of virtual work and d'Alembert's principle to lead up to the central equations of Lagrangian mechanics: Lagrange's equations. Then in Section 4, I discuss variational principles, the reduction of a problem to a simpler problem, and the role of symmetries in making such reductions: I end by proving a simple version of Noether's theorem. Most of the morals will be illustrated, usually more than once, in each of Sections 3 and 4.

Two more preliminary remarks: both of them about the limited scope of this paper, and its companion covering Hamiltonian mechanics and Hamilton-Jacobi theory.

(1): *Eschewing geometry*:—
One main way in which my treatment will be elementary is that I will mostly eschew the use of modern geometry to formulate mechanics. Agreed, the use of geometry has in the last century formed a large part of the glorious development of analytical mechanics that I began by celebrating. For example, the *maestro* is no doubt right when he says 'Hamiltonian mechanics cannot be understood without differential forms' (Arnold, 1989: 163). But apart from a few passages, I will eschew geometry, albeit with regret. One cannot understand everything in one go—sufficient unto the day is the formalism thereof!

(2): *Finite systems: (Ideal)*:—
Another way in which my treatment will be elementary is that I will only consider analytical mechanics' treatment of finite-dimensional systems (often called, for short: finite systems). These are systems with a finite number of degrees of freedom. That is: they are systems whose configuration (i.e. the positions of all component parts) can be specified by a finite number of (real-valued) variables. So any finite number of point-particles is an example of a finite system.

If one takes bodies (i.e. bulk matter) to be continua, i.e. as having matter in every region, no matter how small, of their apparent volume, then one is taking them as infinite-dimensional, i.e. as having infinitely many degrees of freedom. For there will be at least one degree of freedom per spatial point. In that case, to treat a body as finite-dimensional represents a major idealization. But as we shall see, analytical mechanics does this. Indeed, it idealizes yet more. For even if bodies were ultimately discrete, the total number of their degrees of freedom would be enormous, though finite. Yet analytical mechanics typically describes bodies using a small finite number of variables; for example, it might describe the configuration of a bead on a ring by just the position of the centre of mass of the bead. So whether or not bodies are in fact continua (infinite systems), analytical mechanics typically makes the idealization of treating them with a finite, indeed small finite, number of coordinates. These are in effect collective

coordinates that aggregate information about many underlying microscopic degrees of freedom.

This idealization—of treating infinite or "large-finite" systems as "small-finite"—will occur often in what follows; so it will be convenient to have a label for it. I will call it (Ideal).

But (Ideal) will not affect my morals; (and not just because, being an idealization adopted by analytical mechanics itself, my morals must anyway "follow suit"). There are two reasons. First, the very same morals could in fact be drawn from the analytical mechanics of infinite systems: but to do so in this paper would make it unduly long and complicated.[7]

Second, (Ideal) can of course be justified in various ways. It can be justified empirically. For like any idealization, it amounts to limiting the theory to those physical situations where the error terms arising from the idealization are believed to be negligible; (I shall be more precise about idealization in Section 2.1.4). And the countless empirical successes of analytical mechanics using (small!) finitely many degrees of freedom shows that indeed, the errors often are negligible. Besides, (Ideal) can in various ways be justified theoretically; for example, by theorems stating that for systems with many degrees of freedom, a collective coordinate like the system's centre of mass will evolve according to a formula involving only a small number of degrees of freedom.[8]

2 Morals

Recall the idea which I announced as central to all of analytical mechanics: that by considering the set of all possible states of systems, and making appropriate mathematical constructions on it, one can formulate a general scheme with various merits for solving problems—or if not solving them, at least getting significant information about them.

With this idea in hand, I can give a general statement of my four morals. For all four arise from this idea. They fall into two pairs, the first pair concerning scientific method, and the second pair ontology. In each pair, one of the morals is the main one since it is more novel (and perhaps controversial!) than the other one. For the minor morals rebut the idea that classical mechanics is unproblematic, just matter-in-motion. As I said in Section 1, that idea is wrong. But as I admitted, for specialist philosophers of physics its being wrong is old news: so for them, these two rebuttals will be less important.

[7] Anyway, it is pedagogically indispensable to first treat the case of finitely many degrees of freedom. It was also no doubt historically indispensable: even a genius of Lagrange's or Hamilton's stature could not have first analysed the infinite system case, treating the finite system case as a limit.

[8] Such theorems are not special to analytical mechanics: in particular, what is often called 'the vectorial approach' to mechanics has such theorems. For more discussion of (Ideal), cf. Section 2.2.2's statement of my fourth moral.

To help keep track of the morals, I shall give each moral a label, to be used in later Sections. The main moral about method will be labelled (Scheme); the minor one will have two labels, (Reformulate) and (Restrict). The main moral about ontology will be (Modality); the minor one (Accept). (So main morals get nouns, and minor ones verbs.)

The morals are presented in the following Sections. First, the morals about method:

(Scheme): Section 2.1.4. (Sections 2.1.1 to 2.1.3 give necessary preliminaries.) This moral is about analytical mechanics' schemes for representing problems. Section 2.1.4 will also introduce labels for four merits the Lagrangian scheme enjoys; (merits also shared by the Hamiltonian and Hamilton-Jacobi schemes). Those labels are: (Fewer), (Wider), (Reduce), (Separate).

(Reformulate) and (Restrict): Section 2.1.5. This moral is about the methodological value of reformulating and restricting theories.

Then the morals about ontology:—

(Modality) : Section 2.2.1. This moral is about analytical mechanics' involvements in modality (necessity and possibility). I will distinguish three grades of modal involvement, labelled (Modality;1st) through to (Modality;3rd). Lagrangian mechanics will exhibit all three grades; (as will Hamiltonian and Hamilton-Jacobi mechanics).

(Accept): Section 2.2.2. This moral is about the subtle and various ontology of analytical mechanics, in particular Lagrangian mechanics.

2.1 Method

My main moral about method, (Scheme), is that the provision of general schemes for representing and solving problems is a significant topic in the analysis of scientific theories: not least because it falls between two topics often emphasised by philosophers, "laws of nature" and "models". I will urge this moral by showing that Lagrangian mechanics is devoted to providing such a scheme for mechanical problems. But I will lead up to Section 2.1.4's description of Lagrangian mechanics' scheme, and its merits, by :

(i): discussing some of physics' various senses of 'solve a problem': an ambiguous phrase, which repays philosophical analysis! (Section 2.1.1 and 2.1.2);

(ii): reporting some results about differential equations that I will need (Section 2.1.3).

Finally, Section 2.1.5 states my minor moral about scientific method.[9]

[9]I shall duck out of trying to prove my accusation that the philosophical literature has overlooked a significant topic, between "laws of nature" and "models'. But here is one example. Giere gives two extended discussions of classical mechanics to illustrate his views about laws, models and related notions like scientific theories and hypotheses (1988: 62-91; 1999: 106-117, 165-169, 175-180). Very roughly, his view (partly based on ideas from cognitive science) is that science hardly needs laws, and that theories are appropriately structured clusters of models. Maybe: but it is noteworthy that he does not articulate the level of description provided by analytical mechanics. For him, Newton's laws are typical examples of laws, and the harmonic oscillator, or inverse-square orbital motion, are typical

2.1.1 (Scheme): what is it to be "given a function"?

Throughout physics we talk about 'solving a problem'. The broad meaning is clear. To solve a problem is to state the right answer to a question.[10] And similarly for related phrases like 'reducing one problem to another': to reduce one problem to a second one is to show that the right answer to the second immediately yields the right answer to the first; and so on.

But there is a spectrum of meanings of 'stating a right answer', ranging from the "in-principle" to the "useful". The spectrum arises from a point familiar in philosophy: the ambiguity of what it is to be "given" an object (in our case, an answer to a question). One is always given an object under a mode of presentation, as Frege put it; and the mode of presentation may be useless, or useful, for one's purposes.

A standard example in the philosophy of mathematics (specifically, discussion of Church's thesis) makes the point clear. Suppose I give you a function f on positive integers by defining: $f(n) := 1 \quad \forall n$, if Goldbach's conjecture is true; and $f(n) := 0 \ \forall n$, otherwise. Have I given you an effectively computable function? Intuition pulls both ways: Yes, since both the constant-1 and constant-0 functions are effectively computable; No, since the mode of presentation that the definition used makes it useless for your purpose of calculating a value of the function.

Similarly in physics, in particular mechanics. The solution to a problem, the right answer to a question, is typically a function, especially a function describing how the position of a body changes with time. And a number or function can be "given" uselessly (one might say: perversely) as f was in the example. So the extreme in-principle meaning for 'stating the right answer' is logically weak: it tolerates any way of giving the answer. In this sense, one may take a specification of appropriate initial conditions (and perhaps boundary conditions) for any deterministic set of equations to "solve" any problem about the variables' values in the future. Though this in-principle sense is cavalier about whether we could ever "get our hands on" the answer to the problem (i.e. state it in useful form), it is important. It is connected with significant mathematics, about the existence and uniqueness of solutions to differential equations, since for the initial conditions to "solve", even in this in-principle sense, any such problem, requires that they dictate a unique solution. (Hence, the jargon: 'a well-posed problem', 'the initial-value problem' etc.)

On the other hand, there are logically stronger meanings of 'solution'. There is obviously a great variety of meanings, depending on just which ways of stating the right answer (what modes of presentation) are accepted as "useful". Broadly speaking,

models; (in fact, the closest Giere comes to articulating analytical mechanics is in a brief discussion of axiomatic approaches: 1988: 87-89). Similarly, I submit, for the rest of the literature.

[10] For present purposes, 'right' here can mean just 'what the theory dictates' not 'empirically correct'. That is, I can set aside the threat of empirical inadequacy, whether from neglecting non-negligible variables, or from more fundamental flaws, such as those that quantum theory lays at the door of classical mechanics. Of course, nothing I say in this paper about analytical mechanics' schemes for solving all problems is meant to deny the quantum revolution!

there is a spectrum, from the tolerance of "in-principle" to very stringent conceptions of what is acceptable: for example that the function that solves a problem must be one of an elite family of functions, e.g. an analytic function.

Here we meet the long, rich history of the notion of a mathematical function. For, broadly speaking, physics has repeatedly (since at least 1600) come across problems in which the function representing the solution (or more generally, representing a physical quantity of interest) does *not* belong to some select family of functions; and in attempting to handle such "rogue" functions, physics has often prompted developments in mathematics. In particular, these problems in physics have been among the main causes of the successive generalizations of the notion of function: a process which has continued to our own time with, for example, distributions and fractals.

The general schemes provided by analytical mechanics (whether Lagrangian, Hamiltonian or Hamilton-Jacobi) do not secure solutions in any of these stringent senses, except in a few cases. As we shall see, these schemes lie in the middle of the spectrum that ranges from the tolerance of "in-principle" to the stringent senses. So neither these stringent senses, nor the history of how physics had repeatedly to go beyond them, is my main topic; (and each is of course a large topic on which many a book has been written!). But I need to say a little about these senses and this history, just because analytical mechanics' schemes are themselves examples—and historically very significant ones—of how physics' conception of solving problems has gone beyond the stringent senses.

In other words: it will help locate these schemes, and the middle of the spectrum, if I report some of the pressure towards the middle from the stringent extreme. So Section 2.1.2 gives a (brutal!) summary of the history of the notion of function. Then Section 2.1.3 reports some basic results about differential equations, as preparation for Section 2.1.4's return to the schemes.

2.1.2 Generalizing the notion of function

2.1.2: Preamble Since at least the seventeenth century, the notion of a mathematical function (and allied notions like that of a curve) has been successively generalized—and often it has been problems in physics that prompted the generalization. I shall sketch this development, giving in Paragraphs 2.1.2.A-C three examples of how solving problems, in particular solving differential equations, prompted going beyond stringent conceptions of function. (For more details of the history, cf. Bottazini (1986), Lutzen (2003), Kline (1972) and Youschkevitch (1976).)

Warning:— A reassurance at the outset: none of the details in these three Paragraphs will be needed later in the paper, though the main idea of the second Paragraph, viz. quadrature, will be important later.

Nowadays, our conception of function is utterly general: any many-one mapping between arbitrary sets. But this represents the terminus of a long development. In the seventeenth century, a much narrower concept had emerged, mainly from the study of

curves and its principal offspring, the calculus. Authors differed; but speaking broadly (and in modern terms), a function was at first taken to be a real function (i.e. with $\mathbb{R}$ as both domain and codomain) that could be expressed by a (broadly algebraic) formula. One main episode in the formation of this concept was Descartes' critique in his *La Geometrie* (1637: especially Book II) of the ancient Greeks' classification of curves; and in particular, their disparagement of mechanical curves, i.e. curves constructed by suitable machines. More positively, Descartes accepted (and classified as 'geometric') all curves having an algebraic equation (of finite degree) in x and y; thereby including some curves the Greeks disparaged as 'mechanical', such as the conchoid of Nicomedes and the cissoid of Diocles (But Descartes himself, and his contemporaries, of course also studied non-algebraic curves. For details, cf. Kline (1972: 117-119, 173-175, 311-312, 335-340) and Youschkevitch (1976: 52-53).)

But the exploration of new problems, in (what we would call!) pure mathematics and physics, gradually generalized the concept: in particular, so as to include trigonometric and exponential functions, and functions represented by infinite series—though there was of course never an agreed usage of the word 'function'. (The word 'function' seems to be due to Leibniz; as are 'constant', 'variable' and 'parameter'.) Kline (1972: 339) reports that the most explicit seventeenth century definition of 'function' was Gregory's (1667): that a function is a quantity obtained from other quantities by a succession of operations that are either

(i) one of the five familiar algebraic operations (addition, subtraction, multiplication, division and extraction of integral roots), or

(ii) the operation we would call taking a limit.

But Gregory's (ii) was lost sight of. The contemporary emphasis was on (i), augmented with the trigonometric and exponential functions and functions represented by series.

Let us put this more precisely, in some modern jargon which is customary, though not universal (e.g. Borowski and Borwein 2002). The *elementary operations* are: addition, subtraction, multiplication, division and extraction of integral roots, in a given field. (Of course, the general notion of a field dates from the late nineteenth century, together with group, ring etc.; so here we just take the field to be the reals.) An *algebraic function* is any function that can be constructed in a finite number of steps from the elementary operations, and the inverses of any function already constructed; e.g. $\sqrt{(x^2-2)}$. An *elementary function* is any function built up from the exponential and trigonometric functions and their inverses by the elementary operations, e.g. $\log[\tan^{-1}\sqrt{(\exp x^2)}+1]$. A *transcendental function* is one that is not elementary.[11]

So by the early eighteenth century, it had emerged that the function that answers a physical problem, in particular an indefinite integral expressing the solution of a differential equation, is often not elementary. (Though this is now a commonplace of the pedagogy of elementary calculus, it is of course hard to *prove* such functions are not

[11] A related usage: an algebraic number is any number that is a root of a polynomial equation with coefficients drawn from the given field, and a transcendental number is any number that is not algebraic. But in this usage, the field is almost always taken to be the rationals, so that $\sqrt{2}$ is algebraic (and there are only denumerably many algebraic numbers), while π and e are transcendental.

elementary.) Accordingly, what was regarded as a solution, and as a general method of solution, was generalized beyond the elementary functions and techniques associated with them.

To illustrate, I will discuss (in succeeding Paragraphs) three main ways in which such a generalization was made, namely:

(A): to include infinite series; a topic which leads to the foundations of the calculus.

(B): to include integrals, even if these integrals could themselves only be evaluated numerically. (Jargon: *quadrature* means the integration, perhaps only numerical, of a given function).

(C): to include weak solutions; which originated in a famous dispute between d'Alembert and Euler.

Warning: The details of (A)-(C) are not needed later on, and can be skipped. But the main idea of (B), the idea of reducing a problem to a quadrature, will be prominent later on.

2.1.2.A Infinite series, and the rigorization of the calculus I have already mentioned the admission of infinite series. That is to say: such series were not required to be the series for an elementary function, especially since one sought series solutions of differential equations.

For simplicity and brevity, I will only consider first-order ordinary differential equations, i.e. equations of the form $\frac{dx}{dt} = f(x,t)$. To find an infinite series solution of such an equation, one assumes a solution of the form

$$x = a_0 + a_1 t + a_2 t^2 + \dots \quad ; \quad \dot{x} = a_1 + 2a_2 t + 3a_3 t^2 + \dots \ , \tag{2.1}$$

substitutes this into the question, and equates coefficients of like powers of t. Though Newton, Leibniz and their contemporaries had provided such solutions for various equations (including higher order equations), the method came to the fore from about 1750, especially in the hands of Euler. Such series of course raise many questions about convergence, and about what information might be gained by proper handling of divergent series.

Here we meet another large and complicated story: the development of the calculus, especially its rigorization in the nineteenth century by such figures as Cauchy and Weierstrass (Bottazini 1986: Chapters 3, 7; Boyer: 1959 Chapter 7; Kline 1972: Chapter 40). But for present purposes, I need only note two aspects of this story.

(i): With the rigorization of analysis, a function became an arbitrary "rule" or "mapping"; and with the late nineteenth century's set-theoretic foundation for mathematics, this was made precise as a kind of set, viz. a set of ordered pairs. Note the contrast with the eighteenth century conception: for Euler and his contemporaries, a function was first and foremost *a formula*, expressing how one "quantity" (cf. Gregory's definition above) depended on another.

(ii): Again, physics' need for functions that were not "smooth" (in various stringent senses) was a major motivation for this rigorization. It was also a significant reason

why, after the rigorization was accomplished, one could not go back to founding analysis solely on such smooth functions. (But even in the late nineteenth century, many mathematicians (including great ones like Weierstrass and Poincaré!) hankered after such a return. Cf. Lutzen 2003: 477-484, and Paragraph 2.1.2.C below.)

Several episodes illustrate both these aspects, (i) and (ii). For example, Dirichlet's famous example of a function that cannot be integrated occurs in a paper (1829) on the convergence of Fourier series (a physically motivated topic). He suggests

$$\phi(x) := c \quad \text{for } x \text{ rational} ; \quad \phi(x) := d \quad \text{for } x \text{ irrational} ; \tag{2.2}$$

the first explicit statement of a function other than through one or several analytic expressions (Bottazini (1986: 196-201), Lutzen (2003: 472), Kline (1970: 950, 966-967). Another example is the realization (in the 1870s, but building on Liouville's work in the 1830s) how "rarely" is the solution of a differential equation algebraic; (for details, cf. Gray (2000: 49, 70f.).)

2.1.2.B Examples of quadrature Again I will, for simplicity and brevity, only consider first-order ordinary differential equations $\frac{dx}{dt} = f(x,t)$. Here are three standard examples of methods which reduce the integration of such an equation $\frac{dx}{dt} = f(x,t)$ to a quadrature.

(i): The most obvious example is any autonomous equation, i.e. an equation

$$\frac{dx}{dt} = f(x) \tag{2.3}$$

whose right hand side is independent of t. Eq. 2.3 gives immediately $t = \int \frac{1}{f(x)}\, dx$; inverting this, we obtain the solution $x = x(t)$.

(ii): Another obvious example is the separation of variables. That is: If $f(x,t)$ is a product, the problem immediately reduces to a quadrature:

$$\frac{dx}{dt} = g(x)h(t) \quad \Rightarrow \quad \int \frac{1}{g(x)}\, dx = \int h(t)\, dt \ . \tag{2.4}$$

(iii): Less obvious is the method of *integrating factors*, which applies to any linear first-order ordinary differential equation, i.e. an equation of the form

$$\frac{dx}{dt} + p(t)x = q(t) \ . \tag{2.5}$$

This can be integrated by multiplying each side by the integrating factor $\exp[\int^t p(t')dt']$. For the left hand side then becomes $\frac{d}{dt}(x(t).\exp(\int^t p))$, so that multiplying the equation by dt, integrating and rewriting the upper limit of integration as t, the solution is given by

$$x(t).\exp(\int^t p) = \int^t dt' \ \{ \ \exp(\int^{t'} p(t'')dt'') \ . \ q(t') \ \} \ . \tag{2.6}$$

These three examples, indeed all the elementary methods of solving first-order equations, were known by 1740. In particular, the third example (and generalizations of it

to non-linear first-order equations) was given by Euler in 1734/35; (and independently by Clairaut in 1739/40: Kline 1972: 476).

So much by way of examples of quadrature. The overall effect of the developments sketched in this Paragraph and 2.1.2.A was that by the middle of the eighteenth century, the prevalent conception of a function had become: an analytic expression formed by the processes of algebra and calculus.

Here 'processes of algebra and calculus' is deliberately vague, so as to cover the developments I have sketched. And 'analytic expression' emphasises the point above (in (i) of Paragraph 2.1.2.A) that a function was defined as a formula: not (as after the rigorization of analysis) as an arbitrary mapping, indeed a set-theoretic object.

That a function was a formula meant that it was *ipso facto* defined for all values of its variable(s), and that identities between functions were to be valid for all such values. These features were at the centre of a famous dispute, that led to our third generalization of the notion of function ...

2.1.2.C Vibrating strings and weak solutions Namely, the dispute between Euler and d'Alembert over the nature of the solutions of d'Alembert's wave equation (1747) describing the displacement $f(x,t)$ of a vibrating string:

$$\frac{\partial^2 f}{\partial t^2} = a^2 \frac{\partial^2 f}{\partial x^2} \quad . \tag{2.7}$$

Though this paper will be almost entirely concerned with ordinary differential equations, whose theory is enormously simpler than that of partial differential equations, it is worth reporting this dispute. Not only did it represent the first significant study of partial differential equations. More important: Euler's viewpoint foreshadowed important nineteenth century developments in the notions both of function, and of solution of an equation—as we shall see. (For details of this dispute, cf. Bottazini (1986: 21-43), Kline (1972: 503-507), Lutzen (2003: 469-474), and Youschkevitch (1976: 57-72); Wilson (1997) is a philosophical discussion).

More precisely, the dispute was about whether eq. 2.7 could describe a plucked string, i.e. a string whose configuration has a 'corner'. In the simplest case, this would be a matter of an initial condition in which, with the string extending from $x = 0$ to $x = d$:

(i) $f(x,0)$ consists of two straight lines, with a corner at $x = c < d$. That is: $f(x,0) = kx$, k a constant, for $0 \leq x \leq c$ and $f(x,0) = (\frac{-kc}{(d-c)})x + \frac{kcd}{(d-c)}$, for $c \leq x \leq d$.

(ii) the string has zero initial velocity: i.e. $\frac{\partial f}{\partial t} \mid_{t=0} \equiv 0$.

More generally, the question was whether the wave equation can describe a waveform, in particular an initial condition, that is (as we would now say) continuous but not (even once) differentiable.

D'Alembert argued that it could not. His reasons lay in the prevalent contemporary conception of a function just outlined. But his arguments also come close to formulating what later became the standard requirement on a solution of a second-order equation

such as eq. 2.7: viz. that it be twice differentiable in both variables.

Euler took the view that analysis should be generalised so that it could indeed describe a plucked string: as he puts it (1748) 'so that the initial shape of the string can be set arbitrarily ... either regular and contained in a certain equation, or irregular and mechanical'. More generally, Euler advocates allowing functions given by various analytic expressions in various intervals; or even by arbitrary hand-drawn curves for which, he says, the analytic expression changes from point to point. (He calls such functions 'discontinuous'.)

To be precise: according to Truesdell (1956: p. xliii, 1960: p. 247-248), Euler proposes to mean by 'function' what we now call a continuous function with piecewise continuous slope and curvature. Accordingly, he disregards the differentiability conditions implicit in eq. 2.7 and focusses on the general solution he finds for it, viz. $f(x-at)+f(x+at)$ with f an arbitrary function in his sense.

Truesdell stresses the scientific and philosophical importance of Euler's innovation. Indeed, he goes so far as to say that 'Euler's refutation of Leibniz's law [i.e.: Leibniz's doctrine that natural phenomena can be described by what we now call analytic functions] was the greatest advance in scientific methodology in the entire century' (1960: p.248).[12]

That may well be so: I for one will not question either Truesdell's scholarship or Euler's genius! In any case: several major developments thereafter—some of them well into the nineteenth century—vindicated Euler's viewpoint that analysis, and in particular the conception of solutions of differential equations, should be generalised.

To illustrate, I will sketch one such development: *weak solutions* of partial differential equations. Though the idea is anticipated by Euler and contemporaries (e.g. Lagrange in 1761: cf. Bottazzini 1986: p. 31-33), it was properly established only in the nineteenth and twentieth centuries; partly through the investigation of shock waves—another example of physics stimulating mathematics.[13]

The idea is to multiply the given partial differential equation $L[f]=0$ by a test function g (roughly: a function that is sufficiently smooth and has compact support), and then to integrate by parts (formally) so as make the derivatives fall on g. A function f satisfying the resulting equation for all test functions g is called a 'weak solution' of the original equation. Thanks to the integration by parts, such an f will in general not obey the standard differentiability conditions required of a solution of $L[f]=0$. (But I should add that one common strategy for finding solutions in the usual sense is to first construct a weak solution and then prove that it must be a solution in the usual

[12]Truesdell's reading is endorsed by Bottazini (1986: 26-27) and Youschkevitch (1976: 64, fn 18, 67). But I should add that—as all these scholars of course recognize—some of Euler's work (even after 1748) used, and sometimes even explicitly endorsed, more traditional and restrictive notions of function.

[13]For a philosophical discussion of different kinds of "optimism" about mathematics' ability to describe natural phenomena (including praise of Euler's optimism), cf. Wilson (2000). Stöltzner (2004) is a sequel to this paper, arguing that optimism can and should be combined with what Wilson calls "opportunism".

sense.)

To give more details in modern but heuristic terms, I will consider only a linear first-order partial differential equation for the unknown function f of independent variables x and t; (I follow Courant and Hilbert 1962: Chap.V.9, p. 486-490). So the equation is, with partial derivatives indicated by subscripts:

$$L[f] := A(x,t)f_x + B(x,t)f_t + C(x,t) = 0 \quad . \tag{2.8}$$

We define the operator L^* adjoint to L by the condition that $gL[f] - fL^*[g]$ is a divergence expression. That is, we define

$$L^*[g] := -(Ag)_x - (Bg)_t + Cg \quad \text{so that} \quad gL[f] - fL^*[g] = (gAf)_x + (gBf)_t \quad . \tag{2.9}$$

For the domain R in which f is considered, we now consider functions g that have compact support in a subregion S of R (called *test functions*); so that integrating eq. 2.9 over S, we obtain by Gauss' theorem

$$\int\int_S (gL[f] - fL^*[g]) \; dx\, dt = 0 \; . \tag{2.10}$$

If f is a solution of the partial differential equation, i.e. $L[f] = 0$, then

$$\int\int_S \quad fL^*[g] \; dx\, dt = 0 \; . \tag{2.11}$$

(There is a converse, roughly as follows. If eq. 2.11 holds for f with continuous derivatives, for all suitably smooth test functions g with compact support in any suitable subregion S, then eq. 2.11 yields $\int\int_S \; gL[f]\, dxdt = 0$: which implies that $L[f] = 0$.)

This motivates the following (admittedly, non-rigorous!) definition. Suppose a function $f(x,t)$ and its partial derivatives are piecewise continuous; (i.e. at worst, each possesses jump discontinuities along piecewise smooth curves). Such a function f is called a *weak solution* of $L[f] = 0$ in R if for all suitable subregions S of R, and suitably smooth test functions g with compact support in S

$$\int\int_S \quad fL^*[g] \; dx\, dt = 0 \; . \tag{2.12}$$

I shall not further develop the idea of a weak solution, since it will not be used in the rest of the paper. But I note that it gives yet more examples of the theme at the end of Section 1.1: the subtleties of determinism in classical mechanics.

(i): It was discovered (with a shock!) in the mid-nineteenth century that for a non-linear equation, a solution that begins with smooth, even analytic, initial data can develop discontinuities in a finite time. And:

(ii): For weak solutions of the Euler equations for fluids, determinism is strikingly false. Scheffer (1993) and Shnirelman (1997) exhibit weak solutions on $\mathbb{R}^3 \times \mathbb{R}$ with compact support on spacetime. This means that a fluid is initially at rest ($t = 0$) but

later on ($t = 1$) starts to move with no outside stimulus, and later still ($t = 2$) returns to rest: the motion being always confined to a ball $B \subset \mathbb{R}^3$![14]

To conclude this Subsection: in this history of generalizations of the notion of function, I have emphasised the influence of differential equations. I of course admit that other influences were equally important, though often related to differential equations: e.g. the rigorization of analysis.

But my emphasis suits this paper's purposes; and is surely "not too false" to the history. Recall the paper's motto; and these two famous remarks by late nineteenth-century masters. Lie said 'The theory of differential equations is the most important branch of modern mathematics'; and—more evidence of mathematics being stimulated by physics—Poincaré said 'Without physics, we would not know differential equations'.

2.1.3 Solutions of ordinary differential equations; constants of the motion

I now report some basic results of the theory of ordinary differential equations: results which are crucial for Lagrangian mechanics (and for Hamiltonian and Hamilton-Jacobi mechanics). Incidentally, this Subsection will also give a glimpse of a fourth way that functions have been generalized: viz. to include discontinuous functions, so as to describe dynamical chaos. But this paper (and is companion) will not discuss chaos. And here my emphasis will be on very elementary aspects of differential equations. I shall report that:

(A) this theory guarantees the local existence and uniqueness of solutions, and of constants of the motion; but

(B) for most problems, these constants only exist locally.

Both these points, (A) and (B), will be fundamental for later Sections.

2.1.3.A The local existence and uniqueness of solutions I shall follow an exposition of the basic theorems about solutions to systems of ordinary differential equations, by a *maestro*: Arnold (1973).

For our purposes, these theorems can be summed up in the following four propositions. Arnold calls the first, about first-order ordinary differential equations in Euclidean space $\mathbb{R}^n$, the 'basic theorem'. It not only secures the local existence and uniqueness of solutions; it also characterizes the local constants of the motion; and it underpins the corresponding propositions about differential equations that are of higher order than the first, or that are defined on differential manifolds rather than $\mathbb{R}^n$.

[14] These weak solutions are discontinuous unbounded L^2 functions. Underlying Shnirelman's example is the physical idea (which had already been recognized) of an inverse energy cascade in two-dimensional turbulence. Given a force $f(x, t)$ with a small spatial scale, energy is transported via the non-linearity of the Euler equations to the lower frequencies and longer spatial scales. In particular, if f's spatial scale is infinitely small, simple dimension considerations show that it nevertheless takes only a finite time for the energy to reach the low-frequency range. I am very grateful to Tim Palmer for explaining these examples to me.

(i): *The Basic Theorem* (Arnold 1973: 48-49).
Consider a system of n first-order ordinary differential equations

$$\dot{\mathbf{x}} = \mathbf{v}(\mathbf{x}) \ , \quad x \in U \tag{2.13}$$

on an open set $U \subset \mathbb{R}^n$; equivalently, a vector field $\mathbf{v}$ on U. Let $\mathbf{x}_0$ be a non-singular point of the vector field, i.e. $\mathbf{v}(\mathbf{x}_0) \neq 0$. Then in a sufficiently small neighbourhood V of $\mathbf{x}_0$, there is a coordinate system (formally, a diffeomorphism $f : V \to W \subset \mathbb{R}^n$) such that, writing $y_i : \mathbb{R}^n \to \mathbb{R}$ for the standard coordinates on W and $\mathbf{e}_1$ for the first standard basic vector of $\mathbb{R}^n$, eq. 2.13 goes into the very simple form

$$\dot{\mathbf{y}} = \mathbf{e}_1; \ \text{ i.e. } \ \dot{y}_1 = 1, \ \ \dot{y}_2 = \ldots = \dot{y}_n = 0 \ \text{ in } \ W \ . \tag{2.14}$$

(In geometric terms: $f_*(\mathbf{v}) = \mathbf{e}_1$ in W.) On account of eq. 2.14's simple form, Arnold suggests the theorem might be called the 'rectification theorem'. NB: For simplicity, I have here set aside the non-autonomous case where $\mathbf{v} = \mathbf{v}(t, \mathbf{x})$; for details of that case, cf. ibid., p. 56.

(ii): *Solutions*; (ibid.: 12, 50).
Let us define a *solution* (aka: *integral curve*) of eq. 2.13 to be a differentiable mapping $\phi : I \to U$ of the real open interval $I \subset \mathbb{R}$ to U such that for all $\tau \in I$

$$\frac{d}{dt} \mid_{t=\tau} \phi(t) = \mathbf{v}(\phi(\tau)) \ . \tag{2.15}$$

The image $\phi(I)$ is called the *phase curve* (also sometimes, the integral curve).

Then Proposition (i) implies that there is a solution of eq. 2.13 satisfying the initial condition $\phi(t_0) = \mathbf{x}_0$. But NB: this solution need only exist locally in time: as I mentioned in Section 1, a solution need not globally exist even for familiar "deterministic" theories, such as point-particles interacting by Newtonian gravity.

Proposition (i) also implies that the local solution is unique in the obvious sense that any two solutions with the same initial condition are equal on a common sub-interval.

(iii): *Constants of the motion*; (ibid.: 75-78).
A differentiable function $f : U \to \mathbb{R}$ is called a *constant of the motion* (aka: *first integral*) of eq. 2.13 iff its derivative in the direction of the vector field $\mathbf{v}$ vanishes. Equivalently: iff f is constant along every solution $\phi : I \to U$; iff every phase curve $\phi(I)$ belongs to a unique *level set* $f^{-1}(\{c\}), c \in \mathbb{R}$, of f.

Typically, eq. 2.13 has no first integrals other than the trivial constant functions $f(U) = c \in \mathbb{R}$.

But it follows from the Basic Theorem that *locally* there are non-constant first integrals. That is, in the notation of (i):

(a): There is a neighbourhood V of $\mathbf{x}_0$ such that eq. 2.13 has $n - 1$ functionally independent first integrals $f_1, \ldots, f_{n-1}$ in V. (We say $f_1, \ldots, f_m : U \to \mathbb{R}$ are *functionally independent* in a neighbourhood of $x \in U$ if their gradients are linearly independent. More precisely: if the rank of the derivative $f_* \mid_x$ of the map $f : U \to \mathbb{R}^m$ determined by the functions $f_1, \ldots, f_m$ equals m.)

(b): Moreover, any first integral of eq. 2.13 in V is a function of $f_1, \ldots, f_{n-1}$.

(c): Of course, in the coordinate system in which eq. 2.13 take the very simple form eq. 2.14, the coordinates $y_2, \ldots, y_n$ give us $n-1$ functionally independent first integrals; and the other first integrals are all the arbitrary differentiable functions of these coordinates.

Assertions (a)-(c) give a sense in which the Basic Theorem secures the existence of a coordinate system in which the problem of integrating eq. 2.13 is *completely solved, locally*. But I stress that this does *not* mean it is easy to *write down* this coordinate system: to write down the diffeomorphism f. In general, it is very hard to do so!

This point can hardly be over-emphasised. It will be a recurrent theme in this paper—and I will discuss it in philosophical terms already in Section 2.1.4.

(iv): *Other cases*:
Corresponding definitions and propositions hold for:

(a) ordinary differential equations of higher order, principally by the standard device of writing down an equivalent system of first-order equations in which new variables represent the higher derivatives of the original variable or variables (ibid.: 59-61); and

(b) collections of such equations of varying orders (p. 62-63); and

(c) ordinary differential equations defined, not on a patch of $\mathbb{R}^n$ but on a differential manifold (p. 249-250).

There are countless details about (iv) which I will not report; (some more details are in Sections 3.3.2 and 4.7.3). Here I note only the following:—

In Lagrangian mechanics, the dynamics of a system with n configurational degrees of freedom is essentially described by n second-order ordinary differential equations; or equivalently, by $2n$ first-order equations. (In most cases, this system of equations is defined on a manifold, not a patch of $\mathbb{R}^n$.) This system has a locally unique solution, specified by $2n$ arbitrary constants; (roughly, the initial positions and velocities of the system's constituent particles). Besides, we define a first integral of a differential equation of arbitrary order (or of a system of them) as a first integral of the equivalent system of first-order equations. This means that for this system, as in (iii) above: global constants of the motion are rare; but locally, we are guaranteed that there are $2n-1$ of them.

2.1.3.B The rarity of global constants of the motion: the circle and the torus I said in (iii) of Paragraph 2.1.3.A that typically, eq. 2.13 has first integrals other than the trivial constant functions $f(U) = c \in \mathbb{R}$, only *locally*. We will later be much concerned with the few global constants such as energy that arise in mechanics. But it is worth giving at the outset two (related) examples of a system with no global constants of the motion. For they are a simple and vivid illustration of the rarity of such constants. (They are also a prototype for: (i) topics that will loom large in the companion paper, viz. Poincaré's theorem, and the theory of completely integrable systems; (ii) structural stability, a central topic in catastrophe and bifurcation theory.)

One or both examples occur in many books. But again I recommend Arnold. For

proofs of the results below, cf. his (1973: 160-167); or more briefly, his (1989: 72-74). For further results (including details about topics (i) and (ii)), cf. his (1983: 90-112; 1989: 285f.).

First example: the circle:—
The first example is a "toy-model" in discrete time, rather than a classical mechanical system. Consider a circle S, and let $\tau : S \to S$ be a rotation through an angle α. (Think of S as a space of states, and τ as time-evolution in discrete time.) If α is *commensurable* (aka: commensurate) with 2π, i.e. $\alpha = 2\pi(m/n)$ with integers m, n , then τ^n is the identity. So for any point $x \in S$, the orbit of x under the repeated action of τ (the set of images of x) is closed: it eventually rejoins itself. But if α is incommensurable with 2π (i.e. $\alpha \neq 2\pi(m/n)$ for any integers m, n), then the set of images $\{\tau^i(x) \mid i \in \mathbf{Z}\}$ of any point x is everywhere dense in S.

Now suppose we define a constant of the motion for this discrete-time dynamical system on analogy with (iii) of Paragraph 2.1.3.A. We need only require continuity, not differentiability: so we say a continuous function $f : S \to \mathbb{R}$ is a constant of the motion iff throughout each orbit f is constant. (Here 'throughout' emphasises that the definition is "global in time".)

(i): If $\alpha = 2\pi(m/n)$ with integers m, n in their lowest terms, there are many constants of the motion: any continuous real-valued function f defined on an arc of $\frac{2\pi}{n}$ radians defines one. But:—

(ii): If α is incommensurable with 2π, the only constants of the motion are the trivial constant functions $f(S) = \{c\}, c \in \mathbb{R}$.

It is worth expressing (ii) in terms of discontinuous functions. For that will show how even simple systems prompt a general notion of function. (This point gets greatly developed in the study of chaos—though as I said at the start of this Subsection, I will not discuss chaos.) Suppose we partition S under the equivalence relation: $x \equiv y$ iff x is an image of y, or vice versa, under repeated application of τ. Then any function $f : S \to \mathbb{R}$ that is constant on the cells of this partition (i.e. whose level sets $f^{-1}(c)$ are cells, or unions of cells) is either discontinuous at every point of S, or a trivial constant function $f(S) = \{c\}, c \in \mathbb{R}$.

Second example: the torus:—
The second example is a harmonic oscillator in two spatial dimensions, but with different frequencies in the two dimensions. So the equations of motion are

$$\ddot{x}_i + \omega_i^2 x_i = 0 \quad , \quad i = 1, 2 \quad ; \tag{2.16}$$

which have the energy in each dimension as two constants of the motion

$$E_i = \frac{1}{2}\dot{x}_i^2 + \frac{1}{2}\omega_i^2 x_i^2 \quad ; \tag{2.17}$$

and solutions

$$x_i = A_i \cos(\omega_i t + \phi_i) \quad , \quad A_i = \frac{\sqrt{(2E_i)}}{\omega_i} \quad , \quad i = 1, 2 \quad . \tag{2.18}$$

Each E_i defines an ellipse in the $(x_i, \dot{x}_i)$ plane, so that a pair of values (E_1, E_2) defines a two-dimensional torus T (i.e. a product of two ellipses).

As we shall discuss in more detail for Lagrangian mechanics:— Here, the original system has four degrees of freedom, two for configuration and two for velocity, i.e. $x_1, x_2, \dot{x}_1, \dot{x}_2$. But the two constants of the motion have reduced the problem to only two variables. That is: given a pair (E_1, E_2), the system's state—both positions and velocities—is defined by a pair of variables, say x_1, x_2. Besides, the variables are separated: the equations for them are independent of each other.

Furthermore, we can introduce on the surface of the torus T, angular coordinates θ_1, θ_2 mod 2π, each winding around one of the two ellipses, in terms of which the equations of motion become even simpler:

$$\dot{\theta}_i = \omega_i \quad , \quad i = 1, 2 \quad . \tag{2.19}$$

So

$$\theta_i = \omega_i t + \theta_i(0) \quad , \quad i = 1, 2 \quad . \tag{2.20}$$

In terms of the θ_i (which we naturally call *longitude* and *latitude*), the motion winds around the torus uniformly.

The qualitative nature of the motion on T depends on whether the frequencies ω_1, ω_2 are *commensurable* (aka: commensurate, or rationally dependent). That is: on whether the ratio $\frac{\omega_1}{\omega_2}$ is rational. More precisely: it follows readily from the results for the discrete-time system on the circle S that:—

(i): If ω_1, ω_2 are commensurable, then every phase curve of eq. 2.19 on T is closed: it eventually rejoins itself. We say the motion is *periodic*. And similarly to the discrete-time system: any differentiable real-valued function defined on a curve γ in T that intersects every phase curve once (so that the orbit of γ is the whole of T) defines a constant of the motion which is independent of the two energies.

(ii): But if ω_1, ω_2 are incommensurable, then every phase curve of eq. 2.19 on T is everywhere dense on T: for any neighbourhood of any point on it, the curve eventually re-enters the neighbourhood. We say the motion is *quasiperiodic*. Now recall that we defined constants of the motion (in (iii) of Paragraph 2.1.3.A) to be differentiable functions. So as in the discrete-time system on S: phase curves being everywhere dense implies that the only constants of the motion independent of the energies are the trivial constant functions $f(T) = \{c\}, c \in \mathbb{R}$. That is: these are the only constants for all time. Locally, i.e. in a sufficiently small neighbourhood V of any point (θ_1, θ_2), there *are* constants of the motion independent of the energies. They are given as in case (i) by any differentiable real-valued function defined on a curve γ that intersects every phase curve in V just once; and the time-scale on which they "hold good" as constants is set by how long it takes for some such phase curve to wind around the torus and re-enter V.

And again, one could express these points by saying that any function that is constant on every phase curve is either discontinuous at every point of T, or a trivial constant function $f(T) = \{c\}$. (For more details, cf. Arnold (ibid.) who also discusses the analogues using higher-dimensional tori.)

2.1.4 Schemes for solving problems—and their merits

As I announced at the end of Section 2.1.1, analytical mechanics' schemes for representing and solving problems operate in the middle of the spectrum of meanings of 'solve a problem': neither very tolerant (even of the useless), nor very intolerant (e.g. accepting only algebraic functions). To further explain this, I must first distinguish two main topics. The first has received philosophical attention and I will set it aside. The second has not—and is my topic.

The first topic is that of approximation techniques. Mathematics and physics have of course developed an armoury of such techniques, precisely in order to overcome the predicament of how few problems are soluble in more stringent senses (e.g. analytically). That armoury is impressively large and powerful: and in recent years—not before time!—philosophicers have studied it, often as part of studying scientific models.[15]

But I shall not enter into details about this topic. For my point is that analytical mechanics' general schemes bring out another topic. Though these schemes do not (of course!) "solve all problems" in some stringent sense, they are *not* simply examples of the armoury of approximation techniques, for two (related) reasons.

First, there is a sense in which these schemes do not involve approximations—though they do involve idealizations. Agreed, there is no established philosophical usage distinguishing approximation and idealization. But I distinguish between them as follows.[16] Both approximation and idealization involve neglecting some quantities believed (or hoped!) to make little difference to the answer to a problem. But approximation does not involve simplifying, or in any way revising, the mathematical form in which the problem is *posed*. Rather, one applies the approximation in the course of solving the problem as posed. On the other hand, idealization involves neglecting such quantities, precisely by simplifying or otherwise revising the mathematical formulation of the problem; (maybe the simplification occurs implicitly, when formulating a mathematically well-defined problem, once given a verbal physical description). So as I use the terms, idealization is neglecting what you believe negligible when you first

[15]The literature is of course vast, but Morgan and Morrison (1999) is a useful recent anthology. One reason it is vast is that 'model' is used in so many senses: here, Emch's distinction (2002) between L-models (L for 'logic' or 'language') and H-models (H for 'heuristic') is helpful. Emch and Liu (2002) is a gold-mine of information about approximations and models (and much else) in thermodynamics and statistical physics.

[16]Some authors propose similar distinctions. For example, in McMullin's excellent discussion of Galileo (1985), 'causal idealization' (264f.) is like my 'approximation', and 'construct idealization' (254f.) is like my 'idealization'; and more briefly, Teller (1979: 348-349) makes almost exactly my distinction. But agreed: other authors vary, sometimes using other words, such as 'abstraction', e.g. Suppe (1989: 82-83, 94-96) and Cartwright (1989: 183-198); (thanks to Anjan Chakravartty for these two references). In any case, discussion suggests my distinction is an acceptable stipulation; though I admit that for some colleagues, the terms have other connotations, e.g. the semantic one that any idealization is strictly speaking false, while some approximations are true. Earman and Roberts (1999) is a fine discussion of the related topic of *ceteris paribus* clauses in laws of nature.

pose a problem, while approximation is doing so while solving it. As we shall see, the schemes of analytical mechanics involve, in this sense, idealizations—a major example being (Ideal), discussed in Section 1—but not approximations.

Second and more important, these schemes give information—indeed, an amazing amount of information—not just about solutions to individual problems, but about the structure of the *set* of solutions to all problems of a large class. Of course, the nature of this information, and of the large class, can only become clear when I expound the scheme in question, be it Lagrangian (in this paper) or Hamiltonian or Hamilton-Jacobi (in the companion paper). For they vary from one scheme to another. But for all three schemes, this information is independent of approximations.

For these reasons, I think the schemes give a sense of 'solve a problem', that lies in the middle of our spectrum—and is distinctive, because not a matter of approximations. Or rather: they give a group of senses, since the information the scheme provides varies both with the problem and with the scheme:—

(i): Variation from one problem to another is already clear. After all, despite the "pessimism" of Section 2.1.2's review of "rogue" functions: Happily, *some* problems can be solved in one of the stringent senses, e.g. the position of a particle being given at all times by an analytic formula. And as we shall see, the other problems are a mixed group: the information a scheme can provide varies. For example, some problems can be reduced to quadratures (cf. Paragraph 2.1.2.B); others cannot be.

(ii): Variation from one scheme to another. This variation will of course only be clear once the schemes are expounded. But broadly speaking, it is fair to say that for a given problem:

(a): The schemes agree about what kind of solution (stringent, or middle-of-the-spectrum) it has. After all, the schemes can hardly disagree about whether the position of a particle in a well-defined problem is given at all times by analytic formula—*nobody* can disagree about it!

(b): But the schemes can and do differ in the information they provide about the given problem. And this information is not always "just theoretical": it can bear very directly on how to solve the problem (to the extent that it can be solved). The obvious example is the way that Jacobi's invention of transformation theory (for Hamiltonian mechanics) enabled him to solve problems that had thitherto been intractable.

This variation across problems and schemes, points (i) and (ii), reinforces the important point that these schemes are by no means "algorithms" for solving problems. Indeed, they are not such algorithms, even in some single middle-of-the-spectrum sense of 'solve a problem', such as reduction to quadratures. If only such "middling" solutions were always possible—what a neat world it would be!

In particular, be warned:— In Section 3 onwards, one recurrent theme will be the schemes' allowance of arbitrary variables, so that we can adopt those variables best suited to the problem we face—i.e. yielding as stringent a solution to it as is possible. So that allowance will be a major advantage of the schemes. But it will *not* mean that the schemes tell us what are the best variables for our problem. If only!

It would be a good project to define more precisely various middle-of-the-spectrum senses of 'solution', and classify the various problems and schemes with respect to these senses. But I shall duck out of this project, and restrict myself to laying out the kinds of information the schemes provide. In effect this sort of information would be the raw material to use for making such definitions and ensuing classification.

We will see in Sections 3 and 4 that the Lagrangian scheme has the following four merits—as do the other two schemes, Hamiltonian and Hamilton-Jacobi. It will be convenient to have mnemonic labels for these merits, just as it was for the morals.

(Fewer): The use of fewer functions to describe the motion of a complicated system that the number of degrees of freedom suggests. Indeed, in each of the three schemes an arbitrarily complicated system is described by *just one* main function. (Their symbols are respectively L, H and S: so this paper will be concerned with L, where 'L' stands for 'Lagrangian'.)

(Wider): A treatment of a wider class of problems. One main way this happens (in all three schemes) is that the scheme allows a choice of variables, to suit the problem at hand. (If there is a symmetry, or another way to separate (de-couple) variables, the best choice will almost always exploit it; see (Reduce) and (Separate) below.) But all choices of a certain wide class are equally legitimate: this will give a sense in which the general scheme's equations are covariant.[17]

(Reduce): The ability to reduce the number of variables in a problem. By this I do not mean the general idea of holding some variables negligible: whether as a matter of what I labelled approximation or of idealization. Nor do I mean the specific idealization labelled (Ideal) at the end of Section 1.2: i.e. treating infinite or "large-finite" systems as "small-finite".

Rather, I mean a specific kind of elimination of variables from the description of a problem, where the elimination is rigorously justified by the scheme in question. Two main sorts of example of such elimination will recur in this paper. Namely, elimination of:

(i) Variables that describe constraints, or the forces that maintain constraints. This will be prominent in Section 3.

(ii) Variables that describe different values of a constant of the motion. Recall from Section 2.1.3 that knowing the value $c \in \mathbb{R}$ of a constant of the motion f means we can analyse the motion wholly within the level set $f^{-1}(\{c\})$. As we shall see, constants of the motion typically arise from a *symmetry* of the system. This will be prominent in Section 4.4 onwards.

A simple "toy" example of all of (Fewer), (Wider) and (Reduce) is provided by one-dimensional conservative systems: by which is meant any system described by a

[17] The morals arising from allowance of arbitrary coordinates turn out to be very different from those arising from general covariance in spacetime theories. This is not surprising: after all, since the manifold we are concerned with is a state-space, not spacetime, at most one point of the manifold is "occupied" or "realized" at any one time.

differential equation, for a single real variable as a function of time $x(t)$, of the form

$$\ddot{x} = F(x) \ ; \quad F \text{ a differentiable function defined on a real interval.} \tag{2.21}$$

In mechanical terms, these are systems with one configurational degree of freedom, such as a point-particle moving frictionlessly in one spatial dimension.

It is easy to show that for any such system, the total energy E, the sum of the potential and kinetic energies T and V

$$E \ := \ T + V \ := \frac{1}{2}\,\dot{x}^2 \ - \ \int_{x_0}^{x} F(\xi)\,d\xi \tag{2.22}$$

is a constant of the motion. And this implies that the motion of the system can be explicitly solved in the sense of being reduced to a single integration; (though we may be able to do the integration only numerically: 'quadrature'). For by solving eq. 2.22 for $\dot{x}$, the problem of integrating the second-order eq. 2.21 is reduced to integrating

$$\dot{x} = \pm\sqrt{\{2E - V(x)\}} \ . \tag{2.23}$$

This has the solution (cf. Paragraph 2.1.2.B's first example of quadrature, eq. 2.3)

$$\pm \int \frac{dx}{\sqrt{\{2E - V(x)\}}} \ = \ \int dt \equiv t \ ; \tag{2.24}$$

which we then invert so as to give x as a function of t.

To spell out the illustration of the three merits:— The conservation of energy reduces the problem from two dimensions (variables)—from needing to both know x and $\dot{x}$—to one: (Reduce). A single function V describes the system: (Fewer). And our method solves, in the sense of quadrature, an entire class of problems: (Wider).

(Separate): The ability to change to variables that are "de-coupled", in that one has to solve, either:

(a) ideally, independent rather than coupled equations; or

(b) much more commonly, equations that are coupled less strongly (at least not pairwise!) to one another than was the originally given set.

This merit often occurs in association with (Reduce). Reducing a problem from n variables to $n-1$ will typically leave us, after we solve the $(n-1)$-variable problem, having to find the nth variable, q_n say, from a single equation $\frac{dq_n}{dt} = f(q_1, \ldots, q_{n-1})$ where the right-hand side gives $\frac{dq_n}{dt}$ as a function of the other variables, which are now themselves given as functions of time. So q_n can be found by quadrature.

More specifically, it also occurs in the method of separation of variables. Paragraph 2.1.2.B's example (ii) gave the simplest example of this method; but it will come to the fore in Hamilton-Jacobi theory.

I submit that this is an impressive list of merits, especially since all three schemes have all of them. In any case, it fills out my claim that analytical mechanics is devoted to providing general schemes for representing and solving mechanical problems.

In Section 2.1's opening statement of my main moral about method, (Scheme), I also claimed that the provision of such schemes is a significant topic in the analysis of scientific theories—not least because it falls between two topics often emphasised by philosophers, "laws of nature" and "models".

Assessing my claim must largely rest with the reader. But I think there are two reasons why philosophers should take note of (Scheme)—apart from the simple fact that one science, viz. analytical mechanics, is mostly devoted to such schemes, and has succeeded most impressively. These reasons concern how the development of schemes for treating "any" problem, is ignored in the philosophical literature; (or so it seems to me). I think this happens in two ways. The first relates to observations; the second relates to theory, and will be taken up in the next moral.

As to observations: the literature emphasizes the "opposite" idea. That is, it emphasizes what it takes to account for, or give a model of, given observations (or a given phenomenon): and in particular, the nature of the approximations involved. Indeed, one influential vein of literature emphasizes the limitations of theory. Namely, it stresses that one needs to choose a model that one can solve in some strong sense (as people say: exactly, or nearly exactly), and then exploit it as much as possible to deal with other problems. (Here 'model' and 'solve' have various senses, for various examples and various authors: for example, contrast Kuhn's emphasis on exemplars in the Postscript (1970) of his (1962), with Cartwright (1999).) I agree this happens often, maybe "all the time", in science.[18] But—my moral again—we should also note the "opposite", i.e. schemes for treating any problem in a large class.

My second reason for noting (Scheme), relating to theory rather than observations, is covered in my second, minor, moral about method.

2.1.5 Reformulating and Restricting a Theory: (Reformulate) and (Restrict)

Most of the philosophical literature conceives the development of a theory as a matter of increasing the theory's logical strength (information content). The two main ways in which this can happen are taken to be: deepening the theory's account of the phenomena in its domain (especially by invoking a more detailed causal and/or microstructural account); and extending the theory to new phenomena, outside its domain. Again, I agree this happens often, and maybe "all the time". But one should also notice the "opposite" idea: theory development without an increase in logical strength. There are

[18]But I think this literature also misses a point about the frequent exploitation of a single model: namely that there are sometimes good theoretical reasons for the selection of the preferred model, and for the success of its application to other problems. The obvious example in physics concerns analysing problems in terms of harmonic oscillators. Not only is $V \propto x^2$ the simplest polynomial way to specify a spatially varying force; also, Taylor's theorem implies that locally it provides the dominant contribution to a generic smoothly spatially varying force. Of course, such considerations are the springboard for a wealth of theory: the obvious example is the analysis of small oscillations about equilibrium; and catastrophe theory (Butterfield 2004b) provides a very advanced example.

two main cases to consider. The new formulation might be equivalent (theoretically, not just empirically) to the old; I call it (Reformulate). Or it might be logically weaker; I call it (Restrict).

Agreed: the first case, (Reformulate), is old news. That is, it is not controversial that providing equivalent formulations of a theory can be very significant, both methodologically and ontologically. For one of a pair of theoretically equivalent formulations might extend better than the other to another domain of phenomena; or deal better, in some sense, with the given domain. This can even be so when theoretical equivalence is construed strongly enough that theoretical equivalence implies, or near enough implies, that the two formulations have the same ontology.[19]

But analytical mechanics provides several examples of equivalent formulations, for which this old news is worth reading; for three reasons. The first reason relates back to (Scheme): one of the equivalent formulations might have the specific methodological advantage of providing such a scheme. Second, as to ontology: such equivalent formulations help rebut the false idea that classical mechanics gives us a single matter-in-motion picture. Third, these equivalences are subtler than is suggested by textbook impressions, and folklore slogans like 'Lagrangian and Newtonian mechanics are equivalent'.

The second case, (Restrict), at first seems paradoxical: how can one develop a theory by restricting it, i.e. by decreasing its logical strength? My answer to this again relates to (Scheme), but I can state it in general terms. For it reflects the usual trade-off in scientific enquiry between the aims of covering (i.e. describing and explaining) a wide domain of phenomena, and covering phenomena in detail—between width and depth, as people say. So imagine a case where a theory admits, for just a subset of its domain of phenomena, a formulation which offers, for just that subset, advantages of "depth". In such a case, it could be best to restrict one's attention to the subset, and pursue just the special formulation.

Analytical mechanics provides several major examples of this; we will see some as early as Sections 3.1 and 3.2. Besides, in analytical mechanics the advantage of "depth" offered by the special formulation is *not* a more detailed causal and/or microstructural account, or even the possibility of adding such an account. It is rather the provision of a general scheme for solving problems; (or adding further merits to a given scheme). So in these examples, (Restrict) is not simply a preliminary to adding logical strength. Furthermore, in these examples, the special formulation and the original general theory are often logically equivalent, as regards what they say about the given subset of phenomena. So in this way, (Restrict) and (Reformulate) will often be exemplified

[19]Here are two examples from non-relativistic quantum mechanics of a fixed number of particles: (i) the Schrödinger and Heisenberg pictures; (ii) the wave-mechanical and path-integral formulations of the position representation. The members of each pair are provably equivalent; and though the ontology of quantum mechanics is a murky business, I think no one sees a relevant difference between the members of a pair. But when we turn to relativistic quantum theories, in particular quantum field theory, each pair's symmetry is broken: in various ways, the Heisenberg picture and path-integral formulation "win".

together.

2.2 Ontology

In Section 1, I stressed that the ontology of classical mechanics is a subtler affair than the matter-in-motion picture suggests. This general point will be borne out in two morals, which concern respectively: modality and objects.

2.2.1 Grades of modal involvement: (Modality)

As I said at the start of this Section, the starting-point of each of the schemes—Lagrangian, Hamiltonian and Hamilton-Jacobi—is to postulate the *state-space*: the set of all possible states of the system it is concerned with; (though the structure of this set varies between the different schemes).

At first sight, the philosophical import of this would seem to be at most some uncontroversial version of the idea that laws support counterfactuals. That is: whether or not one believes in a firm distinction between laws of nature and accidental generalizations, and whatever one's preferred account of counterfactuals, a theory (or "model") that states 'All As are Bs' surely in some sense warrants counterfactuals like 'If any object were an A, it would be a B'. And so when analytic mechanics postulates state-space and then specifies e.g. laws of motion on it, it seems at first that this just corresponds to the passage from 'All actual systems of this kind (having such-and-such initial states—usually a "small" proper subset of state-space) evolve thus-and-so' to 'If any system of this kind were in any of its possible initial states, it would evolve thus-and-so'.[20]

But this first impression is deceptive. The structures with which state-space is equipped by analytical mechanics, and the constructions in which it is involved, make for a much more varied and nuanced involvement with modality than is suggested by just the idea that laws support counterfactuals. This is my third moral, which I call (Modality).

I propose to delineate, in Quinean fashion, three grades of modal involvement; so I shall write (Modality;1st) etc. Like Quine's three grades, the first is intuitively the mildest grade, and the third the strongest. But this order will not correspond to any ordering of the three schemes, Lagrangian, Hamiltonian and Hamilton-Jacobi. In particular, this paper's scheme, the Lagrangian one, was historically the first, and is in various respects the most elementary, of the three: but it exhibits the *third* grade of modal involvement.

[20]Here, and in all that follows, I of course set aside the (apparent!) fact that the actual world is quantum, not classical; so that I can talk about e.g. an actual system obeying Hamilton's Principle. Since my business throughout is the philosophy of classical mechanics, it is unnecessary to encumber my argument, from time to time, with antecedents like 'If the world were not quantum': I leave you to take them in your stride. Cf. also footnote 10 in Section 2.1.1.

The grades are defined in terms of which kind of actual matters of fact they allow to vary counterfactually. One kind is the given initial conditions, and/or final and/or boundary conditions; roughly speaking, this is the given initial state of the system. Another kind is the given physical *problem*: which I here take as specified by a number of degrees of freedom, and a Lagrangian or Hamiltonian. (In elementary terms, this means: specified by the number of particles involved, and the forces between them.) A third kind is the laws of motion, e.g. as specified by Newton's or Hamilton's equations. Thus I propose the following grades.[21]

(Modality;1st): The first i.e. mildest grade keeps fixed the given actual physical problem and laws of motion. But it considers different initial conditions, and/or final and/or boundary conditions, than the actual given ones; roughly speaking, different initial states of the system. And so it also considers counterfactual histories of the system. (Under determinism, a different initial state implies a different history, i.e. trajectory in state-space.)

So this grade includes the idea above, that laws support counterfactuals. But it will also include subtler modal involvements. Perhaps the most striking case occurs in Hamilton-Jacobi theory. For details, cf. Butterfield (2004d: Sec. 4, 2004e: Sec. 4) or the companion paper. But in short: one solves a problem, as it might be an actual one, by introducing an ensemble of systems, i.e. a set of possible systems, of which the actual system is just one member. Furthermore, the ensemble can be chosen in such a way that the problem is solved *without* performing integrations, i.e. just by differentiation and elimination: a remarkable—one might well say 'amazing'—technique.

(Modality;2nd): The second grade keeps fixed the laws of motion, but considers different problems than the actual one (and thereby in general, different initial states). Such cases include considering a counterfactual number of degrees of freedom, or a counterfactual potential function. Maybe no actual system is, nor even is well modelled as, a Lagrangian system with 5,217 coordinates; and maybe no actual system has a potential given (in certain units) by the polynomial $13x^7 + 5x^3 + 42$. But analytical mechanics (in all three schemes) considers such counterfactual cases. And one can have good reason to do so, the obvious reason being that the counterfactual case provides an idealization or approximation needed to get understanding of an actual system.

However, this second grade also includes more ambitious cases of considering counterfactual problems: namely, cases where one makes a generalization about a whole class of problems. An elementary example is the conservation of energy theorem, which we will see in Lagrangian mechanics.[22]

[21] I don't claim that these three grades are the best way to classify the modal involvements of analytical mechanics. But they have the merit of being obvious, and of showing clearly the variety of modal involvement that occurs. I also think:

(i): the grades could be sub-divided in various ways, for example using Section 3.1.1's classification of kinds of constraints (or finer classifications made in the literature);

(ii): similar grades can be discerned in other physical theories.

But I shall not develop (i) or (ii) here.

[22] Again: an advanced, indeed spectacular, example is the classification of catastrophes by catastro-

(Modality;3rd): The third grade allows different laws, even for a given problem. Again, this can happen even in Lagrangian mechanics. Here one does not explicitly formulate non-actual laws (much less calculate with them). Instead, one states the actual law as a condition that compares the actual history of the system with counterfactual histories of it that do not obey the law (in philosophers' jargon: are *contralegal*). That is, the counterfactual histories share the initial (and final) conditions, but do not obey the given deterministic laws of motion, with the given forces. (Agreed, for any sufficiently smoothly varying counterfactual history, there *could* be forces which in conjunction with the actual laws and initial and final conditions, would yield the counterfactual history. But this does not matter, in the sense that it is not appealed to in the formulation of the actual law.)

This is at first sight surprising, even mysterious. How can it be possible to state the actual law by a comparison of the actual history with possible histories that do not obey it? Besides, metaphysicians will recognize that this seems to contradict a Humean view of laws, and in particular Lewis' doctrine of Humean supervenience (Lewis 1986, pp.ix-x). I address this issue at length elsewhere (2004e: Section 5). For this paper, it suffices to note (and thereby reassure Humeans) that this third grade of modal involvement can be reconciled with Humeanism.

To sum up this moral: the detail of analytical mechanics reveals it to have a varied and nuanced involvement with modality.

2.2.2 Accepting Variety: (Accept)

My second moral is about ontology in a more obvious way than was (Modality): it is about what are the basic objects of analytical mechanics. In general terms, it is that this is a subtler and more varied affair than suggested by the matter-in-motion picture.[23] More specifically, the ontology need not be "just point-particles". I shall develop this moral in three more specific points.

2.2.2.A Infinite as finite, and conversely First, analytical mechanics is much more flexible about its basic ontology than one might think—especially if one thinks of classical mechanics as requiring *au fond* point-particles. In particular, each of my three schemes can treat both finite and infinite systems. (By this I mean, respectively, systems with a finite, or infinite, number of degrees of freedom.) Furthermore, each scheme

phe theory.

[23]This general idea is already suggested by the moral (Reformulate) of Section 2.1.5. For given that the world is in fact quantum, we can only interpret a phrase like 'the basic objects of analytical mechanics' as about the objects postulated by analytical mechanics. And we already know from (Reformulate) that different approaches or theories within analytical mechanics might have heuristic, and even ontological, differences (and might do so, even if they are in some sense theoretically equivalent). So unless some single approach or theory is favoured, we already expect that analytical mechanics might be pluralist about ontology.

can, starting with a system given to it as finite/infinite, treat it as infinite/finite—and under appropriate circumstances, can justify doing so.

This point returns us to (Ideal), discussed at the end of Section 1. I said there that analytical mechanics typically describes bulk matter (bodies) in terms of a *small* finite number of variables (degrees of freedom). But this idealization can be justified, both empirically, and theoretically—by proving theorems to the effect that collective variables of many-dimensional systems (i.e. systems with many, even infinitely many, degrees of freedom) would behave as described by a low-dimensional analysis. So here we see analytical mechanics treating an infinite or "large-finite" system as "small-finite"—and justifying its doing so.

But the "converse" can also happen. Analytical mechanics has (on all three approaches) a rigorous formalism for describing infinite systems (aka: continuous systems); though as mentioned in Section 1, I'll say nothing about these formalisms. But the use of such a formalism does not commit one to the described system being really infinite. When for example, analytical mechanics describes a string or a gas as a continuous system (so as to describe e.g. sound waves in it), it is not committed to the string or gas really having infinitely many degrees of freedom. For a finite system can have so many degrees of freedom as to justify a model that treats it as continuous. For example, one can treat the density or pressure in a gas as a continuous function of spatial position, but take this function to represent, in an idealized way, an average over a macroscopically small volume, of very many microscopic degrees of freedom. And again, the justification for this kind of treatment can be both empirical and theoretical. (Continuous models of discrete phenomena are of course not special to analytical mechanics: they are endemic in physics.)

To sum up: when analytical mechanics successfully describes a system as finite/infinite, the system could really (i.e. in a classical world!) be the "opposite", i.e infinite/finite: a salutary lesson in flexibility, and an antidote to the matter-in-motion picture (whether it takes matter as continuous bodies or as point-particles).

2.2.2.B Beware micro-reductionism Second, analytical mechanics is less micro-reductionist than one might think—especially if one focusses on the matter-in-motion picture. In particular, if a given approach to analytical mechanics is applied to a problem or range of problems, and some objects (or quantities i.e. variables) are defined from the fundamental objects etc. postulated by the approach, then we should accept the derived objects etc. as no less real than the fundamental ones.

Here, I say 'accept as no less real than the fundamental objects', rather than 'accept as real, like the fundamental objects', in order not to presuppose, nor favour, some version of scientific realism—either in general or about analytical mechanics. Like most others including anti-realists like van Fraassen, I take interpreting a physical theory to be a matter of describing what the world would be like if it were true: an endeavour that makes sense even if as an anti-realist, one is agnostic, or even atheistic, about theories' assertions about unobservables.

I admit that this point raises issues about reductionism etc. which I cannot address here.[24] Suffice it to make two remarks in defence of the point.

(a): Since it concerns *defined* objects and quantities, it is surely plausible: why deny reality to what is defined in terms of the real?

(b): This point is of course related to Paragraph 2.2.2.A above. For many if not most examples of defined quantities are collective variables, typically of infinite or "large-finite" systems. And as in Paragraph 2.2.2.A, there are two main cases: the defined variable might be used in a "small-finite" description of the system; or it might be used in a continuous description (for example, taking a continuous function of spatial position to represent a macroscopically local average of very many microscopic degrees of freedom). So for collective variables as examples of defined quantities, my second point is in effect an application of my first to the topic of reductionism.

Here is an example combining these points. Consider a circular wave spreading on a still pond. An analytical mechanical treatment will no doubt identify a few variables, such as the radius and height of the wave as relevant; and so describe the crest as propagating radially, though no water molecules do (except briefly). But apart from these few variables, analytical mechanics can treat the problem in three main ways. Either it uses just these few variables; or it takes the water to be composed of a vast number of particles; or it takes the water to be a continuum:(Paragraph 2.2.2.A). According to the last two ways, the few selected variables are collective variables of a very complex system; but they are no less real than they are on the first treatment: (this Paragraph).

To put these points in a slogan: we should accept that analytical mechanics suggests a varied ontology. Hence my label, (Accept). This slogan will be sharpened in the next Paragraph. Though it mostly concerns infinite systems i.e. continua, while my subsequent Sections are confined to finite systems, it is worth expounding it here—not least because it exposes a common "micro-reductionist" error.

2.2.2.C Beware the particles-in-motion picture One might reply to Paragraphs 2.2.2.A and 2.2.2.B that nevertheless point-particles are the "basic micro-ontology" of analytical mechanics. For when analytical mechanics conceives a body or a fluid as a continuum, it thereby postulates point-sized "bits of stuff", "cheek by jowl" with one another. Agreed, these are not point-particles in the sense of mass-points separated from each other by a void; (the sense usually associated with Boscovitch—though in fact the notion was introduced two decades earlier by Euler). But they are close cousins, and deserve the name 'point-particle'. For each has a definite position and so also its time-derivatives (velocity, acceleration etc.), mass-density—and other properties, such as pressure, depending on the details of the continuum being treated.

[24]Section 2 of Butterfield and Isham (1999) is a brief statement of my general views. It stresses how widespread explicit definability is within physics. Wilson stresses such definability in analytical mechanics, and puts it to work in a critique of currently popular doctrines in the metaphysics of mind and of properties; (1985: 230-238; 1993: 75-80).

I reply: fair comment. I am willing to agree that point-particles, in this weak sense including point-sized bits of matter in a continuum as well as Boscovitchean point-particles, are the "basic micro-ontology" of analytical mechanics. Accordingly, I will from Section 3 onwards adopt the common habit of saying 'particle' to mean, according to context, either:

(i) a point-particle (Boscovitchean or not); or following (Ideal),

(ii): a small solid (maybe rigid) body, which is assigned just one position vector $\mathbf{r}$, i.e. treated as having no internal structure or orientation.

But beware! Point-particles being the basic objects does *not* mean that analytical mechanics should be, or even can be, understood in a particle-by-particle way. And in fact, it cannot be. Here, we meet what Section 1.1 called the second error in the idea that classical mechanics is unproblematic: what I call the 'particles-in-motion picture'. This picture claims that analytical mechanics can and should be understood in a particle-by-particle way: i.e. that analytical mechanics not only takes matter to be composed of point-particles (in the above weak sense), but also analyses all the physics of matter's behaviour in terms of particle-to-particle relations.

It is this second claim that is false. Agreed, some parts of analytical mechanics conform to it. The main example is of course the analytical mechanics of point-particles in the stricter i.e. Boscovitchean sense, with action-at-a-distance forces. Here the main illustration of the claim is that the total force on each particle is the sum of the forces exerted on it by each other particle. That is, all the fundamental forces are from one particle to another; in physics jargon, all interactions are two-body, not many-body. The standard example is of course Newtonian gravity.[25]

Agreed also, some parts of the analytical mechanics of continua conform to it. For example, the forms of the terms in the Lagrangian and Hamiltonian densities for continua are in many cases deduced by conceiving the continua as an infinite limit of a large finite assembly of point-particles. For example, one assumes that each point-particle in the assembly interacts only with its nearest neighbours, and does so by a quadratic potential (which might be modelled by a spring).[26]

[25]The way I have stated this illustration assumes that the component forces are just as real as the total force on a particle. Though I fully accept this assumption, it has been denied. No matter: the illustration could be restated in more cumbersome language so as to be independent of this controversy.

[26]This kind of deduction (which goes back to at least Green (1855) and Thomson (1863)) raises an interesting methodological and historical point. Namely, if the deduced continuum model is empirically successful, it is tempting to think the assumptions are not "merely heuristic", a ladder to be thrown away *à la* Wittgenstein, once the continuum treatment is written down. That would represent "too great a coincidence": we should expect a body or fluid, which is well modelled as a continuum with such Lagrangian and Hamiltonian densities, to consist in fact of a vast number of point-particles of some sort, that (to a good approximation) interact as assumed, e.g. with a nearest-neighbour quadratic potential. In other words (with less connotation of scientific realism—and of our acquaintance with quantum theory!): we should *interpret* analytical mechanics, when it successfully describes a body or fluid as a continuum with such densities, as idealizing in the way discussed in Paragraph 2.2.2.A—as taking the body or fluid to be a swarm of such interacting point-particles.

Indeed, this line of thought was pursued and contested in the nineteenth century debates about the foundations of classical mechanics; but I shall not go into this. In any case, it is only in many, not in

Nevertheless, the claim is false. In fact, the analytical mechanics of continua has to be formulated in terms of *spatially extended* regions and their properties and relations. Two examples will suffice: one is kinematical, the other dynamical.

(i): I mentioned above that a point-sized bit of stuff in a continuum (a point-particle, in the weaker sense) has a mass-density. At first sight, that seems to support the particles-in-motion picture: that the mass of a finite volume is the integral of the mass-density seems to be a case, albeit a very simple one, of "micro-reductionism" or the "supervenience of the global on the local". But in fact a rigourous formulation proceeds in the opposite direction. It takes as primitive the attribution of masses to finite volumes, and defines mass-density as a limit of ratios of mass to volume, as the volume tends to zero; (the details are taken over from measure theory). Besides this opposite procedure is necessary, in order to avoid various conundrums; (unsurprisingly, these conundrums are not really specific to mass—they have analogues in general measure theory.)

(ii): One cannot understand the forces operating in continua (whether solids or fluids) as particle-to-particle. In particular, returning to Section 1.1's example of two continuous bodies that touch, at a point or over a finite region: it is wrong to think each point-sized bit of matter exerts a force on some (maybe "nearby") particles, or on all other particles. Rather, one needs to consider, for each arbitrary finite (i.e. not infinitesimal) portion of matter: (a) a force exerted on its entirety by matter outside it, and (b) a force exerted at each point of its surface by matter outside it. (Agreed, this strategy, of describing the forces on all the countless overlapping extended sub-regions of a continuum, is highly "redundant", in the sense that each sub-region is described countless times, viz. as a part of the description of a larger region in which it is included. Nevertheless, analytical mechanics adopts—and needs to adopt—this strategy.)

To sum up: (i) and (ii) both show that in the mechanics of continua, one cannot analyse the physics wholly in terms of particle-to-particle relations: the particles-in-motion picture is false. Rather, one needs to take spatially extended regions as primitives.[27]

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all, cases that the Lagrangian or Hamiltonian densities are deduced by taking such an infinite limit.

[27] My 2004a gives a more detailed critique of the particles-in-motion picture; it also connects the picture to philosophical concerns about intrinsic properties, and Humean supervenience. My 2004 applies this critique to the philosophical debate about the nature of objects' persistence through time.

For more technical details about (i) and (ii), cf. e.g.: Truesdell (1991, Sections II.2, III.1, III.5); and for (ii), Marsden and Hughes (1983), Section A.2 and Chapter 2. Topic (ii) has a rich history. It was Euler who in 1775 first realized continuum mechanics' need for (a) and (b); but the road to general acceptance was long and complex. For a glimpse of this history (emphasising the relation to the principle of rigidification), cf. Casey (1991: especially 333, 362-369).

3 Analytical mechanics introduced

3.0 Difficulties of the vectorial approach to mechanics In expounding the analytical mechanics of finite-dimensional systems, one can of course take various routes. In this Section, my route will be based on those of Goldstein et al. (2002) and Lanczos (1986).

First of all, I will motivate analytical mechanics by considering two difficulties faced by the more elementary approach to mechanics familiar from high school (mentioned in Section 1.1). Roughly speaking, this approach takes the motion of each body to be determined by the vector sum of all forces on it. More precisely, a body is taken to be either:

(i) a Boscovitchean point-particle; or

(ii) (following (Ideal), the idealization discussed at the end of Section 1): a body which is small and rigid enough to be assigned just one position vector $\mathbf{r}$, i.e. to be treated as having no internal structure or orientation; or

(iii) composed of particles in sense (i) or (ii).

I shall from now on use 'particle' in this way, i.e. to mean (i) or (ii). Then according to this approach, the motion of each particle is to be determined by the vector sum of all forces on it. So to determine the motion of a system of particles labelled by i is in principle to solve

$$m_i\ddot{\mathbf{r}}_i = \mathbf{F}_i = \Sigma_j \mathbf{F}_{ij} + \mathbf{F}_i^{(e)} \tag{3.1}$$

where $\mathbf{F}_{ij}$ is the force on particle i due to particle j, and $\mathbf{F}_i^{(e)}$ is the external force on particle i. (Here I will wholly set aside the topic, familiar in philosophy of physics, of the need to identify inertial frames with respect to which the quantities in eq. 3.1 are to be measured.) This is often called the *vectorial approach* to mechanics.[28]

In general, solving eq. 3.1 is so complex as to be utterly intractable, since there are countless particles in any macroscopic body. But the vectorial approach has two tactics, one empirical and one theoretical, for simplifying the problem. They are both aspects of (Ideal).

(i): It is often empirically adequate to treat the body in question as a particle (e.g. a bob on a pendulum), or as a small set of particles; perhaps with other simplifications, such as all the forces acting at the body's centre of mass.

(ii): Some problems allow an assumption about, and-or an analysis of, the many inter-particle forces, that greatly simplifies the problem. The simplification may even allow the problem to be solved in some "medium sense" such as quadrature. The standard example is the rigid body: conceived as composed of particles, it is defined as having constant inter-particle distances. The configuration of the body in space can

[28] It is also called 'Newtonian'; eq. 3.1, or '$\mathbf{F} = m\mathbf{a}$', being the most familiar form of Newton's second law. But this name is anachronistic. In particular: (i) it was Euler in 1749 who first wrote down $\mathbf{F} = m\frac{d^2\mathbf{r}}{dt^2}$, though in cartesian coordinates rather than vector notation (which was developed only in the nineteenth century e.g. by Heaviside and Gibbs (Crowe 1985)); (ii) it was Boscovitch (1758) who advocated an ontology of point-particles moving in a void.

then be specified by just six numbers.[29] And other assumptions may further simplify the problem. For example, the assumption that internal forces lie along the lines between particles implies that internal forces do no work; which further implies that if these forces are derived from a potential, then the internal potential energy is constant.

But these tactics are of limited value when one considers *constrained* systems, i.e. systems that are required to be placed, or to move, in certain limited ways. For constraints lead to two difficulties, which tactics (i) and (ii) cannot in general overcome—and which analytical mechanics, in particular Lagrangian mechanics, *does* overcome. Namely:—

(Dependent): Constraints imply that the $\mathbf{r}_i$ are not *independent*: i.e. one cannot in imagination vary each of them while leaving all the others fixed. (This implication holds good, whether $\mathbf{r}_i$ represents the position of a body, using (Ideal), or the position of a point-particle.) Note that independence and dependence of the $\mathbf{r}_i$ is a modal notion: I shall return to this in Section 3.1.

(Unknown): In general, the forces that maintain the constraints (*forces of constraint*; aka: forces of reaction) are not known. Thus we may suppose that we know what are often called the *applied forces* (aka: impressed, or given, or active, forces) on each particle: forces like gravity, spring forces etc. that apply to the parts of the system whether constrained or not. (So 'applied' does not mean 'external': the source of an applied force can be internal to the system.) Even so, we will in general not know the constraint forces on the system, e.g. from the surface on which it rests and across which it is constrained to move. And so the use of eq. 3.1 is forestalled.[30]

Lagrangian mechanics overcomes these difficulties; (as do Hamiltonian and Hamilton-Jacobi mechanics). In short: for (Dependent), one eliminates variables so as to work with a smaller set of variables which *are* independent. As for (Unknown): under remarkably general conditions, one can solve problems without knowing the constraint forces! Furthermore, after solving the problem, one can then calculate the constraint forces: in effect, they have the values they need to have so as to maintain the constraints.

So for both difficulties, Lagrangian mechanics will illustrate the strategy of reducing the number of variables that describe a problem—i.e. the merit (Reduce).

Section 3.1 will describe how to overcome (Dependent). Section 3.2 will begin on the topic of how to overcome (Unknown): it describes the principle of virtual work and Lagrange's method of multipliers. The last Subsection, Section 3.3, introduces

[29] Here is an argument for the number six (which works even if the body is continuous). (1): Rigidity implies that the positions of all the particles are fixed once we fix the position of just three of them. For imagine: if the tips of three of your fingers were placed at three given positions within a rigid brick, and someone specified the exact positions of the finger-tips, then they would have implicitly specified the positions of all the brick's constituent parts. (2): the position of three particles given as forming a certain triangle can be specified by six numbers.

[30] This point is not affected by the vagueness of my distinction between applied and constraint forces. For if one wishes, one can *define* the applied forces as the known ones (e.g. Desloge 1982: 528), so that it is merely usual that unknown forces maintain constraints.

d'Alembert's principle, and from that deduces Lagrange's equations. These lie at the centre of Lagrangian mechanics, which is further developed in Section 4.

Here I should stress a qualification about this order of exposition: a qualification which will become clearer in Section 3.3. To get to Lagrange's equations, it is *not* necessary to proceed as I do, *via* the principle of virtual work and d'Alembert's principle. One could go "straight" from eq. 3.1 to Lagrange's equations; and some fine expositions do. But as John Bell said, about how to teach special relativity: 'the longer road sometimes gives more familiarity with the country' (1987: 77).

We will see all four of my morals illustrated in this Section. I already mentioned the merit (Reduce). By and large, the minor morals, (Reformulate), (Restrict) and (Accept) will be a bit more prominent than the main ones, (Scheme) and (Modality). But these two will come to dominate in Section 4 onwards.

3.1 Configuration space

The key to overcoming the first difficulty, (Dependent), lies in what can claim to be Lagrangian mechanics' leading idea: configuration space. It will be clearest to first describe how this idea addresses (Dependent) (Section 3.1.1), and then turn to describing dynamics in terms of configuration space (Section 3.1.2).

3.1.1 Constraints and generalized coordinates

The idea is to represent the configuration of the entire system by a point in a higher-dimensional space. This is the basic version of the idea of state-space, which, as discussed in Section 2, underlies all my morals and all three schemes of analytical mechanics. Indeed, one might expect this, simply in view of determinism—which implies that for a given system and given forces on it,"all problems" are fixed by all initial conditions, and so by the state-space: (Modality;1st). But we can already be more specific.

Namely, we can state two important advantages of the idea of state space.

(i): Such a space can be described using many different coordinate systems (in ways analysed in detail in differential geometry). This yields a striking generalization of the idea of changing variables the better to solve a problem. Stated thus, the idea is endemic to science and hardly remarkable. But allowing arbitrary coordinates on a state-space suggests that one develop a scheme that encompasses all choices of variables, with a view to obtaining the most tractable representation (if one is lucky: a solution, at least in Section 2.1.1's "medium sense") of any problem: the merit (Wider).

(ii) Using a state-space allows us to relate dynamics to the geometry of higher-dimensional spaces, in particular their curvature. (These two advantages were first emphasized by Jacobi.)

Returning to the difficulty (Dependent), the main ingredient for overcoming it is the idea of allowing arbitrary coordinates on configuration space, i.e. (i) above. To

spell this out, I need to first state two independent distinctions between constraints. I will also need these distinctions throughout what follows; in fact most of my discussion will concern the first half of each distinction.

Holonomic constraints are expressible in the form of say k equations

$$f_j(\mathbf{r}_1, \mathbf{r}_2, \ldots, t) = 0; \text{ where } j = 1, 2, \ldots k \tag{3.2}$$

governing the coordinates $\mathbf{r}_i$. The obvious example is any rigid body: the constraints are expressed by the equations stating the fixed inter-particle distances. (The term "holonomic" is due to Hertz.) On the other hand, non-holonomic constraints are not thus expressible; though they might be expressible by inequalities, or by equations governing differentials of coordinates. For example:–

(i): confinement of a particle to one side of a surface (e.g. one particle confined to the region beyond a sphere of radius a centred at the origin—expressed by the inequality $\mid \mathbf{r}_1 \mid^2 > a^2$);

(ii): a rigid body rolling without slipping on a surface; the condition of rolling is a condition on the velocities (i.e. that the point of contact is stationary), a differential condition which can be given an integrated form only once the problem is solved.

Scleronomous (respectively: *rheonomous)* constraints are independent of (dependent on) time. (The terminology is due to Boltzmann: "scleronomic" and "rheonomic" are also used.) So the time-argument in eq. 3.2 (and in corresponding equations for non-holonomic constraints) is to allow for rheonomous constraints.

As to overcoming the difficulty (Dependent), the situation is clearest for holonomic constraints. The idea is to use our free choice of coordinate systems on configuration space so as to describe the system by appropriate independent variables, called *generalized coordinates*. So consider a system of N particles (i.e. point-particles or bodies small and rigid enough to be described by a single position vector $\mathbf{r}$), with k holonomic constraints. The system's configuration $(\mathbf{r}_1, \mathbf{r}_2, \ldots, \mathbf{r}_N)$ is at each time confined to a hypersurface in $\mathbb{R}^{3N}$ which will in general be $(3N - k)$-dimensional. (Rheonomous constraints just mean that the hypersurface in question varies with time.)

More precisely: we say the k constraints eq. 3.2, $f_j = 0$ are *(functionally) independent* if at each point on the hypersurface, the $3N$-dimensional gradients ∇f_j (i.e. with cartesian coordinates $(\frac{\partial f_j}{\partial x_1}, \ldots, \frac{\partial f_j}{\partial z_N})$) are linearly independent: i.e. if at each point, the $k \times 3N$ matrix with entries $\frac{\partial f_j}{\partial x_1}, \ldots, \frac{\partial f_j}{\partial z_N}$ has maximal rank k.[31]

If the constraints are independent, then the hypersurface to which the system is confined (for rheonomous constraints: at a given time) is $(3N - k)$-dimensional, and there is a system of coordinates on $\mathbb{R}^{3N}$ in which the constraints become

$$q_{3N-k+1} = 0, \ \ldots, \ q_{3N} = 0 \ . \tag{3.3}$$

(These generalized coordinates need not have the dimension of position.) In other words, there are $3N - k$ independent variables $q_1, \ldots, q_{3N-k}$ coordinatizing the hyper-

[31] Cf. the definition of functional independence in Paragraph 2.1.3.A, (iii).

surface, such that (again allowing for rheonomous constraints)

$$\mathbf{r}_i = \mathbf{r}_i(q_1, \ldots, q_{3N-k}, t), \quad i = 1, 2, \ldots, N. \tag{3.4}$$

Such coordinates are said to be *adapted* to the constraints. I shall usually focus on the hypersurface, not the ambient $3N$-dimensional configuration space; and I shall write n for $3N - k$, and therefore write the generalized coordinates as $q_1, \ldots, q_n$. The n-dimensional space is sometimes called the *constraint surface* in the $3N$-dimensional space; 'configuration space' is used for either of these spaces.

Configuration space, and the way it underlies (Scheme), will be centre-stage in this Section and Section 4.

First of all, let me address two doubts which can be raised about this strategy of analysing the system wholly within the lower-dimensional constraint surface. (The second is more important in what follows.)

(A): In everyday problems the constraints are often non-holonomic; as in examples (i) and (ii) above. Here we see our first example of the moral (Restrict); and a main one to boot. In fact, many of the methods and results of analytical mechanics depend on the constraints, if any, being holonomic; and most of my discussion will assume this.

(B): Since forces of constraint are in fact finite, a constrained system is not rigorously confined to the constraint surface—it can depart slightly from it; (more precisely, its configuration can depart slightly).

There are three replies to this. The simplest and dullest is that the forces of constraint are often strong enough that this strategy is an empirically adequate idealization. (Incidentally, it is an idealization in Section 2.1.4's proposed usage of neglecting the negligible while posing, not while solving, the problem.)

A more interesting reply is that there are theorems proving that in the limit as the forces of constraint become infinitely strong, the system's dynamics in the full configuration space becomes as analytical mechanics describes it, on the constraint surface. Such theorems illustrate (Ideal) and (Accept). I will report such a theorem in Paragraph 3.3.2.A.

Finally, the most interesting reply is one of the triumphs of Lagrangian mechanics—and a prime example of the merit (Reduce). Namely, suppose we maintain—say on the strength of the first two replies—that the constraint equations hold, i.e. the system is confined to the constraint surface. Then: under some very general conditions, Lagrangian mechanics enables us to rigorously solve the mechanical problem, i.e. to find the generalized coordinates $q_1, \ldots, q_n$ as functions of time (at least in a "medium sense" such as quadrature), without ever knowing the forces of constraint![32] Furthermore, after solving the problem in this way, we can come back and calculate what the constraint forces were (as a function of time).

[32]To anticipate a little: it is sufficient for this ability that the constraints are holonomic and ideal, and the applied forces are monogenic.

3.1.2 Kinetic energy and work

In this Subsection, I describe how to represent on configuration space, notions which are close cousins of the two sides of Newton's second law—the $m\mathbf{a}$ representing a body's inertia and the $\mathbf{F}$ representing the force on it. For in analytical mechanics, these Newtonian notions are displaced as central concepts by these cousins: respectively, the kinetic energy, and the work done by the forces (which latter is in many cases the derivative of a certain function, the work function, i.e. the negative of the potential energy). Thus:–

(a): *The analogue of inertia*:– The kinetic energy $T := \Sigma_i \frac{1}{2} m_i \mathbf{v}_i^2$ defines a line-element (in modern jargon: a metric) in the $3N$-dimensional configuration space by

$$(ds)^2 \equiv ds^2 := 2Tdt^2 = \Sigma_i m_i \mathbf{v}_i^2 dt^2 = \Sigma_i m_i (dx_i^2 + dy_i^2 + dz_i^2) \ . \tag{3.5}$$

Incidentally, this implies that

$$T = \frac{1}{2} m \left(\frac{ds}{dt} \right)^2 \quad \text{with} \quad m = 1 \ ; \tag{3.6}$$

i.e. the system's kinetic energy is represented by the kinetic energy (relative to the new line-element) of a single particle of mass 1.

(b): *The analogue of force*:– To explain this, I need the idea, which will be crucial in all that follows, of a *virtual displacement*. This idea also provides a main example of the moral (Modality;1st). That is, it considers counterfactual states, but not counterfactual problems or laws—indeed, so far laws are not in play.

A virtual displacement, $\delta \mathbf{r}_i$, of particle i is defined as a possible displacement of i that is consistent with both the applied force $\mathbf{F}_i^a$, and the force of constraint $\mathbf{f}_i$, on particle i, at the given time. Here, it will be consistency with the constraints *at the given time* that matters. We similarly define a virtual displacement of the system as possible displacements of its particles that are jointly consistent with the constraints at the time. We will usually be concerned with arbitrarily small virtual displacements, often called 'infinitesimal'; and I will usually omit this word.

We can express this more precisely by supposing the constraints yield differential conditions on the coordinates. (*Warning*: This paragraph is more precise than I will need: it is not used later on.) If the constraints are holonomic, as in eq 3.2, we can differentiate to get the differential conditions; if the constraints are rheonomous, at least one of the conditions will be time-dependent. So if the conditions are, in terms of n generalized coordinates (and with k constraints),

$$\Sigma_j A_{ij}(q_1, \ldots, q_n, t) dq_j + A_{it}(q_1, \ldots, q_n, t) dt = 0 \ ; \quad i = 1, \ldots, k, \tag{3.7}$$

then a virtual displacement of the system at time t is defined to be any solution $(\delta q_1, \ldots, \delta q_n)$ of the k equations

$$\Sigma_j A_{ij}(q_1, \ldots, q_n, t) \delta q_j = 0 \ ; \quad i = 1, \ldots, k. \tag{3.8}$$

Obviously, virtual displacements need not be actual. But beware: the converse also fails: actual displacements need not be virtual, because the forces and constraints might vary with time (the rheonomous case), and virtual displacements must be consistent with the forces and constraints *at the given time*.[33] Note also that 'virtual' will be used in this sense, not just for displacements, but more generally. We distinguish between virtual and actual variations of any quantity; and use the respective notations (introduced by Lagrange), δ and d, for them. Also, I shall sometimes use δ, either

(i): for a small (not infinitesimal) variation; or

(ii): to indicate that the differential is not exact.

If the total applied force on particle i has cartesian components X_i, Y_i, Z_i, the total work done by this applied force in an infinitesimal virtual displacement can be written as a differential form in the cartesian coordinates, i.e. as a sum over the $3N$ cartesian coordinates

$$\delta w = \Sigma_{i=1}^{N} X_i \delta x_i + Y_i \delta y_i + Z_i \delta z_i; \tag{3.9}$$

or, transforming to generalized coordinates, say $q_1, \ldots, q_n$, as a differential form in the generalized coordinates

$$\delta w = \Sigma_{j=1}^{n} F_j \delta q_j \tag{3.10}$$

where the transformation eq. 3.4 determines the F_j in terms of the cartesian components of force X_i, Y_i, Z_i.

In general, δw is not integrable. But if it is, then its integral is called the *work function* U. Lanczos (1986, p. 30) calls applied forces of this kind *monogenic* ('monogenic' for 'single origin') as against *polygenic*. Goldstein et al (2002) follow him; and I shall also adopt these terms.[34]

In many of the simpler cases of monogenic forces, U is independent of both the time, and the generalized velocities $\dot{q}_i$, so that $U = U(q_1, \ldots, q_n)$. If so, we say the system is *conservative*; and we have

$$\Sigma_j F_j \delta q_j = \frac{\partial U}{\partial q_j} \delta q_j \tag{3.11}$$

so that by the mutual independence of the qs, we can set all but one of the δq_j equal to 0, and so infer

$$F_j = \frac{\partial U}{\partial q_j}. \tag{3.12}$$

[33] So there is no real conflict with the venerable principle of modal logic, that the actual is possible. But the point is important: it underpins the derivation of the conservation of energy from D'Alembert's principle, cf. Paragraph 3.3.1.A.

[34] But beware: Lanczos' text sometimes suggests 'monogenic' is also defined for constraint forces. Agreed, in statics at equilibrium, the total applied force is the negative of the total constraint force: so if the former are monogenic with work function U, one can also call the latter monogenic with "work function" $-U$. But in dynamics (Section 3.3), this correspondence breaks down: even with monogenic applied forces, most constraint forces will not be derivable from a work function. Thanks to Oliver Johns for this point.

Writing $V := -U$, we have $F_j = -\frac{\partial V}{\partial q_j}$ and we interpret V as a *potential energy*. This corresponds to the "cartesian definition" of conservative systems, viz. that the force on the ith particle $\mathbf{F}_i$ is derivable ($\forall i$) from a time-independent scalar function V on configuration space, i.e.

$$\mathbf{F}_i = -\nabla_i V \tag{3.13}$$

where the i indicates that the gradient is to be taken with particle i's coordinates. (Incidentally: Here we meet the prototypical case of an integrability condition. For each i, eq. 3.13, with given $\mathbf{F}_i$, has a solution V only if in the domain considered $\mathbf{F}$ is curl-free, i.e. $\nabla \wedge \mathbf{F} = 0$: or equivalently, any closed loop integral $\oint \mathbf{F}_i \cdot d\mathbf{s}$ vanishes.)

When we add to the assumption of conservativity (i.e. V depending only on the generalized coordinates, not on the time, nor on the generalized velocities), the assumption that constraints, if any, are scleronomous, we get the conservation of energy, i.e. the constancy in time of $T + V$. This can be deduced in various ways, e.g. from d'Alembert's principle in Section 3.3 below. But however the theorem is derived, its being a generalization over many problems means it illustrates (Modality;2nd).[35]

3.2 The Principle of Virtual Work

3.2.1 The principle introduced

I turn to the second difficulty, (Unknown), faced by vectorial mechanics at the start of this Section: that the forces that maintain the constraints ('forces of constraint', 'forces of reaction') are not known. So we ask: can we somehow formulate mechanics in such a way that we do not need to know the forces of constraint? In fact for many problems, we can: viz. problems in which these forces would do no work in a virtual displacement. Such constraints are called *ideal*. The prototypical case is a rigid body; where indeed the work done by the internal forces (with $\mathbf{F}_{ij}$ assumed to lie along the line between particles i and j) is zero. But this condition is quite common, even for non-holonomic constraints; though to be sure, it also excludes very many cases e.g. friction.

Restricting ourselves to ideal constraints (characterized, note, by a counterfactual!) is a crucial example of the moral (Restrict). It is even more important an example than my previous one (viz. analytical mechanics' frequent restriction to holonomic constraints). For most of analytical mechanics depends on this restriction. In particular, those of its great principles that I will discuss in this paper—the principle of virtual work, d'Alembert's principle, the principle of least action, and Hamilton's principle—

[35] Two remarks about the more general case where U depends on the velocities and time, so that $U = U(q_j, \dot{q}_j, t)$. (1) As we shall see, analytical mechanics and its variational principles apply in this case. (2) In particular, for scleronomous systems with monogenic forces—i.e. systems with a work-function U that has no explicit dependence on time, but may be dependent on velocities $U = U(q_j, \dot{q}_j)$—there is a conservation of energy theorem: $T + V$ is conserved but with V defined by $V := \Sigma_j \frac{\partial U}{\partial \dot{q}_j} \dot{q}_j - U$.

do so. Accordingly, I (like many authors) will often *not* repeat that constraints are being assumed to be ideal.

Besides, as envisaged at the end of Section 2.1.5, the principles are often closely related to each other, and in some cases equivalent under the assumption of ideal and/or holonomic constraints; so that they exemplify (Reformulate) as well as (Restrict).

The first of the principles governing such problems is the *principle of virtual work*. Again we need Section 3.1's idea of an (infinitesimal) virtual displacement (and so (Modality;1st)). For the principle of virtual work concerns the total work done in such a displacement in the special case of equilibrium.

So let us assume that the system is in *equilibrium*. This means that for each particle i the total force $\mathbf{F}_i$ on it vanishes. Then for any virtual displacement $\delta\mathbf{r}_i$, $\mathbf{F}_i \cdot \delta\mathbf{r}_i = 0$ and so, summing, $\Sigma_i \mathbf{F}_i \cdot \delta\mathbf{r}_i = 0$. Let us split $\mathbf{F}_i$ in to the applied force (impressed force) $\mathbf{F}_i^{(a)}$, and the force of constraint $\mathbf{f}_i$. And let us make our restricting assumption that the constraints are ideal, i.e. the virtual work of the $\mathbf{f}_i$ is 0. Then we have

$$\Sigma_i \, \mathbf{F}_i^{(a)} \cdot \delta\mathbf{r}_i = 0. \tag{3.14}$$

That is: a system is in equilibrium only if the total virtual work of all the applied forces vanishes.

Under certain conditions the converse of this statement also holds; as follows. First note that we cannot conclude from eq 3.14 that each $\mathbf{F}_i^{(a)} = 0$, because the $\delta\mathbf{r}_i$ are not linearly independent vectors—recall that we are considering virtual displacements. But it does represent a statement of orthogonality; as does the corresponding statement in generalized coordinates (cf. the transition from eq. 3.9 to eq. 3.10)

$$\Sigma_{j=1}^{n} F_j \delta q_j = 0, \tag{3.15}$$

which says that the "vector" of the F_j must be "orthogonal" to the surface of allowed variations in the coordinates q_j.

On the other hand, suppose the following two conditions hold.

(i): The coordinates —whether $\mathbf{r}_i$ or q_j—*are* indeed independent; i.e. in the case of q_j: the constraints are holonomic so that we work in their n-dimensional space, the constraint surface; *and*

(ii): the displacements are *reversible* in the sense that if δq_j is allowed by the constraints, so is $-\delta q_j$).

Then each $F_j = 0$. For only the zero vector can be orthogonal to all vectors. And so we have the converse of the above statement. That is, we have *the principle of virtual work*:

> A system (subject to our restrictions) is in equilibrium if and only if for any virtual displacement the total virtual work of all the applied forces vanishes. (It is of course the 'if' half of the principle that is substantive.)

(This is an example of what Section 2.1.5 envisaged: (Restrict) and (Reformulate) together.)

Note that if the applied forces are monogenic, the total virtual work of these forces is the variation of the work-function U. So in this case equilibrium means: $\delta U = -\delta V = 0$. So the topic of equilibrium with holonomic constraints leads to the topic of a function being stationary, subject to other functions taking prescribed values. And similarly, the topic of equilibrium with non-holonomic constraints leads to the topic of a function being stationary, subject to conditions other than prescribed value(s) of function(s)—conditions that might be expressed as equations relating some functions' values or functions' differentials. (Here, 'being stationary' means, as in elementary calculus, having a zero derivative: details below. But again as in calculus, our interest in stationary points of functions is often that they are extrema, i.e. maxima or minima. And so I will often talk of 'extremizing a function' etc., to avoid cumbersome phrases like 'find a point at which a function is stationary'—there is no word 'stationarize'!)

This is one of the several reasons for analytical mechanics' endemic use of the method of *Lagrange multipliers*, to analyse extremizations of a function subject to constraints. I finish this Subsection with a brief introduction to this method. It will lead us back to the topic of overcoming the difficulty (Unknown), that we do not know the forces of constraint.

3.2.2 Lagrange's undetermined multipliers

This method has two significant advantages over the obvious method of eliminating as many variables as there are constraint equations, and then using differential calculus to perform an unconstrained extremization in fewer variables.

(i): In many cases, there is no natural choice of variables to be eliminated: either because of the symmetrical, or nearly symmetrical, way that the variables occur; or because any choice makes for cumbersome algebra.

(ii): The Lagrange method is more powerful. It can handle constraints given by differential conditions (in mechanical terms: non-holonomic constraints); which the elimination method cannot.

Apart from its advantages (i) and (ii), it is also worth noting that:—

(a): The method is not confined to the context in which it is often met, viz. variational principles (where the function to be extremized is an integral).

(b): In mechanics, the method has a physical interpretation, which provides a way to calculate the forces that maintain the constraints; more details in Paragraph 3.2.2.B.

3.2.2.A Lagrange's method The idea is clearest in a visualizable elementary setting. Suppose we want to extremize $f(x, y, z)$ i.e. $f : \mathbb{R}^3 \to \mathbb{R}$ subject to two constraints $g_1(x, y, z) = 0$ and $g_2(x, y, z) = 0$. Generically, the constraint surfaces meet in a line, and at the solution point (x_0, y_0, z_0) the gradient ∇f must be orthogonal to the line's tangent vector $\mathbf{v}$; (otherwise f could be increased or decreased by a displacement along the line). But $\mathbf{v}$ lies in the intersection of the two tangent planes of the constraint surfaces, and these planes are normal to ∇g_1 and ∇g_2 respectively. So at the solution

point (x_0, y_0, z_0) the gradient ∇f must lie in the plane defined by ∇g_1 and ∇g_2, i.e. it must be a linear combination of ∇g_1 and ∇g_2;

$$\nabla f = \lambda_1 \nabla g_1 + \lambda_2 \nabla g_2. \tag{3.16}$$

This argument generalizes to higher dimensions (say n), and an arbitrary number (say m) of constraints. So using ∇ for the n-dimensional gradient,

$$\nabla f = \Sigma_j \lambda_j \nabla g_j. \tag{3.17}$$

We now put the argument (in higher dimensions) algebraically. We will use x_i not q_i, even though the argument makes no use of cartesian coordinates; this has the merit of indicating that the equations, eq. 3.16 and 3.17, and eq. 3.18 below, refer to the 'larger' configuration space, i.e. whose dimension, n say, exceeds the dimension of the constraint surface by m, where m is the number of constraints. We are to find the point $x_0 := (x_{0_1}, \ldots, x_{0_n})$ at which $\delta f = 0$ for small variations $x' - x_0$ such that $g_j(x') := g_j(x'_1, \ldots, x'_i, \ldots, x'_n) = 0$ for $j = 1, 2, \ldots, m$. So eq. 3.17 requires that there are λ_j such that defining $h(x) := f(x) + \Sigma_j \lambda_j g_j(x)$, we have:

$$\delta h = \delta f + \Sigma_j \lambda_j \delta g_j = 0 \quad \text{, i.e.} \quad \frac{\partial f}{\partial x_i} + \Sigma_j \lambda_j \frac{\partial g_j}{\partial x_i} = 0, \quad \forall i = 1, \ldots, n \tag{3.18}$$

We use these n equations, together with the m equations $g_j(x_i) = 0$ to find the $n + m$ unknowns (the n x_i and the m λ_j).

Thus the fundamental idea is to replace a constrained extremization in n variables subject to m constraints by an unconstrained extremization in $n + m$ variables. I will make three brief comments, (1)-(3), about further developments, before I turn to the physical interpretation.

(1): Thinking of the λ_j as variables, and so of h as a function of the $n+m$ variables (x_i, λ_j) we can ask that it be stationary. This variation problem gives eq. 3.18 again, if we vary with respect to x_i; and the constraint equations $g_j = 0$, if we vary with respect to λ_j. In short: Variation of the λ_j gives back the constraint equations *a posteriori*.

(2): Lagrange's method also applies to constraints expressed not by equations $g_j = 0$ but only by conditions on differentials, i.e. a set of equations

$$\delta g_j := G_{j1}\delta x_1 + \ldots + G_{jn}\delta x_n = 0; \quad j = 1, 2, \ldots, m \tag{3.19}$$

where the left-hand side uses the δ to indicate that it is not an exact differential, i.e. G_{ji} is not the ith partial derivative of a function g_j. Lagrange's method again applies and we get the condition

$$\delta f + \lambda_1 \delta g_1 + \ldots + \lambda_m \delta g_m = 0, \tag{3.20}$$

where we are to treat all the $x_i, i = 1, \ldots, n$ as independent variables; and into this equation the expressions for δg_j from eq. 3.19 can be substituted. (It is just that the left-hand side of eq. 3.19 is not the differential of a function g_j, as it was above).

(3): Lagrange's method (including the above two comments) also applies to the central idea of calculus of variations—the extremization of an integral, which we will meet in Section 4; (details in Section 4.3.1).

3.2.2.B Physical interpretation: the determination of the constraint forces
When Lagrange's multiplier method is applied to mechanics, it has a physical interpretation. The interpretation is easily seen for our present topic, equilibria for monogenic applied forces. As we have seen, for such forces, there is a V which, once added to some linear combination of constraints, is to be extremized. In short, the physical interpretation is that the Lagrange multipliers give the forces of constraint. I shall develop this interpretation only for the special case of holonomic constraints and constraint forces that are derivable from a potential (unusual though this is: cf. footnote 35). But the interpretation holds much more generally.

So suppose that the constraints are holonomic (as well as ideal), so that we are to extremize V subject to the constraints that all the $g_j = 0$, i.e. to extremize $V + \lambda_j g_j$. Suppose also that each force of constraint is "monogenic", i.e. is derivable from a potential energy. Then, two results follow. First, for each j, $\lambda_j g_j$ represents the potential energy of the jth force of constraint. Second, the fact that each λ_j is known only at the solution-point x_0 reflects our scanty knowledge about the forces of constraint. For in forming the gradient of the jth additional potential energy $\lambda_j g_j$, we get as the contribution F_{ji} to the i cartesian component of the force

$$F_{ji} := -\frac{\partial}{\partial x_i}(\lambda_j g_j) = -\lambda_j \frac{\partial g_j}{\partial x_i} - \frac{\partial \lambda_j}{\partial x_i} g_j \quad \text{(no summation on } j\text{)}; \tag{3.21}$$

at the solution-point x_0, g_j vanishes, i.e. $g_j(x_0) = 0$, so that at x_0

$$F_{ji} = -\lambda_j \frac{\partial g_j}{\partial x_i} \quad \text{(no summation on } j\text{)}. \tag{3.22}$$

Remarkably, this physical interpretation carries over to the case of non-holonomic (but ideal!) constraints, and to the case where the ideal constraint forces do not have a work-function, i.e. no potential energy $\lambda_j g_j$; (as Lanczos might say, the case of polygenic constraint forces). One proceeds as in comment (2) of Paragraph 3.2.2.A; the forces are again given by the λ-method. Furthermore, this physical interpretation carries over to the case of non-equilibrium, i.e. dynamics, for both holonomic and non-holonomic constraints. I will discuss this a little more in Sections 4.3.2 and 4.6; but for more details, cf. Desloge (1982: 532-534) and Johns (2005: Chapter 3.4).

This physical interpretation underpins the striking way in which Lagrangian mechanics enables one to solve problems without knowing the forces of constraint. Again, I will not go into details: not even in Section 3.3's discussion of dynamics—since there I use ideal constraints and d'Alembert's principle to reduce mechanical problems very directly to a description on the constraint surface which does not even *mention* the constraint forces: vividly illustrating the merit (Reduce).

But in short, the idea is that, similar to just above: the ith generalized component ($i = 1, \ldots, n$) of the jth constraint force ($j = 1, \ldots, m$) is $-\lambda_j \partial g_j / \partial q_i$. This means that knowing the constraint equations as functions of the generalized coordinates $g_j(q_1, \ldots, q_n) = 0$ is enough. For as in Paragraph 3.2.2.A, there are enough

equations to determine not just the system's motion $q_i(t)$, but also the λs, and thereby the forces of constraint.

So to sum up: under some widespread conditions, we can, after we solve the problem (i.e. find the motion of the system) without even knowing the constraint forces, go back and calculate the constraint forces. In effect, the idea of this calculation is that the constraint forces have whatever values they need to have so as to maintain the constraints on the previously calculated motion. In this way we can overcome the difficulty (Unknown) in the best possible way. (For details, cf. Desloge (1982: 545-546, 549-552, 555), Johns (2005: Chapter 3-5,3-8,3-11).)

3.3 D'Alembert's Principle and Lagrange's Equations

3.3.1 From D'Alembert to Lagrange

To sum up Section 3.2: it described how the principle of virtual work eliminates the force of constraint $\mathbf{f}_i$ on each particle i for the case of equilibrium (thus overcoming the difficulty (Unknown) for that case). The idea of D'Alembert's principle is to eliminate the $\mathbf{f}_i$ also for non-equilibrium situations, by the simple and ingenious device of treating the negative of the mass-acceleration, $-\dot{p}_i$, as a force; as follows.[36]

Newton's second law $\mathbf{F}_i = \dot{p}_i$ "reduces to statics" if we rearrange it as if there were a "reversed effective force" $-\dot{p}_i$; i.e. if we write

$$\Sigma_i \left(\mathbf{F}_i - \dot{p}_i\right) \cdot \delta\mathbf{r}_i = \Sigma_i \left(\mathbf{F}_i^{(a)} - \dot{p}_i\right) \cdot \delta\mathbf{r}_i + \Sigma_i \, \mathbf{f}_i \cdot \delta\mathbf{r}_i = 0. \tag{3.23}$$

Again we assume that the virtual work of the forces of constraint $\mathbf{f}_i$ is 0, so that:

$$\Sigma_i \left(\mathbf{F}_i^{(a)} - \dot{p}_i\right) \cdot \delta\mathbf{r}_i = 0. \tag{3.24}$$

This is *d'Alembert's Principle*. Since the forces of constraint $\mathbf{f}_i$ are now eliminated, I will now drop the superscript $^{(a)}$ for 'applied'.

This prompts three immediate comments: technical, philosophical and strategic.

(i): Given d'Alembert's Principle, we can argue, as we did after eq 3.15. Namely, suppose that the coordinates are independent (so that constraints, if present, are holonomic and we focus on the constraint surface), and that the displacements reversible. Then each $\mathbf{F}_i - \dot{p}_i = 0$. This difference of the applied force and the inertial force, $\mathbf{F}_i - \dot{p}_i$, is sometimes called the 'effective force' on particle i; (and also sometimes the 'constraint force', despite equalling $-\mathbf{f}_i$!). So d'Alembert's principle eq. 3.24 implies: the total virtual work done by the effective forces is zero. This is sometimes written as: $\delta w^e = 0$, where δw^e is a non-exact differential; (non-exact because in general $\mathbf{f}_i$ is not derived from a work function).

(ii): Again, we see (Restrict) and (Modality;1st) at work.

[36] As usual, the history of the principle is much more complicated than modern formulations suggest. For d'Alembert's original formulation, cf. Fraser (1985a).

(iii): A warning about my chosen route for expounding analytical mechanics. On this route, d'Alembert's principle figures large (despite having so simple a deduction from the principle of virtual work). Thus I shall report in the Paragraphs just below how it implies a form of the conservation of energy, and a form of Lagrange's equations. It also underlies the other central principles of analytical mechanics, including the most important one, Hamilton's Principle—which I will discuss in Section 4. Besides, 'underlies' here is logically strong: it means 'implies when taken together with just pure mathematical apparatus, and (for some implications) some general physical assumptions; and in some cases, the converse implication holds'.

But I admit: many fine expositions adopt other routes, on which d'Alembert's principle hardly figures. In particular, one can proceed directly from Newton's equations to Lagrange's equations. For example, cf. Woodhouse (1987: 31-34,41-47), Johns (2005: Chapter 2-2,2-7); or in more detail, for successively less straightforward systems, e.g. first for holonomic, then for non-holonomic, constraints, Desloge (1982: 522-523, 542-545, 554-557, 564-565).

As it stands, d'Alembert's principle has a significant disadvantage; (so that it does not itself supply a general scheme, on a par with the Lagrangian or Hamiltonian one). Though the virtual work of the applied forces is often an exact differential (i.e. the forces are monogenic, there is a work-function) there is no such function for the virtual work of the inertial forces.

It is one of the key insights of Lagrangian mechanics that this disadvantage can be overcome, by expressing d'Alembert's principle in terms of configuration space. In particular, by integrating d'Alembert's principle with respect to time, we can derive Hamilton's principle itself. More precisely: by integrating the total virtual work of the effective force δw^e along the system's history (trajectory in configuration space) with time as the integration variable, the inertial forces become monogenic: for details cf. Section 4.2.

Besides, we can similarly derive from d'Alembert's principle other principles of Lagrangian mechanics. (In some cases, this is done via Hamilton's principle, i.e. by first deriving Hamilton's principle from d'Alembert's principle.) For reasons of space, I shall not report such derivations, though they illustrate well my moral (Reformulate). For some details, cf. Lanczos (1986: 106-110). He discusses in order:

(a): how Gauss' principle of least constraint expresses d'Alembert's principle as a minimum principle;

(b): the merits and demerits of Gauss' principle; and

(c): Hertz' interpretation of Gauss' principle as requiring the system's path in configuration space be of minimal curvature—an idea developed by Jacobi's principle, which I will discuss in Section 4.6.

I emphasise that these derivations and discussion are all conducted under the restriction we imposed at the start of Section 3.2; viz. that the constraints are ideal, i.e. the virtual work of the forces of constraint is 0. Thus we again see (Restrict).[37]

[37] Incidentally, Lanczos raises this restriction to a postulate, called Postulate A (1986: 76).

But I shall report: (A) how d'Alembert's principle implies the conservation of energy (under appropriate conditions); and (B) how it also implies Lagrange's equations. These equations, set in the context of Hamilton's principle, will be centre-stage in the next Section; so it is also worth seeing that they are implied by d'Alembert's principle directly, i.e. not via Hamilton's principle.

Then (in the next Subsection) I will end this Section by discussing how Lagrange's equations, regardless of how they are deduced, represent mechanical problems and illustrate (Scheme).

3.3.1.A Conservation of energy Integrating D'Alembert's Principle, under certain assumptions, yields as a result the conservation of energy; as follows. If the applied forces are monogenic, then the Principle becomes

$$\delta V + \Sigma_i m_i \ddot{\mathbf{r}}_i \cdot \delta \mathbf{r}_i = 0. \tag{3.25}$$

Then: If (and only if!) the system is not only holonomic, but also scleronomous in work-function and constraints (i.e. the work-function U can be $U(q_i, \dot{q}_i)$ but U cannot be an explicit function of t, and the constraints are $g_j(q_i) = 0$ but not $g_j(q_i, t) = 0$), then we can choose the $\delta \mathbf{r}_i$ to be the *actual* changes $d\mathbf{r}_i$ in an infinitesimal time dt; (Lanczos 1986: 92-94; cf. also footnote 33). This implies:

$$\Sigma_i m_i \ddot{\mathbf{r}}_i \cdot \delta \mathbf{r}_i = \Sigma_i m_i \ddot{\mathbf{r}}_i \cdot d\mathbf{r}_i = dT \tag{3.26}$$

so that D'Alembert's Principle gives:

$$dV + dT = d(T + V) = 0. \tag{3.27}$$

This result, being a generalization across a whole class of problems, illustrates my moral (Modality;2nd).

3.3.1.B Deducing Lagrange's equations The deduction of Lagrange's equations from d'Alembert's Principle illustrates (Reformulate) and (Restrict). In the course of the derivation, one makes two restrictions in addition to constraints being ideal (which is implicit in d'Alembert's principle): first to holonomic constraints, and then to monogenic systems. (Conservativity, which is used in some expositions, is not necessary.) *Warning:* The details of this derivation are not used later on.

The idea will be to transform d'Alembert's principle eq. 3.24 to generalized coordinates q_j, of which there are say n (e.g. above we had $n = 3N - k$, with N particles and k constraints). So the transformed equation will concern virtual displacements δq_j. Then we will assume the constraints are holonomic, i.e. the q_j are independent, so that each coefficient of δq_j in the transformed equation must vanish.

We begin by noting that the transformation (i again labelling particles)

$$\mathbf{r}_i = \mathbf{r}_i(q_1, q_2, \ldots, q_n, t) \tag{3.28}$$

yields

$$\mathbf{v}_i := \frac{d\mathbf{r}_i}{dt} = \Sigma_j \frac{\partial \mathbf{r}_i}{\partial q_j}\dot{q}_j + \frac{\partial \mathbf{r}_i}{\partial t}. \tag{3.29}$$

This implies "cancellation of the dots", i.e.

$$\frac{\partial \mathbf{v}_i}{\partial \dot{q}_j} = \frac{\partial \mathbf{r}_i}{\partial q_j}; \tag{3.30}$$

and also commutation of differentiation with respect to t and q_j, i.e.

$$\frac{d}{dt}\frac{\partial \mathbf{r}_i}{\partial q_j} = \frac{\partial \mathbf{v}_i}{\partial q_j}. \tag{3.31}$$

Besides, note that

$$\delta \mathbf{r}_i = \Sigma_j \ \ \frac{\partial \mathbf{r}_i}{\partial q_j}\delta q_j \tag{3.32}$$

implies

$$\Sigma_i \ \ \mathbf{F}_i \cdot \delta \mathbf{r}_i = \Sigma_j \ \ Q_j \delta q_j \quad \text{with} \quad Q_j := \Sigma_i \, \mathbf{F}_i \cdot \left(\frac{\partial \mathbf{r}_i}{\partial q_j}\right). \tag{3.33}$$

The Q_j are the components of *generalized force*. Though the Q_j need not have the dimensions of force (and will not if the q_j do not have the dimensions of length), $Q_j \delta q_j$ must have the dimensions of work.

Applying these results to the second term of d'Alembert's Principle, eqn 3.24, i.e. to

$$\Sigma_i \ \ \dot{p}_i \cdot \delta \mathbf{r}_i = (\Sigma_i \ \ m_i \dot{\mathbf{v}}_i) \cdot \left(\Sigma_j \ \ \frac{\partial \mathbf{r}_i}{\partial q_j}\delta q_j\right) \tag{3.34}$$

and using the definition of total kinetic energy $T := \Sigma_i \frac{1}{2} m_i \mathbf{v}_i^2$, d'Alembert's Principle becomes:

$$\Sigma_j [\{\frac{d}{dt}(\frac{\partial T}{\partial \dot{q}_j}) - \frac{\partial T}{\partial q_j}\} - Q_j]\delta q_j = 0. \tag{3.35}$$

Now let us assume the constraints are holonomic, so that the q_j are independent. Then we can conclude that for each j

$$\frac{d}{dt}(\frac{\partial T}{\partial \dot{q}_j}) - \frac{\partial T}{\partial q_j} = Q_j. \tag{3.36}$$

Equations 3.36 are sometimes called *Lagrange's equations*. But this name is more often reserved for the form these equations take for a system that is not just holonomic, but also monogenic with a velocity-independent work function. That is: If each applied force $\mathbf{F}_i$ (the force on the ith particle) is a gradient (with respect to i's coordinates) of a (possibly time-dependent) scalar function V on configuration space, i.e.

$$\mathbf{F}_i = -\nabla_i V \tag{3.37}$$

then the definition of Q_j, eq. 3.33, immediately yields

$$Q_j = -\frac{\partial V}{\partial q_j} \tag{3.38}$$

so that defining the *Lagrangian* $L := T - V$, we get from eqn 3.36:

$$\frac{d}{dt}(\frac{\partial L}{\partial \dot{q}_j}) - \frac{\partial L}{\partial q_j} = 0. \tag{3.39}$$

(Furthermore, we can get this same form for the equations (again with $L = T - V$) if there is velocity-dependence, provided the generalized forces Q_j are then obtained by

$$Q_j = -\frac{\partial V}{\partial q_j} + \frac{d}{dt}\left(\frac{\partial V}{\partial \dot{q}_j}\right). \tag{3.40}$$

This formula applies in electromagnetism; (cf. e.g. Goldstein et al (2002: 22).)

This is a good point at which to note the form of the kinetic energy T in terms of the generalized coordinates; and in particular, the form T takes when the constraints are scleronomous—which is the case I will mostly consider. We transform between cartesian and generalized coordinates by equations 3.28 and 3.29. Note in particular that

$$T = \Sigma_i \ \frac{1}{2} m_i v_i^2 = \Sigma_i \ \ \frac{1}{2} m_i \left(\Sigma_j \frac{\partial \mathbf{r}_i}{\partial q_j}\dot{q}_j + \frac{\partial \mathbf{r}_i}{\partial t}\right)^2 \tag{3.41}$$

and that expanding this expression, we can express T in terms of the generalized coordinates as

$$T = a + \Sigma_j a_j \dot{q}_j + \Sigma_{j,k} a_{jk} \dot{q}_j \dot{q}_k \tag{3.42}$$

where a, a_j, a_{jk} are definite functions of the $\mathbf{r}$'s and t, and hence of the q's and t. Besides, if the transformation equations 3.28 and 3.29 do not contain time explicitly (i.e. the constraints are scleronomous), then only the last term of eq. 3.42 is non-zero: i.e. T is a homogeneous quadratic form in the generalized velocities.

This result has a geometric significance. For we saw in eq. 3.5 that T defines a metric (a line-element) in the $3N$-dimensional configuration space of N particles; we now see that for scleronomous constraints it defines a metric on the constraint surface. This geometric viewpoint will be developed in Paragraph 3.3.2.E.

3.3.2 Lagrange's equations: (Accept), (Scheme) and geometry

Lagrange's equations (especially in the form of eq 3.39) are the centre-piece of Lagrangian mechanics; and since Lagrangian mechanics is the basis of other approaches to analytical mechanics, such as Hamiltonian mechanics, these equations can fairly claim to be the crux of the subject. I end this Section with five comments (in five Paragraphs) about these equations.

The first comment is foundational: it concerns using Lagrange's equations to justify analysing a system with holonomic constraints wholly in terms of the constraint surface. This comment illustrates my moral (Accept). The second, third and fourth comments are about solving Lagrange's equations, and the practical advantages of using them, i.e. my moral (Scheme). These comments lead in to the fifth comment, about the modern geometric description of Lagrangian mechanics, and the representation it provides of the solution of a mechanical problem.

3.3.2.A Confinement to the constraint surface I said at the end of Section 3.1 when I first introduced the idea of the constraint surface, that there were theorems proving that in the limit as the forces of constraint become infinitely strong, the system's dynamics in the full configuration space becomes as analytical mechanics describes it, on the constraint surface. With Lagrange's equations eq. 3.39 in hand, we can state such a theorem (cf. Arnold 1989: 75-77).

Suppose again we are given N particles, and so a $3N$-dimensional configuration space M, which we equip with the line-element eq 3.5. Let S be an n-dimensional hypersurface of M; (so we imagine there are $k := N - n$ holonomic constraints); let $\mathbf{q}_1$ be n coordinates on S and let $\mathbf{q}_2$ be k coordinates in directions orthogonal to S. Let the potential energy have the form $V = V_0(\mathbf{q}_1, \mathbf{q}_2) + C\mathbf{q}_2^2$. The idea is that we will let C tend to infinity, to represent a stronger and stronger force constraining the system to stay on S. So consider the motion in M according to eq 3.39 (with $3N$ coordinates) of a system with initial conditions at $t = 0$

$$\mathbf{q}_1(0) = \mathbf{q}_1^0 \quad \dot{\mathbf{q}}_1(0) = \dot{\mathbf{q}}_1^0 \quad \mathbf{q}_2(0) = \dot{\mathbf{q}}_2(0) = 0 \tag{3.43}$$

Then as $C \to \infty$, a motion on S is defined with the Lagrangian

$$L_* = T \mid_{\mathbf{q}_1 = \mathbf{q}_2 = 0} - V_0 \mid_{\mathbf{q}_2 = 0} . \tag{3.44}$$

This result illustrates (Accept) and (Ideal). This conception of a constrained system as a limit also plays a role in the equivalence of some analytical mechanical principles; cf. Section 4.2 and Arnold (1989: 91f.).

3.3.2.B Integrating Lagrange's equations I begin with three general comments about solving Lagrange's equations, in the usual form, eq 3.39.

(1): *Differential equations on a manifold: velocity phase space*:—
The first point to stress is that the n second-order differential equations eq. 3.39 are (despite the appearance of the partial differentials!) ordinary differential equations; equations which are defined on a differential manifold, the constraint surface.

In Paragraph 2.1.3.A's brief review of the theory of ordinary differential equations, I mentioned that the basic theorem about the local existence and uniqueness of solutions of (and local constants of the motion for) first-order ordinary differential equations carried over to higher-order equations defined on a differential manifold. And Paragraph

3.3.2.E will give more details about the description of Lagrangian mechanics in terms of modern geometry, i.e. manifolds. But there are two important points to make about this, which do not require any modern geometry.

(i): It is worth introducing jargon for the $2n$-dimensional space coordinatized by the qs and $\dot{q}$s taken together (n is the number of configurational degrees of freedom). After all, the crucial function, the Lagrangian $L(q, \dot{q})$ is defined on this space. It is called *velocity phase space*. (Sometimes, it is called 'phase space'; but this last term is more often used for the *momentum phase space* of Hamiltonian mechanics.) It is often denoted by TQ: here T stands for 'tangent', not 'time', for reasons given in Paragraph 3.3.2.E.

Incidentally: In writing $L(q, \dot{q})$ and saying L is defined on TQ, I have simplified by setting aside time-dependent potentials and rheonomous constraints. For treating them, there is again useful jargon. If Q is a configuration space given independently of time, then the space $Q \times \mathbb{R}$, with points (q, t), $t \in \mathbb{R}$ representing a time, is often called the *extended configuration space*. And the treatment of time-dependent potentials and-or rheonomous constraints might then proceed in what can be called *extended velocity phase space* $TQ \times \mathbb{R}$.

(ii): As regards integrating Lagrange's equations:— Recall the idea from elementary calculus that n second-order ordinary differential equations have a (locally) unique solution, once we are given $2n$ arbitrary constants. This idea holds good for Lagrange's equations, even in the "fancy setting" of a manifold TQ or $TQ \times \mathbb{R}$. And the $2n$ arbitrary constants can be given just as one would expect: as the initial configuration and generalized velocities $q_j(t_0), \dot{q}_j(t_0)$ at time t_0. Comments (2) and (3) expand a little on this.

(2): *The Hessian condition*:—
Expanding the time derivatives in eq. 3.39 gives

$$\frac{\partial^2 L}{\partial \dot{q}_k \partial \dot{q}_j} \ddot{q}_k = -\frac{\partial^2 L}{\partial q_k \partial \dot{q}_j} \dot{q}_k - \frac{\partial^2 L}{\partial t \partial \dot{q}_j} + \frac{\partial L}{\partial \dot{q}_j} \quad . \tag{3.45}$$

So the condition for being able to solve these equations to find the generalized accelerations at some initial time t_0, $\ddot{q}_j(t_0)$, in terms of $q_j(t_0), \dot{q}_j(t_0)$ is that the *Hessian* matrix $\frac{\partial^2 L}{\partial \dot{q}_j \partial \dot{q}_k}$ be nonsingular. Writing the determinant as $|\ \ |$, and partial derivatives as subscripts, the condition is that:

$$\left| \frac{\partial^2 L}{\partial \dot{q}_j \partial \dot{q}_k} \right| \quad \equiv \quad | L_{\dot{q}_j \dot{q}_k} | \quad \neq \quad 0 \quad ; \tag{3.46}$$

This *Hessian condition* holds in very many mechanical problems; and henceforth, we impose it. Indeed it underpins most of what follows: it will also be the condition needed to define the *Legendre transformation*, by which we will pass from Lagrangian to Hamiltonian mechanics.

But I should also stress that the Hessian condition can fail, and does fail in important problems. The point has been recognized since the time of Lagrange and

Hamilton; though it was only in the mid-twentieth century, that Dirac, Bergmann and others developed a general framework for mechanics that avoided the Hessian condition. I shall make just three remarks about this: one mathematical, one physical and one terminological.

(i): It is easy to show that the Hessian condition implies that L cannot be homogeneous of the first degree in the $\dot{q}_i$. That is, L cannot obey, for all $\lambda \in \mathbb{R}$: $L(q_i, \lambda\dot{q}_i, t) = \lambda L(q_i, \dot{q}_i, t)$. It is also easy to show that homogeneity of the first degree in the $\dot{q}_i$ for positive λ is equivalent to an integral of L (viz., the integral that is central to the calculus of variations: cf. Section 4.2) being independent of the choice of its integration variable (called being 'parameter-independent'). (For details, cf. e.g. Lovelock and Rund (1975: Section 6.1).)

(ii): Some problems are naturally analysed using an L that is homogeneous in this sense, and so has a parameter-independent integral.

Perhaps the best-known case occurs in Fermat's principle in geometric optics. It says, roughly speaking, that a light ray between spatial points P_1 and P_2 travels by the path that minimizes the time taken. If one expresses this principle as minimizing an integral with time as the integration variable, one is led to an integrand that is in general, e.g. for isotropic media, homogeneous of degree 1 in the velocities $\dot{q}_i$—conflicting with the Hessian condition eq. 3.46. So geometric optics usually proceeds by taking a spatial coordinate as the integration variable, i.e. the parameter along the light path. For details and references, cf. e.g. (Butterfield 2004c: Sections 5,7).

But also in mechanics as against optics, there are cases of homogeneous L and parameter-independence. This is especially true in relativistic theories—beyond this paper's scope! (Cf. Johns (2005: Part II) for a beautifully thorough account.)

(iii): Beware of jargon. The framework of Dirac et al. is called 'constrained dynamics"—so be warned that this is a very different sense of 'constraint' than ours.

Of course, even with eq. 3.46, it is still usually hard *in practice* to solve for the $\ddot{q}_j(t_0)$: they are buried in the left hand side of eq. 3.45. This circumstance prompts the move to Hamiltonian mechanics, taken up in the companion paper. Meanwhile, the topic of the practical difficulty of solving equations prompts (3).

(3): *The ineluctable*:—
I admit that from a very general viewpoint, Lagrange's equations represent no advance over the vectorial approach to mechanics. Namely: the dynamical problem of n degrees of freedom is still expressed by n second-order differential equations. Broadly speaking, this "size" of the dynamical problem is an ineluctable consequence of Newton's second law being second-order in time. By and large, the most that a general scheme can hope to do to reduce this "size" is:

(i) to provide help in finding and-or using new variables that simplify the problem; especially by reducing the number of equations to be solved, by some of the new variables dropping out (cf. (Reduce) and (Separate));

(ii) to make a useful trade-in of second-order equations for first-order equations.

As we shall see, Lagrangian mechanics does (i). (Hamiltonian mechanics does both (i) and (ii).)

So much by way of general remarks about integrating Lagrange's equations. I now turn to the practical advantages of using them to solve problems. We can already see two substantial advantages—advantages that are valid for all holonomic systems (eq. 3.36), not just those holonomic systems which are monogenic with a velocity-independent work function (eq. 3.39).

3.3.2.C Covariance; (Wider) The above deduction of Lagrange's equations, eq. 3.36 and 3.39, makes it clear that they are covariant under any coordinate transformations (aka: *point-transformations*) $q_j \to q'_j$. (Of course, one can also prove this covariance directly i.e. assume the equations hold for the q_j, and assume some transformation $q_j \to q'_j$, and then prove they also hold for the q'_j.) This covariance means we can analyse a problem in whichever generalized coordinates we find convenient: whichever coordinates we choose, we just write down the Lagrangian in those coordinates and then solve Lagrange's equations in the form eq. 3.36 or eq. 3.39. This is one of our main illustrations of (Scheme), and its merit (Wider). (We will later see this covariance as an automatic consequence of a variational principle; cf. the end of Section 4.2.)

3.3.2.D One function; (Fewer) In any such generalized coordinates, and for any number of particles (or generalized coordinates), the solution of the problem is encoded in a smaller number of functions than the number of degrees of freedom immediately suggests. In eq. 3.36, the inertia is encoded in one function T (cf. (a) in Section 3.1.2). And more remarkably, eq. 3.39 encodes the forces in one function V; besides, it encodes the solution to the problem in just one function, viz. $L := T - V$. This illustrates merit (Fewer) of (Scheme). This situation prompts a technical comment, and some philosophical remarks.

(1): *Equivalent Lagrangians*:— The technical comment is that my phrase 'one function' needs clarifying. To explain this, let us consider just holonomic conservative systems, described by eq. 3.39. It is easy to show that two Lagrangians L_1 and L_2 determine the very same equations eq. 3.39 if they differ by the time derivative of a function $G(q_j(t))$ of the generalized coordinates, i.e. if $L_1(q,\dot{q}) - L_2(q,\dot{q}) = \frac{d}{dt}\, G(q(t))$. Such Lagrangians are called *equivalent*.

The converse is false: two inequivalent Lagrangians can yield the same equations of motion. A two-dimensional harmonic oscillator gives an example. We met this system in Paragraph 2.1.3.B, with different frequencies in the two dimensions. Now we need only the special case of a common frequency. So the usual Lagrangian and its Lagrange equations are (with cartesian coordinates written as qs):

$$L_1 = \frac{1}{2}\left[\dot{q}_1^2 + \dot{q}_2^2 \;-\; \omega^2(q_1^2 + q_2^2)\right] \;\;;\;\; \ddot{q}_i + \omega^2 q_i = 0 \;\;, i = 1, 2. \tag{3.47}$$

But the same Lagrange equations, i.e. the same dynamics, is given by

$$L_2 = \dot{q}_1\dot{q}_2 - \omega^2 q_1 q_2 \;\;, \tag{3.48}$$

which is not equivalent to L_1. This example is given by José and Saletan (1998: 68, 103, 145), together with a proof that the converse results holds *locally*.[38]

(2): *How the Lagrangian controls the motion*:—
Turning to philosophy: it is at first sight puzzling that the motion in 3-dimensional space of an arbitrary number of particles can be controlled by fewer functions than there are degrees of freedom: how so?

Part of the answer is of course that the functions are defined not on physical space. In particular, V is defined on configuration space Q (or for a time-dependent potential on extended configuration space $Q \times \mathbb{R}$). And L and T are defined on the $2n$-dimensional velocity phase space TQ with points $(q, \dot{q})$ (or again: on the extended velocity phase space $TQ \times \mathbb{R}$). So these functions encode properties of the entire system's configuration, or of the configuration taken together with the n generalized velocities. And there is no difficulty in general about how a single function on a higher-dimensional space might determine a motion in the space. After all, one could take the function's (higher-dimensional) gradient.

But this is of course *not* how these functions determine the motion; (though incidentally, it is in effect how another function, the Hamiltonian, determines the motion in Hamiltonian mechanics). So the question arises how they do so, i.e. whether there is some notion—perhaps a geometric one, like taking the gradient—that underlies how these functions determine the motion, via eq.s 3.36 and 3.39.

The short answer is that there *is* such a notion: these equations reflect the fact that the dynamical laws (the determination of the motion) can be given a *variational formulation*. In particular, for holonomic conservative systems (cf. eq. 3.39): it turns out that when we consider L's values not just at various times for the actual motion, but also for suitably similar *possible* motions, then the collection of all these values encodes the physical information that determines the motion.

But this short answer immediately invites two further questions, one philosophical and one technical. The philosophical question is: 'how can it be that *possible* values of a function such as L determine *actual* motions?' As I mentioned at the end of Section 2.2.1, I address this issue at length elsewhere (2004e: Section 5) and will not pursue it here. The technical question is (again, stated just for holonomic conservative systems): 'how do L's values for some merely possible motions determine the actual motion?' I will answer that in detail in Section 4; (and Hamiltonian mechanics gives a deeper perspective on the answer).

3.3.2.E Geometric formulation I turn to give a brief description of the elements of Lagrangian mechanics in terms of modern differential geometry. (*Warning: This Paragraph is not used later on.*) Here 'elements' indicates that:

(i): As this paper mostly eschews modern geometry, I will here assume without explanation various geometric notions, in particular: manifold, vector, one-form, met-

[38]Thanks to Harvey Brown for alerting me to this example and José and Saletan's discussion; and to two other uses of this example in connection Noether's theorem (Section 4.7.5).

ric, Lie derivative and tangent bundle. (But a reassurance: Section 4.7.3 gives some explanations of manifold, vector field and tangent bundle, which apply equally here.)

(ii): I make the simplifying assumptions that led to the usual form of Lagrange's equations eq. 3.39: in particular, that the constraints are holonomic, scleronomous and ideal, and that the system is monogenic with a velocity-independent work-function. But much of the description below can be generalized in various ways to avoid these assumptions.

(iii): I will also simplify by speaking "globally, not locally". For example, I will speak as if the relevant scalar functions, and vector fields and their integral curves, are defined on a whole manifold; when in fact all that Lagrangian mechanics can claim in application to most systems is a corresponding local statement—as we already know from Paragraph 2.1.3.A's report that differential equations are guaranteed the existence and uniqueness only of a *local* solution.

Finally, a warning:— Hitherto I have written q_j, q_k etc. for the generalized coordinates. But in this Paragraph, I need to respect the distinction between contravariant and covariant indices (in more modern jargon: vectors and forms). So I write the coordinates as q^i, q^j etc. Similarly, I will in this Paragraph, though not elsewhere in the paper, adopt the convention that repeated indices are summed over.

We begin by assuming that the configuration space (i.e. the constraint surface) is a manifold Q. So the kinetic energy T, being a homogeneous quadratic form in the generalized velocities (cf. discussion of eq. 3.42), defines a metric on Q.

The physical state of the system, taken as a pair of configuration and generalized velocities, is represented by a point in the tangent bundle TQ. That is, writing T_x for the tangent space at $x \in Q$, TQ has points $(x, \tau), x \in Q, \tau \in T_x$; so TQ is a $2n$-dimensional manifold. As I said in Paragraph 3.3.2.B, TQ is sometimes called *velocity phase space*. We will of course work with the natural coordinate systems on TQ induced by coordinate systems q on Q; i.e. with the $2n$ coordinates $(q, \dot{q}) \equiv (q^i, \dot{q}^i)$.

The fundamental idea is now that this tangent bundle is the arena for the geometric description of Lagrangian mechanics: in particular, the Lagrangian is a scalar function $L : TQ \to \mathbb{R}$ which "determines everything". But I must admit at the outset that this involves limiting our discussion to Lagrangians, and coordinate transformations, that are time-independent.

More precisely: recall, first, the simplifying assumptions in (ii) above. Velocity-dependent potentials and-or rheonomous constraints would prompt one to use the extended configuration space $Q \times \mathbb{R}$, and-or the extended velocity phase space $TQ \times \mathbb{R}$.

So would time-dependent coordinate transformations.[39] I admit that this last is a considerable limitation from a philosophical viewpoint, since it excludes boosts, i.e. transformations to a coordinate system moving at constant velocity with respect to another; and boosts are central to the philosophical discussion of spacetime symmetry groups, and especially of relativity principles. To give the simplest example: the Lagrangian of a free particle in one spatial dimension is just its kinetic energy, i.e.

[39] Thanks to Harvey Brown for urging this last limitation.

in cartesian coordinates $\frac{1}{2}m\dot{x}^2$. Under a boost with velocity v in the x-direction to another cartesian coordinate system, $x \mapsto x' := x - vt$; i.e. the point labelled x in the first system is labelled by $x - vt$ in the second system (assuming the two spatial coordinate systems coincide when $t = 0$). For example, if $v = 5$ metres per second, the point first labelled $x = 10$ metres is labelled by the second system at time $t = 2$ seconds as $10 - 5.2 = 0$, i.e. as the origin. So $\dot{x}' = \dot{x} - v$, and the Lagrangian, i.e. the kinetic energy, is not invariant under the boost: by choosing v equal to the velocity of the particle in the x-direction, one can even make the particle have zero energy. (I shall return to the topic of transformations under which the Lagrangian is invariant, though again with my limitation to TQ, when presenting Noether's theorem; Section 4.7, cf. especially Section 4.7.3).

But setting aside these *caveats*, I now describe Lagrangian mechanics on TQ, with four comments.

(1): $2n$ *first order equations; the Hessian again*:—
The Lagrangian equations of motion (in the natural coordinates $(q, \dot{q})$) are now $2n$ *first-order* equations for the functions $q^i(t), \dot{q}^i(t)$, determined by the scalar function $L : TQ \to \mathbb{R}$. The $2n$ equations fall in to two groups: namely

(a) the n equations eq. 3.45, with the $\ddot{q}^i$ taken as the time derivatives of $\dot{q}^i$ with respect to t; i.e. we envisage using the Hessian condition eq. 3.46 to solve eq. 3.45 for the $\ddot{q}^i$, hard though this usually is to do in practice;

(b) the n equations $\dot{q}^i = \frac{dq^i}{dt}$.

(2): *Vector fields and solutions*:—

(a): These $2n$ first-order equations are equivalent to a vector field on TQ. This vector field is called the 'dynamical vector field', or for short the 'dynamics'. I write it as D (to distinguish it from the generic vector field $X, Y, ...$). So the solutions are integral curves of D.

(b): In the natural coordinates $(q^i, \dot{q}^i)$, the vector field D is expressed as

$$D = \dot{q}^i \frac{\partial}{\partial q^i} + \ddot{q}^i \frac{\partial}{\partial \dot{q}^i} \quad ; \tag{3.49}$$

and the rate of change of any dynamical variable f, taken as a scalar function on TQ, $f(q, \dot{q}) \in \mathbb{R}$ is given by

$$\frac{df}{dt} = \dot{q}^i \frac{\partial f}{\partial q^i} + \ddot{q}^i \frac{\partial f}{\partial \dot{q}^i} = D(f). \tag{3.50}$$

(c): Again, the fundamental idea of the Lagrangian framework is that the Lagrangian L "determines everything". In particular, it determines: the dynamical vector field D, and so (for given initial $q, \dot{q}$) a solution, a trajectory in TQ, $2n$ functions of time $q(t), \dot{q}(t)$ (with the first n functions determining the latter).

(d): The (local) existence and uniqueness of solutions to sets of first-order equations means not just that initial conditions $q^i(t_0), \dot{q}^i(t_0)$ determine a unique solution; but this solution is now a curve (parametrized by time) in TQ. This separation of solutions/trajectories within TQ is important for the visual and qualitative understanding of solutions.

(3): *Geometric formulation of Lagrange's equations*:—
We can formulate Lagrange's equations in a coordinate-independent way, by using three ingredients. Namely: L itself (a scalar, so coordinate-independent); the vector field D that L defines; and the one-form on TQ defined by L (locally, and in terms of the natural coordinates $(q^i, \dot{q}^i)$) by

$$\theta_L := \frac{\partial L}{\partial \dot{q}^i} dq^i \quad . \tag{3.51}$$

(This one-form takes a central role in Hamiltonian mechanics, where it is called the *canonical one-form*.)

The Lie derivative of θ_L along the vector field D on TQ defined by L is, by the Leibniz rule:

$$\mathcal{L}_D \theta_L = (\mathcal{L}_D \frac{\partial L}{\partial \dot{q}^i}) dq^i + \frac{\partial L}{\partial \dot{q}^i} \mathcal{L}_D (dq^i) \quad . \tag{3.52}$$

But the Lie derivative of any scalar function $f : TQ \to \mathbb{R}$ along any vector field X is just $X(f)$; and for the dynamical vector field D, this is just $\dot{f} = \frac{\partial f}{\partial q^i} \dot{q}^i + \frac{\partial f}{\partial \dot{q}^i} \ddot{q}^i$. So we have

$$\mathcal{L}_D \theta_L = (\frac{d}{dt} \frac{\partial L}{\partial \dot{q}^i}) dq^i + \frac{\partial L}{\partial \dot{q}^i} d\dot{q}^i \quad . \tag{3.53}$$

Rewriting the first term by the Lagrange equations, we get

$$\mathcal{L}_D \theta_L = (\frac{\partial L}{\partial q^i}) dq^i + \frac{\partial L}{\partial \dot{q}^i} d\dot{q}^i \equiv dL \quad . \tag{3.54}$$

We can conversely deduce the familiar Lagrange equations from eq. 3.54, by taking coordinates. So we conclude that these equations' coordinate-independent form is:

$$\mathcal{L}_D \theta_L = dL \quad . \tag{3.55}$$

(4): *Limitations*:—
Finally, a comment about the Lagrangian framework's limitations as regards solving problems, and how they prompt the transition to Hamiltonian mechanics.

Recall the remark at the end of Paragraph 3.3.2.B (2), that the n equations eq. 3.45 are in general hard to solve for the $\ddot{q}^i(t_0)$: they lie buried in the left hand side of eq. 3.45. On the other hand, the n equations $\dot{q}^i = \frac{dq^i}{dt}$ (the second group of n equations in (1) above) are as simple as can be.

This makes it natural to seek another $2n$-dimensional space of variables, ξ^α say $(\alpha = 1, ..., 2n)$, in which:

(i): a motion is described by first-order equations, so that we have the same advantage as in TQ that a unique trajectory passes through each point of the space; but in which

(ii): all $2n$ equations have the simple form $\frac{d\xi^\alpha}{dt} = f_\alpha(\xi^1, ...\xi^{2n})$ for some set of functions $f_\alpha (\alpha = 1, ..., 2n)$.

Indeed, Hamiltonian mechanics provides exactly such a space: viz., the cotangent bundle of the configuration manifold, instead of its tangent bundle.

4 Lagrangian mechanics: variational principles and reduction of problems

4.0 Preamble This Section will begin with conceptual discussion, and then move to more technical matters. I will first introduce the two main variational principles of Lagrangian mechanics: the principle of least action (understood as it was by Lagrange and Euler), and Hamilton's Principle; (Section 4.1).

Beware: Hamilton's Principle is often called a (or even: the) least action principle. Indeed, a more general warning is in order. 'Action' has, unfortunately, various meanings; there is no agreed and exact usage, though it always has the dimension of momentum $\times$ length = energy $\times$ time. In this Section, action will tend to mean the integral with respect to time, along a possible history or trajectory of the system in configuration space, of a quantity with the dimension of energy: $\int E\ dt$.

(But the companion paper will give increasing prominence to:
(i) the integral (not always along a possible history of the system!) of a momentum p with respect to length $\int p\ dq$; or more generally, summing over degrees of freedom $\int \Sigma_i\ p_i\ dq_i$;[40] and
(ii) the integral (again, not always along a history) of the *difference*, $\int \Sigma_i\ p_i\ dq_i - E\ dt$.)

After Section 4.1, Hamilton's Principle takes centre-stage. Its technical features are reported in Sections 4.2, 4.3. Then the rest of the Section is dominated by the theme of symmetry: especially, how a symmetry can help reduce the number of variables of a problem—again, the merit (Reduce).

First, I introduce generalized momenta in the context of the conservation of energy (Section 4.4). Section 4.5 begins with the simple but important result that the generalized momentum of any cyclic coordinate is a constant of the motion, and so reduces the dimension of the dynamical system by one. The rest of the Section develops this result in two ways.

(1): First, I describe the method of Routhian reduction. This leads to Section 4.6's explanation of how, starting from Hamilton's Principle, Routhian reduction applied to time as a cyclic coordinate recovers Euler's and Lagrange's principle of least action. The discussion will also cover another famous variational principle of Lagrangian mechanics, Jacobi's principle.

(2): Finally in Section 4.7, I describe Noether's theorem, which provides a powerful general perspective on symmetry.

(All these aspects of this Section's discussion of symmetry will have analogues, and further developments, in Hamiltonian mechanics.)

As regards my four morals, this Section will illustrate all of them. But the main morals will be prominent:

(i): the four merits of (Scheme); i.e. (Fewer), (Wider), (Reduce) and (Separate); as just discussed, (Reduce) will be especially prominent in connection with symmetries.

(ii): all three grades of (Modality); but especially the third grade, (Modality;3rd),

[40] $\int \Sigma_i\ p_i\ dq_i$ is the canonical one-form, which is central to Hamiltonian mechanics.

which involves considering possibilities that violate the actual laws.

4.1 Two variational principles introduced

Analytical mechanics contains many variational principles, which are closely related (and in some cases equivalent) to one another. But I will focus on just two principles, and their relationship to each other: Euler's and Lagrange's "principle of least action"; and Hamilton's Principle.[41] In this Subsection, I introduce them without technicalities. In particular, I will present them as using two different kinds of 'variation of a path': a distinction that I will gloss philosophically in terms of (Modality)'s three grades of modal involvement.

4.1.1 Euler and Lagrange

For simplicity, let us consider a single point-particle in a time-independent potential $V = V(\mathbf{r})$; in short, a conservative one-particle system. (We will remark later that the principle in fact applies much more widely.) Suppose one is given the initial conditions that the particle is at spatial point P_1 at time t_1, with a given total energy $E = T + V(P_1)$ compatible with the value of $V(P_1)$, i.e. $E \geq V(P_1)$. Then for any spatial path γ starting from P_1, the initial conditions, together with the conservation of energy, determine the particle's motion over the next time-step, if we assume that it must start out travelling along γ. For the initial value of T and the specification of γ determine an initial velocity. And so they determine at which point P' along γ the particle will be at time $t_1 + dt$ i.e. an infinitesimal time-step later. Furthermore, the conservation of energy, and value of $V(P')$ determine (by $T = E - V(P')$) what the speed of the particle is at the time $t_1 + dt$. The argument can be iterated. That is: if we assume that also at time $t_1 + dt$ the particle must continue to travel along our chosen path γ , then its motion over the next time-step is determined—and so on.[42]

Of course, we chose γ arbitrarily; and so (since V is given) we were almost certainly *wrong* to suppose that the particle must follow γ, even assuming it starts out along γ at time t_1. That is: the imagined motion is not just counterfactual but contralegal: it violates the dynamical laws (i.e. Newton's equations). But Euler and Lagrange discovered that: for this system, these laws are equivalent to a statement about a whole class of possible paths through P_1. To formulate this statement, first note that the previous paragraph also shows: The initial conditions and the requirement of energy conservation at all times also determine, for any time-interval $[t_1, t_2]$, the time-integral of T along the path γ. And similarly, for any other possible path, η say, through P_1:

[41] The history of the principles' discovery and evolution is fascinating: in particular, Lagrange himself worked with Hamilton's Principle—the name was coined by Jacobi, and only became prevalent in the 20th century. But I will not go into this history.

[42] By the way: the argument so far clearly also works for V a prescribed [i.e. independent of the particle] function of time. But the principle of least action , to follow, requires V independent of time.

the initial conditions, in particular the initial energy, and the requirement of energy conservation at all times determine, what the time-integral of T along η would be if the particle were to traverse η.

We can now state Euler's and Lagrange's principle of least action, for a single particle. The idea is as follows: Given

(i): the initial conditions that the particle is at spatial point P_1 at time t_1, with a given velocity $\mathbf{v}$ (which fixes the total energy $E = T + V(P_1)$); and given also

(ii): the particle later passes through the point P_2 (i.e. one assumes that P_2 lies on the particle's actual spatial path,, as determined by the dynamical laws and the given potential V): it follows that

(iii): the actual path traversed will be that path among all possible paths connecting P_1 to P_2, motion along which, with a common fixed initial energy E, makes the time-integral of T along the path, a minimum.

That is the idea. But (iii) needs amendment. For the actual path might not make the integral a minimum: even in comparison with just the class of all paths close to the actual one, rather than all possible paths. The precise statement of the principle is rather that the actual path makes the integral *stationary* in comparison with all sufficiently close paths.

Here 'stationarity' means that a derivative is zero. The details are made precise in the calculus of variations, and reviewed in Section 4.2. For the moment, we only need the idea that, as in elementary calculus, a zero derivative is compatible, not only with a minimum of the function in question, but also with a maximum or a turning-point of it. (We similarly replaced minimality by stationarity at the end of Section 3.2.1.)

This distinction, between minimization and stationarity of a function, has both a historical and a terminological significance—and both points will apply just as much to Hamilton's Principle as to the principle of least action.

Historically, some early advocates of the principle of least action asserted that the actual path minimized the integral. Besides, this was regarded as a remarkable "efficiency" or "economy" on the part of Nature—and as suggesting a proof of the existence of God. The main example of this tendency is Maupertuis, who announced (an obscure form of) the principle, claiming minimization, in 1744. But already in the same year, Euler published his ground-breaking treatise on the calculus of variations, in an addendum of which the principle is expounded as a precise theorem. (For details, cf. Fraser (1994); or more briefly, Kline (1972: 577-582), Yourgrau and Mandelstam (1979: 19-29).) In due course, it became clear that only the stationarity version of the principle held good; and similarly, that Hamilton's Principle (Section 4.1.2) was a matter of stationarity, not of minimization. Thus for example, Hamilton in 1833 criticized the idea of minimization, noting that 'the quantity pretended to be economized is in fact often lavishly expended': so he preferred to speak of a 'principle of stationary action'.

This distinction also raises a significant mathematical question, regardless of mechanics: what are the necessary or sufficient conditions, for problems in the calculus of variations, of securing a minimum rather than just stationarity? Though the inves-

tigation of this question has a long and distinguished history (starting essentially with Legendre in 1786), and a good deal is now known about it, we will not need any of these details. (For the history, cf. Kline (1972: 589-590, 745-749); for the technicalities, cf. e.g. Courant and D. Hilbert (1953: 214-216), Fox (1987: Chapters 2, 9).)

Finally, this distinction also has a terminological aspect. It is easier to say 'minimize a function' or 'extremize a function' (where 'extremize' means 'minimize or maximize'), than to say 'render a function stationary': there is no English word 'stationarize'! So despite the remarks above, I shall from now on (for Hamilton's Principle as well as the principle of least action) usually say 'minimize' or 'extremize': in fact, this is a widespread practice in the textbooks. But 'stationarize' is to be understood!

This principle of least action for a single particle is a remarkable principle—and a fine example of (Reformulate). But much more is true: the principle can be extended to systems with an arbitrary number N of particles. Here, I do not just mean the trivial N-fold conjunction of the one-particle principle, which follows immediately if we assume no particle-particle interactions (and no collisions in the time-period).[43] I mean, rather, that for any system (i) which is conservative, and (ii) for which the constraints, if any, are ideal, holonomic and scleronomous (so that we can work in the constraint surface): a corresponding principle holds.

This will be stated very precisely in Section 4.6. For the moment, we only need the main idea, that:

> For such a system, whatever the details of the interactions between its parts (encoded in V), the representative point in configuration space moves along the curve between given initial and final configurations that makes stationary (in comparision with neighbouring curves) the time-integral along the curve of the total kinetic energy T.

This is a very striking, even amazing, principle; both technically and philosophically. Technically, one can apply the calculus of variations to deduce the corresponding Euler-Lagrange equations; (Section 4.2 gives more explanation). Indeed, Euler and Lagrange did just this, obtaining the correct equations of motion for conservative systems that, if constrained, have ideal, holonomic and scleronomous constraints.

As regards philosophy, there are three immediate comments. The first concerns my morals; the other two are more general, and have a historical aspect.

(1): *Morals*:—
Clearly, the principle is a fine illustration of (Scheme), and the merit (Fewer). (Once

[43] That is:— Given a system of N point-particles, at spatial points $P_{1_1}, \ldots, P_{1_N}$ at time t_1, subject to an external potential $V(\mathbf{r})$, and with given initial velocities $\mathbf{v}_1, \ldots, \mathbf{v}_N$ (and so initial kinetic energies $T_1(t_1), \ldots, T_N(t_1)$); and given N points $P_{2_1}, \ldots, P_{2_N}$ (mutually distinct and in general distinct from $P_{1_1}, \ldots, P_{1_N}$), through which, respectively, the N particles later pass (with no collisions): for each particle i, the actual path traversed will be the path connecting P_{i_1} to P_{i_2}, motion along which, with a fixed initial energy $E_i = T_i(t_1) - V(P_{i_1})$ common to all comparison paths, makes the time-integral of T_i, along the path, stationary.

Section 4.2 connects it with the Euler-Lagrange equations, we shall also see the other merits, (Wider) etc.; and the moral (Reformulate).)

The principle is also a fine illustration of (Modality). Recall that Section 2.2.1 distinguished three broad grades of modal involvement: (Modality;1st) to (Modality;3rd). In (Modality;1st) we keep fixed the problem and laws of motion, but vary the initial and/or final conditions;[44] while in (Modality;2nd) we consider various problems but again keep fixed the laws of motion; and in (Modality;3rd) we vary the laws of motion in the sense that we consider histories that violate the actual laws (for the given forces, i.e. problem).

Broadly speaking, the principle of least action illustrates all three grades, though there is a minor wrinkle about (Modality;1st).

Applying the principle to a given problem obviously involves (Modality3rd): most of the various counterfactual histories, along the paths not traversed, are contralegal. And the principle itself clearly involves (Modality;2nd), since it generalizes across a whole class of problems.

The wrinkle about (Modality;1st) is that, despite the variety of positions and speeds at intermediate times (i.e. times after t_1 but before arrival at P_2), the principle does *not* vary the initial or final conditions in the sense of position and *speed*. The reason is that

(i): the given initial position P_1, and so $V(P_1)$, and velocity $\mathbf{v}$ determine a total energy E; (indeed, to determine E, one needs only $V(P_1)$ and the speed v); and

(ii): the conservation of the energy E, together with the given final position P_2, and so $V(P_2)$, determines the speed with which the particle would arrive at P_2 along any path—whether the unique dynamically allowed one, or another one.

Nevertheless the principle illustrates (Modality;1st). For the variety of paths makes for a variety of (both initial and final) velocity or momentum, though not of speed—and that variety counts as varying the initial and final conditions.

(2): *Other formulations*:—
Historically, the principle for a single particle was of course formulated first; and it was often formulated in terms of minimizing the integral along the path of the particle of the momentum $m\mathbf{v}$, with distance s as the integration variable. (Thus wrote Euler in 1744.) It was also formulated in terms of the integral of twice the kinetic energy $2T$, with time as the integration variable. These alternative formulations are trivially equivalent to the single-particle principle above. But they are historically important, because of the role they played in discussions of the relative dynamical importance of momentum and kinetic energy; in particular, in the controversy about *vis viva*, which was in effect defined as $2T$. I shall not need the first alternative, using $\int m\mathbf{v}\, ds$, at all. But in Section 4.6, I shall recover the second (with $2T$) from Hamilton's Principle.

[44] In some usages of the word 'problem', varying the initial conditions would count as varying the problem. But not mine: in Section 2.2.1, I stipulated that a problem is specified by the number of degrees of freedom and the forces involved, here coded in the potential function; (and more generally in a Lagrangian or Hamiltonian).

(3): *Teleology foresworn*:—
The principle's reference to the final configuration suggests teleology and final causes: that the values attained by the integral at the end of various possible trajectories through configuration space somehow determines (or even: causes or explains) *from the start* which trajectory is traversed. (This is clearest for our first case, the single particle: it looks as if the particle's final position determines which path it takes to that position.)

This suggestion is of course not peculiar to this principle. It arises for any variational principle that fixes a final condition (in time), and so it is endemic in analytical mechanics. In particular, we will see that Hamilton's Principle similarly refers to the final configuration of the system. Accordingly, this aspect of analytical mechanics has been much discussed. Indeed, teleology was the dominant topic in philosophical discussion of variational principles, from the beginnings in the eighteenth century (with Maupertuis' theological arguments, mentioned above), to Planck's advocacy of them in the late nineteenth century: a dominance which helps explain the logical empiricists' strong rejection of variational principles. (For discussion and references, cf. Yourgrau and Mandelstam (1979: 163-165, 173-175) and Stöltzner (2003).)

But I shall not pursue this topic, for two reasons. First, it would involve discussion of causation and explanation—large subjects beyond my scope. Second and more important: there is a strong reason to reject an interpretation of the principle, and of other final-condition variational principles in analytical mechanics, in terms of final causes—as against one in terms of efficient causes. This reason is based on the formalism of mechanics, and does not depend on any general philosophical objections to final causes.

Namely: given such a principle, the calculus of variations deduces a set of differential equations, the corresponding Euler-Lagrange equations, which are (in the cases we will consider) *equivalent* to the principle; and these equations suggest an interpretation in terms of efficient causes. In particular, we will see that Lagrange's equations in their most familiar form, eq. 3.39, are equivalent to Hamilton's Principle for a holonomic and monogenic system. Recalling that these equations are n second-order equations (for n degrees of freedom), and so need n positions and velocities as initial conditions (just like Newton's equations), we can surely regard the initial conditions, together with the equations, as determining (or causing or explaining) which trajectory in configuration space is traversed; (and so in particular, the value of the integral of T).

This interpretation is surely just as good as the one in terms of final causes. Besides, we can similarly defend an efficient-cause interpretation for principles other than the principle of least action and Hamilton's; (though I will not go into details).[45]

To sum up this Subsection: Euler and Lagrange discovered that for any conservative system that, if constrained, has ideal, holonomic and scleronomous constraints: two

[45]I claim no originality for appealing to the Euler-Lagrange equations to suggest an efficient-cause interpretation of variational principles, as a reply to the proposed final-cause interpretations. This appeal is common enough; e.g. Torretti (1999: 92). But so far as I know, my emphasis on modality, here and in (2004e), is novel.

functions, T and V, determine the motion of the system, by a principle that selects the actual motion from a whole class of conceivable motions. Note that the motions in the class have a common fixed start-time t_1, common initial and final configurations, and a common fixed energy (and so a common $T(t_1)$). So the times of arrival at the final configuration *vary*. This will *not* be so for Hamilton's Principle.

4.1.2 Hamilton

Hamilton's Principle replaces Euler and Lagrange's "common-energy, varying arrival times" variation, with a variation that has "varying energies, common arrival time". But as for Euler and Lagrange, the variations have fixed initial and final configurations (i.e. spatial positions P_1, P_2 for one particle, and $\{P_{i_1}; P_{i_2}\}$ for N particles).

With this kind of variation, it turns out that for many kinds of system, just *one* function determines the motion, again *via* a variational principle that selects the actual motion by comparison with a class of "nearby" motions. In particular, for a conservative system that, if constrained, has ideal and holonomic constraints, the function is the Lagrangian, the difference $L := T - V$ of the total kinetic and potential energies of the system.

That is: Hamilton's Principle for such a system, consisting of N particles, says:

> The actual motion between a configuration $\{P_{i_1}\}$ at time t_1 and $\{P_{i_2}\}$ at time t_2 will be the motion that makes stationary the time-integral, along the trajectory in configuration space, of $L := T - V$.

This calls for two immediate comments. The first concerns my morals; the second is about Hamilton's Principle's advantages over the principle of least action.

(1): *Morals*:—
Like the principle of least action, Hamilton's principle is a fine illustration of (Scheme), and (Fewer); and we will later see the other merits, (Wider) etc.

And again like the principle of least action, Hamilton's Principle illustrates all three grades of modal involvement. But the details about (Modality;1st) and (Modality;3rd) are a bit different from the case of the principle of least action, because now the various histories have varying energies and a common arrival time. There are two points here.

First: even for a single problem (specified by a number of degrees of freedom and the forces), the counterfactual histories have to have different energies one from another, in order for them to have a common arrival time; e.g. a constituent particle could have different initial speeds in two histories. This illustrates (Modality;1st) and (Modality;3rd), as the principle of least action did. But in so far as one finds it unnatural in counterfactual suppositions to fix some future actual fact (arrival time), and thereby have to counterfactually vary present facts (energy, speed)—rather than *vice versa*—one will find Hamilton's Principle's (Modality;1st) more "radical" than the principle of least action's.

Second: for Hamilton's Principle, the energy is in general not preserved (constant)

within a counterfactual history—even for a system that actually obeys the conservation of energy, i.e. a conservative scleronomous system. Indeed, for any problem with prescribed initial and final conditions: almost all (in a natural measure) of the histories considered will violate energy-conservation. This is because Hamilton's principle considers all smooth curves in configuration space that are close to the actual trajectory; (details in Section 4.2).

(1): *Advantages*:—
I emphasise the three main advantages of Hamilton's Principle over Euler and Lagrange's principle of least action. In ascending order of importance, they are:–

(i): It encompasses the principle of least action, in two senses. First, it immediately yields the Euler-Lagrange equations, eq. 3.39—describing conservative systems, whose constraints, if any, are ideal, holonomic and scleronomous—that the principle of least action also obtains; cf. Section 4.2. Second, it *explains* how the principle of least action obtains those equations; cf. Section 4.6.

(ii) It can be extended to other kinds of system, even some non-holonomic systems. For some of these kinds, it again uses as its integrand $L = T - V$ (Section 4.2); for others, it uses an analogous integrand; (cf. Section 4.3).

(iii) It leads to Hamilton's equations, and thereby to Hamiltonian mechanics; which have various advantages over Lagrange's equations, and indeed Lagrangian mechanics; cf. the companion paper.

The first two advantages will be spelt out in the sequel. I begin with an exact statement of Hamilton's Principle.

4.2 Hamilton's Principle for monogenic holonomic systems

Beware: this Section's title is somewhat misleading, for two reasons. First, the discussion is as usual restricted to ideal constraints, not just to monogenic and holonomic systems.

Second and more important: 'monogenic' is a slight misnomer. For one of my main points will be that Hamilton's Principle with $L = T - V$ as integrand is equivalent to Lagrange's equations in the familiar form eq 3.39. As discussed in Section 3.3, these equations are physically correct, i.e. follow from d'Alembert's principle, not only when the potential V is time-independent and velocity-independent (i.e. conservative systems), or when V is time-dependent but velocity-independent, but *also* in some cases of velocity-dependence, e.g. when eq. 3.40 holds. Such conditions are a mouthful to say; and this Section's title abbreviates that mouthful in the somewhat more general word 'monogenic'. But this inaccuracy will be harmless.

Thus understood, Hamilton's Principle for a monogenic holonomic system says:—

The motion in configuration space between prescribed configurations at

time t_1 and time t_2 makes stationary the line integral

$$I = \int_{t_1}^{t_2} L(q_1, \ldots, q_n, \dot{q}_1, \ldots, \dot{q}_n, t)\, dt \tag{4.1}$$

of the Lagrangian $L := T - V$. (Note the inclusion of time t as an argument to allow V to be time-dependent.)

Hamilton's Principle (in this form) is a necessary and sufficient condition for Lagrange's equations, i.e. eq. 3.39: ((Scheme) with merits (Fewer) and (Wider), again). I will not prove necessity; (for this, cf. e.g. Whittaker (1959: Section 99: 245-247) and Lanczos (1986: 58-59, 116)). But I show sufficiency, since the argument:

(i) simply applies the basic result of the calculus of variations, that the unconstrained stationarity of an integral requires the Euler-Lagrange equations; and

(ii) makes clear that the Principle involves (Modality;3rd), as announced in Section 4.1.2.

The basic result of the calculus of variations:—
The variation, with fixed end-points, of an integral

$$J = \int_{x_1}^{x_2} f(y_1(x), \ldots, y_n(x), \dot{y}_1(x), \ldots, \dot{y}_n(x), x)\, dx \tag{4.2}$$

(where the dot $\dot{}$ indicates differentiation with respect to x) is obtained by considering J as a function of a parameter α which labels the possible curves $y_i(x, \alpha)$. We take $y_1(x, 0), y_2(x, 0), \ldots$ as the solutions of the stationarity problem; and we let $\eta_1(x), \eta_2(x), \ldots$ be arbitrary functions except that they vanish at the end-points, i.e. $\eta_i(x_1) = \eta_i(x_2) = 0 \ \ \forall i$. Then we write:

$$y_i(x, \alpha) = y_i(x, 0) + \alpha \eta_i(x) \ \ \forall i. \tag{4.3}$$

(Such variations can be analysed using the idea of a functional derivative, and for infinite i.e. continuous systems they need to be; but I shall not need that idea.)

The condition for stationarity is then that

$$\left(\frac{\partial J}{\partial \alpha}\right)_{\alpha=0} = 0; \tag{4.4}$$

and the variation of J is given in terms of that of α by

$$\delta J = \frac{\partial J}{\partial \alpha} d\alpha = \int_{x_1}^{x_2} \Sigma_i \left(\frac{\partial f}{\partial y_i} \frac{\partial y_i}{\partial \alpha} d\alpha + \frac{\partial f}{\partial \dot{y}_i} \frac{\partial \dot{y}_i}{\partial \alpha} d\alpha \right) dx. \tag{4.5}$$

Integrating by parts, for each i, the second term in the integrand, and using the fact that the variations $\delta y_i = (\frac{\partial y_i}{\partial \alpha})_0 d\alpha$ are independent, we get that $\delta J = 0$ only if

$$\frac{\partial f}{\partial y_i} = \frac{d}{dx} \frac{\partial f}{\partial \dot{y}_i} \quad \forall i = 1, \ldots, n; \tag{4.6}$$

which are called the *Euler-Lagrange* equations. (They first occur in a 1736 paper of Euler's. But the δ-notation and this neat deduction is due to Lagrange: he developed his approach in letters to Euler from 1754, but first published it in 1760: Kline (1972: 582-589), Fraser (1983, 1985).)

We remark that for a variational principle that uses fixed end-points, the integrand is undetermined up to the total derivative with respect to the independent variable x of a function $g(y)$. That is: suppose we are given a variational principle $\delta J := \delta \int f dx = 0$, and accordingly its Euler-Lagrange equations. Then exactly the same Euler-Lagrange equations would arise from requiring instead $\delta J' := \delta \int [f + \frac{dg}{dx}] dx = 0$. For the end-points being fixed means that $\delta \int \frac{dg}{dx} dx = \delta[g(x_2) - g(x_1)] \equiv 0$; so that $\delta J = 0$ iff $\delta J' = 0$.

Though simple, this result is important: it corresponds to the result in Paragraph 3.3.2.D that Lagrangians differing by a total time derivative determine identical Lagrange's equations. (It also underlies the idea of generating functions for canonical transformations—developed in the companion paper.)

Now I return to mechanics. Consider a monogenic holonomic system: Hamilton's system implies, by the above argument but with the substitutions

$$x \to t \ ; \ y_i \to q_i \ ; \ f \to L, \tag{4.7}$$

Lagrange's equations (3.39), i.e.:

$$\frac{\partial L}{\partial q_i} = \frac{d}{dt} \frac{\partial L}{\partial \dot{q}_i} \ \forall i = 1, \ldots, n. \tag{4.8}$$

The use of arbitrary functions η in the variation problem means that the Principle mentions contralegal histories, illustrating (Modality;3rd).

Note that the assumption of holonomic constraints is *used* in the argument; for we appeal to independent variations δq_i to get eq. 4.8, in the way we got eq. 4.6 from eq. 4.5. (For the modification of Hamilton's Principle to cover non-holonomic systems, see the next Subsection.)

On the other hand, the argument proceeds independently of how L is defined, and so of the assumption of monogenicity. The role of this assumption is, rather, to limit of the scope of Hamilton's Principle, for the sake of empirical correctness (cf. Lanczos 1986: 114).

This deduction of Lagrange's equations from Hamilton's Principle implies that they have an important property. Namely, they are covariant under coordinate transformations (point-transformations) $q_j \to q'_j$; (the merit (Wider)). For since the stationarity of a definite integral is *ipso facto* independent of a change of the independent variable, $q_j \to q'_j$, deducing Lagrange's equations from Hamilton's Principle implies the covariance of the equations (a covariance that holds even if the point-transformation $q_j \to q'_j$ is time-dependent). (This property also followed from the deduction from d'Alembert's principle; cf. Paragraph 3.3.2.C.)

I end this Section by discussing the relation between Hamilton's Principle and d'Alembert's principle. As I mentioned at the start of Section 3.3.1, one can deduce Hamilton's Principle from d'Alembert's principle. More precisely, one can deduce Hamilton's Principle in the above form—for a holonomic system whose applied forces are monogenic with their work function U independent of velocities—from d'Alembert's principle for such a system. Besides, the deduction can be reversed: this is an equivalence, illustrating (Reformulate).

Lanczos (1986: 111-113) gives details of this. So does Arnold (1989: 91-95). Arnold's discussion has the merit that it also formulates an equivalence with the conception of a constrained system as a limit (cf. eq. 3.43 in Paragraph 3.3.2.A)—(Reformulate) again! But Arnold also assumes conservative systems, and makes some use of modern geometry. I will just summarize Lanczos' deduction in the first direction, i.e. from d'Alembert's principle to Hamilton's Principle.

The idea is to overcome the intractable because polygenic character of the inertial forces, by representing them by the kinetic energy T (and by boundary terms that, by the variation in Hamilton's principle having fixed end-points, are equal to zero). One integrates, with respect to time, the total virtual work δw done by what Section 3.3.1 called the 'effective forces': i.e. the work done by the difference between the applied force on particle i, $\mathbf{F}_i$ and the rate of change of i's momentum, $\dot{p}_i$, summed over i.

d'Alembert's Principle sets this total virtual work δw equal to 0. So we write, summing over particles i and working in cartesian coordinates:

$$\int \delta w \; dt = \int \Sigma \left(\mathbf{F}_i - \frac{d}{dt}(m_i \mathbf{v}_i) \right) \cdot \delta \mathbf{r}_i \; dt = 0 \tag{4.9}$$

Assuming that the $\mathbf{F}_i$ are monogenic with their U independent of velocities, and setting $V = -U$, we deduce (by integrating by parts) that

$$\int \delta w \; dt = \delta \int L \; dt - [\Sigma \; m_i \mathbf{v}_i \cdot \delta \mathbf{r}_i]_{t_1}^{t_2} = 0 \text{ where } L := T - V. \tag{4.10}$$

Then requiring that the $\delta \mathbf{r}_i$ vanish at the end-points of the integration makes the right-hand side the variation of a definite integral: i.e.

$$\int \delta w dt = \delta \int L \; dt = 0 \text{ with } L := T - V. \tag{4.11}$$

Finally, the assumption that the system is holonomic (i.e. q_j freely variable) means that the variations in this last equation match those of Hamilton's principle.[46]

4.3 Extending Hamilton's Principle

In this Subsection, I discuss (simplifying Goldstein et al (2002: 46f.)) how Hamilton's Principle can be extended to some kinds of non-holonomic system. But *warning*: this

[46] By the way: the boundary term that is here zero will later be very important in Hamiltonian mechanics and Hamilton-Jacobi theory. It is essentially the canonical one-form I mentioned before.

Section can be skipped, in that its material is not central to the rest of this paper. However, the topic does illustrate the merit (Reduce); and it involves an important application of Lagrange's method of undetermined multipliers, viz. its application to extremizing integrals. I begin with an interlude about this; (for more details, cf. Lanczos (1986: 62-66)).[47]

4.3.1 Constrained extremization of integrals

Recall from Section 3.2.2 the main idea of Lagrange's method. We are to find the point $x_0 := (x_{0_1}, \ldots, x_{0_n})$ at which $\delta f = 0$ for small variations $x' - x_0$ such that $g_j(x') = g_j(x'_1, \ldots, x'_i, \ldots, x'_n) = 0$ for $j = 1, 2, \ldots, m$. So defining $h(x) = f(x) + \Sigma_j \lambda_j g_j(x)$, we have:

$$\delta h = \delta f + \Sigma_j \ \lambda_j \delta g_j = 0 \ \text{, i.e.} \quad \frac{\partial f}{\partial x_i} + \Sigma_j \ \lambda_j \frac{\partial g_j}{\partial x_i} = 0, \qquad i = 1, \ldots, n \tag{4.12}$$

We use these n equations, together with the m equations $g_j(x_i) = 0$ to find the $n + m$ unknowns (the n x_i and the m λ_j).

I now show how to apply this technique to the case where f is an integral; treating first (A) holonomic, then (B) non-holonomic, constraints. With an eye on applications to Hamilton's Principle, I assume the independent variable of the integral is time t, i.e. we are to extremize $f = \int F(q_i, \dot{q}_i, t)\, dt$, subject to some constraints. Also:

(1): I will now use qs not xs to emphasise that the coordinates need not be cartesian.

(2) Because of the constraints, the qs in this Subsection (unlike Section 4.2) are *not* independent.

(A): *Holonomic constraints*:— Assume that the constraints are holonomic, so that we have equations $g_j(q_i) = 0$ for $j = 1, \ldots, m$. Then variation of the constraint equations gives

$$\delta g_j = \Sigma_i \frac{\partial g_j}{\partial q_i} \delta q_i \text{ for each } j = 1, \ldots, m \ . \tag{4.13}$$

Lagrange's method is to multiply each of these equations by an undetermined multiplier λ_j. But since these constraint equations are to hold for each t, each λ_j becomes an undetermined function of t, and an integral over t is added to the summation over j. So the condition for extremization (the variational principle) is:

$$\delta \int F \ dt + \int (\lambda_1 \delta g_1 + \ldots + \lambda_m \delta g_m)\ dt = 0. \tag{4.14}$$

[47] Also beware: Lanczos remarks (1986: 92, 114) that Hamilton's principle applies only to holonomic systems. That is wrong. Lanczos seems best interpreted as mis-reporting the *true* requirement (also stated by him, p. 112) on Hamilton's principle in the form using $L := T - V$, that the applied forces have a work function—so that we can write $V = -U$. The confusion arises because Lanczos also sometimes (e.g. p. 85) takes holonomic (respectively: non-holonomic) constraints to be maintained by monogenic (polygenic) forces. I deny that (cf. footnote 34 in Section 3.1.2). But even if it were true, it would be a point about the forces of constraint, not about the applied forces. So it would not make the true requirement above, that the applied forces have a work function, imply that Hamilton's Principle is restricted to holonomic systems.

Then the usual calculus of variations argument, using the fact that $F = F(q_i, \dot{q}_i, t)$ while each g_j is a function only of the q_i, leads to the Euler-Lagrange equations, for each $i = 1, \ldots, n$:

$$\frac{\partial F}{\partial q_i} - \frac{d}{dt}\left(\frac{\partial F}{\partial \dot{q}_i}\right) + \lambda_1 \frac{\partial g_1}{\partial q_i} + \ldots + \lambda_m \frac{\partial g_m}{\partial q_i} = 0. \tag{4.15}$$

In other words: the original variational problem of extremizing $\int F \; dt$ subject to $g_j(q_i) = 0$ is replaced by the equivalent problem of extremizing (subject to *no* constraints)

$$\int F + \lambda_1 g_1 + \ldots + \lambda_m g_m. \tag{4.16}$$

We have so far considered the λ_j as constants of the variation problem. But, as in Section 3.2.1, comment (1), after eq. 3.18: we do not have to do so; and if we vary the λ_j, we get back the constraint equations *a posteriori*.

(B): *Non-holonomic constraints*:— We now suppose that the constraints are non-holonomic, so that we do not have equations $g_j(q_i) = 0$ for $j = 1, \ldots, m$. But suppose that, as in Section 3.2.1, comment (2) and its eq. 3.19, we have differential constraint equations

$$\delta g_j = \Sigma_i \; G_{ji} \delta q_i = 0 \text{ for each } j = 1, \ldots, m. \tag{4.17}$$

Lagrange's method again applies. We get again eq. 4.15, but with factors G_{ji} (as in eq. 3.19) replacing the $\frac{\partial g_j}{\partial q_i}$ of eq. 4.15. That is, we get:-

$$\frac{\partial F}{\partial q_i} - \frac{d}{dt}\left(\frac{\partial F}{\partial \dot{q}_i}\right) + \lambda_1 G_{1i} + \ldots + \lambda_m G_{mi} = 0. \tag{4.18}$$

We will see this in more detail with eq. 4.19 et seq. below.

4.3.2 Application to mechanics

It is the second case, (B), that we need when formulating Hamilton's Principle for non-holonomic systems. I will consider only those non-holonomic systems whose equations of constraint can be expressed as a relation between the differentials of the q's, i.e. can be put in the form of say m equations

$$\Sigma_i \; a_{ji} dq_i + a_{jt} dt = 0, \quad j = 1, \ldots, m. \tag{4.19}$$

The variation considered in Hamilton's Principle holds constant the time, so that virtual displacements δq_i must satisfy

$$\Sigma_i \; a_{ji} \delta q_i = 0. \tag{4.20}$$

This implies that for any undetermined functions of time $\lambda_j(t)$, we have $\lambda_j \Sigma_i a_{ji} \delta q_i = 0$. We sum over j, and integrate over an arbitrary time interval, to get

$$\int_{t_1}^{t_2} \Sigma_{i,j} \; \lambda_j a_{ji} \delta q_i \; dt = 0. \tag{4.21}$$

We now assume the integrated statement of Hamilton's Principle in the form

$$\int_{t_1}^{t_2} dt \;\; \Sigma_i \left(\frac{\partial L}{\partial q_i} - \frac{d}{dt}\frac{\partial L}{\partial \dot{q}_i} \right) \delta q_i = 0; \tag{4.22}$$

and add the equations, so that

$$\int_{t_1}^{t_2} dt \;\; \Sigma_i \left(\frac{\partial L}{\partial q_i} - \frac{d}{dt}\frac{\partial L}{\partial \dot{q}_i} + \Sigma_j \; \lambda_j a_{ji} \right) \delta q_i = 0. \tag{4.23}$$

We can then choose the first $n - m$ of the δq_i freely (so that the last m are thereby fixed); and we can choose the λ_j so that:

$$\left(\frac{\partial L}{\partial q_i} - \frac{d}{dt}\frac{\partial L}{\partial \dot{q}_i} + \Sigma_j \; \lambda_j a_{ji} \right) = 0, \;\; \text{for } i = n - m + 1, \ldots, n; \tag{4.24}$$

(these are in effect equations of motion for the last m of the q_i). Then eq. 4.23 becomes

$$\int_{t_1}^{t_2} dt \;\; \Sigma_i^{n-m} \left(\frac{\partial L}{\partial q_i} - \frac{d}{dt}\frac{\partial L}{\partial \dot{q}_i} + \Sigma_j \; \lambda_j a_{ji} \right) \delta q_i = 0, \tag{4.25}$$

an equation which involves only the independent δq_i, so that we can deduce by the usual calculus of variations argument

$$\frac{\partial L}{\partial q_i} - \frac{d}{dt}\frac{\partial L}{\partial \dot{q}_i} + \Sigma_j \; \lambda_j a_{ji} = 0 \text{ for } i = 1, \ldots, n - m. \tag{4.26}$$

Putting this together with eq 4.24 (and with $\lambda \to -\lambda$), we have n equations (cf. eq. 4.18)

$$\frac{\partial L}{\partial q_i} - \frac{d}{dt}\frac{\partial L}{\partial \dot{q}_i} = \Sigma_j \; \lambda_j a_{ji} \;\; i = 1, \ldots, n. \tag{4.27}$$

There are altogether $n + m$ unknowns (the q_i and λ_j). The other m equations are the equations of constraint relating the q_i's, now considered as differential equations

$$\Sigma_i \;\; a_{ji}\dot{q}_i + a_{jt} = 0, \;\; j = 1, \ldots, m. \tag{4.28}$$

Finally, I note that the λ_j again have the physical interpretation discussed in Paragraph 3.2.2.B; (but now with $\lambda \to -\lambda$, and without the assumption of cartesian coordinates). This interpretation is related to the issue of justifying treating motion wholly within the constraint surface, first raised after eq 3.4—and so to (Reduce), and also (Ideal) and (Accept). For discussion, cf. the references given in Paragraph 3.2.2.B and Lanczos (1986: 141-145) who relates the interpretation to the fact that the constraints are microscopically violated.

So much by way of extending Hamilton's Principle to non-holonomic systems. NB: The rest of this Section will assume that the constraints, if any, are holonomic—and as usual, ideal.

4.4 Generalized momenta and the conservation of energy

The rest of this Section is dominated by one idea: *using a symmetry to reduce the number of variables of a problem.* First, in this Subsection, I introduce generalized momenta and discuss the conservation of energy. This is a preliminary to Section 4.5's result that the generalized momentum of any cyclic coordinate is a constant of the motion. Though very simple, that result is important: for it is the basis of both Routhian reduction and Noether's theorem—which will take up the rest of the Section.

In Section 3.3 we deduced the conservation of energy, for a (ideal and holonomic) system that is scleronomous in both work function and constraints, from d'Alembert's principle—by choosing virtual displacements $\delta \mathbf{r}_i$ equal to the actual displacements $d\mathbf{r}_i$ in an infinitesimal time dt. We can similarly deduce the conservation of energy for such a system from Hamilton's principle, by considering such a variation for the generalized coordinates q_j, i.e. with $dt = \epsilon$

$$\delta q_j = dq_j = \epsilon \dot{q}_j. \tag{4.29}$$

This deduction is important in that it introduces two notions which will be important in what follows.

Since this variation does not fix the configurations at the end-points, we get a boundary term when we perform the integration by parts in the usual derivation of the Euler-Lagrange equations (cf. eq. 4.5 in Section 4.2.). That is, we get from Hamilton's principle and an integration by parts

$$\delta \int_{t_1}^{t_2} L \, dt = \left[\Sigma_j \frac{\partial L}{\partial \dot{q}_j} \delta q_j \right]_{t_1}^{t_2} \tag{4.30}$$

Here we see for the first time two notions that will be central in the sequel.

(i): Generalized momenta:—
Elementary examples prompt the definition of the *generalized momentum*, p_j, *conjugate* to a coordinate q_j as: $\frac{\partial L}{\partial \dot{q}_j}$; (Poisson 1809). So Lagrange's equations for a holonomic monogenic system, eq. 4.8, can be written:

$$\frac{d}{dt} p_j = \frac{\partial L}{\partial q_j} \quad ; \tag{4.31}$$

and we can write eq. 4.30 as:

$$\delta \int_{t_1}^{t_2} L \, dt = [\Sigma_j p_j \delta q_j]_{t_1}^{t_2} \tag{4.32}$$

Note that p_j need not have the dimensions of momentum: it will not if q_j does not have the dimension length. And even if q_j is a cartesian coordinate, a velocity-dependent potential will mean p_j is not the usual mechanical momentum.

(ii): The differential $\Sigma_j p_j \delta q_j$, as in the right-hand side of eq. 4.32:—
As I have mentioned, this will be important in Hamiltonian mechanics.

If L does not depend on time explicitly, i.e. $L = L(q_j; \dot{q}_j)$, then using $dq_j = \epsilon \dot{q}_j \Rightarrow d\dot{q}_j = \epsilon \ddot{q}_j$, we get:

$$\delta L = dL = \epsilon \dot{L} \tag{4.33}$$

so that

$$\delta \int L \, dt = \int \delta L \, dt = \int \epsilon \dot{L} \, dt = \epsilon \left[L \right]_{t_1}^{t_2} . \tag{4.34}$$

Then eq. 4.32 implies

$$\left[\Sigma_j p_j \dot{q}_j - L \right]_{t_1}^{t_2} = 0. \tag{4.35}$$

Since the limits t_1, t_2 are arbitrary, we get

$$H := \Sigma_j p_j \dot{q}_j - L = \text{ constant} \tag{4.36}$$

This function H ('H' for 'Hamiltonian') can be called the 'total energy' of the system; (though as we shall see in a moment, only under certain conditions is it the sum of potential and kinetic energies).

We can also deduce directly from Lagrange's equations, rather than from Hamilton's Principle, that H is constant for a system that is both:

(i) monogenic and holonomic (so that Lagrange's equations take the familiar form 4.8), and

(ii) scleronomous, so that L is not an explicit function of time.

The deduction simply applies Lagrange's equations to the expansion of dL/dt:

$$\frac{dL}{dt} = \Sigma_i \frac{\partial L}{\partial q_i} \dot{q}_i + \frac{\partial L}{\partial \dot{q}_i} \ddot{q}_i. \tag{4.37}$$

We get immediately that

$$H := \Sigma_i \dot{q}_i p_i - L \tag{4.38}$$

is a constant of the motion.

Now let us add the assumption that the system is conservative in the sense that (the applied) forces are derived from potentials, and that the potentials are velocity-independent. Then, using again the assumption that the system is scleronomous, we can show that H is the sum of potential and kinetic energies—as follows.

These assumptions imply that $p_i := \frac{\partial L}{\partial \dot{q}_i} = \frac{\partial T}{\partial \dot{q}_i}$. Now recall from the discussion of equation 3.42, that the system being scleronomous implies that T is a homogeneous quadratic function of the $\dot{q}_i$'s. This means that the first term of H, i.e. $\Sigma_i \dot{q}_i p_i = \Sigma_i \dot{q}_i \frac{\partial T}{\partial \dot{q}_i}$ must be $2T$ (Euler's theorem). So we have:-

$$H = 2T - (T - V) = T + V. \tag{4.39}$$

4.5 Cyclic coordinates and their elimination

4.5.1 The basic result

We say a coordinate q_i is *cyclic* if L does not depend on q_i. (The term comes from the example of an angular coordinate of a particle subject to a central force. Another

term is: *ignorable*.) Then the Lagrange equation for a cyclic coordinate q_n say, viz. $\dot{p}_n = \frac{\partial L}{\partial q_n}$, becomes $\dot{p}_n = 0$, implying

$$p_n = \text{constant, } c_n \text{ say.} \tag{4.40}$$

So: *the generalized momentum conjugate to a cyclic coordinate is a constant of the motion.*

In other words, thinking of p_n as a function of the $2n+1$ variables $q, \dot{q}, t$, $p_n = p_n(q, \dot{q}, t)$, and using the terminology of Paragraph 2.1.3.A (iii): the motion of the system is confined to a unique level set $p_n^{-1}(c_n)$. And assuming scleronomous constraints, so that we work with the velocity phase space (tangent bundle) TQ: this level set is a $(2n-1)$-dimensional sub-manifold of TQ.

So finding a cyclic coordinate[48] simplifies the dynamical system, i.e. the problem of integrating its equations of motion. The number of variables (degrees of freedom of the problem) is reduced by one—the merit (Reduce) of my moral (Scheme).

This result is simple but important. For first, it is straightforward to show that it encompasses the elementary theorems of the conservation of momentum, angular momentum and energy; (Goldstein et al (2002: 56-63)). Let us take as a simple example, the angular momentum of a free particle. The Lagrangian is, in spherical polar coordinates,

$$L = \frac{1}{2}m(\dot{r}^2 + r^2\dot{\theta}^2 + r^2\dot{\phi}^2 \sin^2\theta) \tag{4.41}$$

so that $\partial L/\partial\phi = 0$. So the conjugate momentum

$$\frac{\partial L}{\partial \dot{\phi}} = mr^2\dot{\phi}\sin^2\theta \ , \tag{4.42}$$

which is the angular momentum about the z-axis, is conserved.

Secondly, this result leads into important theoretical developments about how to use symmetries, so as to simplify (reduce) problems. The rest of this Section is devoted to two such developments: in this Subsection, Routhian reduction; and later (Section 4.7), Noether's theorem. Noether's theorem will be more general in the sense that Routhian reduction requires us to have identified cyclic coordinates, while Noether's theorem does not.

4.5.2 Routhian reduction

My discussion of Routhian reduction has two main aims:

(i): in this Subsection, to illustrate using symmetries to secure (Reduce); and

[48]Beware: The condition $\partial L/\partial q_n = 0$ is of course a property not just of the coordinate q_n, but of the entire coordinate system, since partial derivatives depend on what other variables are held constant. So one can have another coordinate system q' such that $q'_n = q_n$ but $\partial L/\partial q'_n \neq \partial L/\partial q_n$. Besides, one can have $\partial L/\partial \dot{q}'_n \neq \partial L/\partial \dot{q}_n$, i.e. the momenta conjugate to $q'_n = q_n$ are distinct.

(ii) in Section 4.6, to vindicate Section 4.1.1's principle of least action from the perspective of Hamilton's Principle. (*Warning*: This use of Routhian reduction is a topic in the foundations of classical mechanics, not much related to my philosophical morals: it will not be used later, and can be skipped.)

So suppose q_n is cyclic. To exploit this fact so as to reduce the number of variables in a mechanical problem, we can proceed in either of two ways: (a) after writing down Lagrange's equations; or (b) before doing so. I treat these in order; Routhian reduction is (b).

(a): Notice first that since q_n does not occur in L, it does not occur in $\frac{\partial L}{\partial \dot{q}_n}$, so that we can solve $p_n = \frac{\partial L}{\partial \dot{q}_n} = c_n$ for $\dot{q}_n$ as a function of the other variables, i.e.

$$\dot{q}_n = \dot{q}_n(q_1, \ldots, q_{n-1}, \dot{q}_1, \ldots, \dot{q}_{n-1}, c_n, t) \tag{4.43}$$

and substitute the right-hand side of this into Lagrange's equations. This reduces the problem of integrating Lagrange's equations to a problem in $n-1$ variables. Once solved we can find $\dot{q}_n$ by eq. 4.43, and then find q_n by quadrature. (Recall that 'quadrature' is jargon for integration of a given function: if we cannot do the integral analytically, we do it numerically.)

(b): But we can also instead reduce the problem *ab initio*, i.e. when it is formulated as a variational problem. This is Routhian reduction. It is important both historically and conceptually. As to history, it was Routh who first emphasised the importance of cyclic coordinates (and his work led to e.g. Hertz' programme in mechanics). And as to conceptual aspects, Routhian reduction yields a proper understanding of how the principle of least action is based on Hamilton's Principle. Indeed, since the principle of least action preceded Hamilton's principle, and thus also this understanding, the historical and conceptual roles are related—as we shall see in the next Subsection, Section 4.6.[49]

Note first that we cannot just replace $\dot{q}_n$ in Hamilton's principle

$$\delta \int L(q_1, \ldots, q_{n-1}, \dot{q}_1, \ldots, \dot{q}_{n-1}, \dot{q}_n, t) = 0 \tag{4.44}$$

by the right-hand side of eq. 4.43. For eq. 4.43 makes $\dot{q}_n$ a function of the non-cyclic variables in the sense that $p_n \equiv \frac{\partial L}{\partial \dot{q}_n} = c_n$ is to hold not just for the actual motion but also for the varied motions. This restriction on the variations is not objectionable (since the integral's variation is to vanish for *arbitrary* variations). But the fact that q_n is obtained by a quadrature means that the variation of q_n does not vanish at the end-points. Rather we have (cf. eq 4.30):

$$\delta \int L \, dt = [p_n \delta q_n]_{t_1}^{t_2}. \tag{4.45}$$

[49]Beware: many textbooks (including fine ones like Goldstein et al) only treat Routhian reduction as an aspect of Hamiltonian theory: in short as involving a Legendre transformation on only the cyclic coordinates—details in the companion paper. That lacuna is another reason for describing Routhian reduction within the Lagrangian framework. My discussion will follow Lanczos (1986: 125-140).

But p_n is constant along the system's (actual or possible) trajectory in configuration space, so that

$$[p_n \delta q_n]_{t_1}^{t_2} = p_n \, \delta \int \dot{q}_n \, dt = \delta \int p_n \dot{q}_n \, dt \tag{4.46}$$

so that eq. 4.45 becomes

$$\delta \int (L - p_n \dot{q}_n) \, dt = 0. \tag{4.47}$$

To sum up: our problem is reduced to extremizing a modified integral in $n-1$ variables

$$\delta \int (L - c_n \dot{q}_n) \, dt = 0; \tag{4.48}$$

where q_n does not occur in the modified Lagrangian $\bar{L} := L - c_n \dot{q}_n$; *and nor does* $\dot{q}_n$ explicitly occur, since it is eliminated by using eq. 4.43, i.e. the momentum first integral $\frac{\partial L}{\partial \dot{q}_n} = c_n$.

Once this problem in $n-1$ variables is solved, we again (as at the end of (a) above) find $\dot{q}_n$ by using the momentum integral eq. 4.43; and then we find q_n by quadrature.

We can easily adapt the argument of (b) to the case where the given problem has more than one cyclic coordinate. The modified Lagrangian $\bar{L}$ subtracts the corresponding sum over cyclic coordinates:

$$\bar{L} := L - \Sigma_{k \text{ ignorable}} \; c_k \dot{q}_k \; . \tag{4.49}$$

Three final comments. (1): Anticipating the companion paper a little:— This kind of subtraction of $p_n \dot{q}_n$ from the Lagrangian will be crucial in the discussion of the Legendre transformation which carries us back and forth between the Lagrangian and Hamiltonian frameworks. This is the reason why, as mentioned above, Routhian reduction is often treated in textbooks just as an aspect of Hamiltonian theory: in short as involving a Legendre transformation on only the cyclic coordinates.

(2): This reduction has consequences for the form of the modified Lagrangian $\bar{L} := L - c_n \dot{q}_n$; (details in Lanczos: 128-132).

First: there is a new velocity-independent potential term. (An historical aside: This fostered Hertz's (1894) speculation that mechanics could be *forceless*, i.e. that all macroscopically observable forces are analysable in terms of monogenic forces whose potential energy arises in just this way from the elimination of cyclic microscopic coordinates. Hertz's proposal reflects, and contributed to, a long tradition of philosophical suspicion of forces. The root idea, present already in the seventeenth century, was that forces are unobservable while other quantities in mechanics, especially mass, length and time, are observable. For details, cf. Lutzen (1995).)

Second: in general, the reduction also gives new kinetic terms in $\bar{L}$ which are linear in the generalized velocities $\dot{q}_i, i = 1, 2, \ldots, n-1$ (called 'gyroscopic terms').

(3): Note that Routhian reduction assumes we have identified cyclic coordinates. In Section 4.7, Noether's theorem will provide a perspective on symmetry and constants of the motion that does not assume this.

4.6 Time as a cyclic coordinate; the principle of least action; Jacobi's principle

Warning: This Section can be skipped: it is not used later on. But I include it on two grounds:—

(i): It is worth seeing how to use Routhian reduction to understand the principle of least action (and another principle, Jacobi's), with which we began in Section 4.1.1.

(ii): It illustrates the idea of a parameter-independent integral, which came up in Paragraph 3.3.2.B, (2), in connection with the fact that the Hessian condition can fail; though this paper will not pursue the topic.

Any holonomic system whose Lagrangian does not contain time explicitly provides an important case of the theory of Section 4.5. For with such a system we can take the time itself as a cyclic coordinate. If furthermore the system is conservative (i.e. forces are derived from potentials, and the potentials are velocity-independent) and also the system is scleronomous, so that $\Sigma p_j \dot{q}_j = 2T$ (cf. end of Section 4.4), then the theory of Section 4.5 yields the principle of least action—i.e. the variational principle at the centre of Lagrange's and Euler's formulations of analytical mechanics.

But there are subtleties about this principle. As we shall see, the correct form of this principle is due to Jacobi. And on the other hand, we shall also be able to explain why earlier authors like Lagrange were able to get the right Euler-Lagrange equations for holonomic conservative scleronomous systems—even though some of these authors lacked Hamilton's Principle.

If the Lagrangian L of a holonomic system does not contain time explicitly, we can treat time t 'like the qs', to give a problem in $n+1$ variables. That is, we write the integral to be extremized (by Hamilton's principle) in terms of a differentiable parameter $\tau = \tau(t)$, which is such that $d\tau/dt > 0$ but is otherwise arbitrary; (for more discussion of why this can be useful, cf. e.g. Butterfield (2004c: Sections 5,7)). So with a prime indicating differentiation with respect to τ, and $dt \equiv \frac{dt}{d\tau} d\tau \equiv t' d\tau$, Hamilton's Principle becomes

$$\delta \int L \left(q_1, \ldots, q_n; \frac{q_1'}{t'}, \ldots, \frac{q_n'}{t'} \right) t' \, d\tau = 0. \tag{4.50}$$

So the generalized momentum conjugate to t must be conserved. One immediately calculates that this momentum is the negative of the Hamiltonian as defined in general by eq. 4.36. That is:

$$p_t := \frac{\partial (Lt')}{\partial t'} = L - \left(\Sigma_j \, \frac{\partial L}{\partial \dot{q}_j} \frac{q_j'}{t'^2} \right) t' = L - \Sigma_j \, p_j \dot{q}_j = \text{ constant.}^{50} \tag{4.51}$$

But now let us apply the theory of Section 4.5. That is: let us eliminate t, to get a reduced variational problem—which will determine the path in configuration space

[50] As we also saw in Section 4.4, if the system is conservative and scleronomous, the Hamiltonian is the sum of the kinetic and potential energies, so that we here get *yet another* derivation of the conservation of energy: (Reformulate) again!

without regard to the passage of time. The modified Lagrangian is:

$$\bar{L} := Lt' - p_t t' = \Sigma_j \; p_j \dot{q}_j \; t' \tag{4.52}$$

so that the reduced variational problem is

$$\delta \int \Sigma_j \; p_j \dot{q}_j \; t' \; d\tau = 0. \tag{4.53}$$

If furthermore the system is conservative and scleronomous, so that $\Sigma p_j \dot{q}_j = 2T$, we can write this as:

$$\delta \; 2 \int T t' \; d\tau = 0. \tag{4.54}$$

Eq. 4.54 is the principle of least action of Section 4.1.1. But as Jacobi emphasised, we should not write this

$$\delta \; 2 \int T \; dt = 0, \tag{4.55}$$

since the cyclic coordinate t obviously cannot be used as the independent variable of our problem: t does not occur in eq. 4.54.

However, eq. 4.55 *does* occur in the earlier authors using the principle of least action, e.g. Euler and Lagrange themselves.[51] At the end of this Subsection, I will report how the practice of Euler and his contemporaries was in fact justified, by using Lagrange's method of multipliers (the λ-method).

Let us now apply the theory of Section 4.5 to the principle of least action, i.e. to our modified Lagrangian problem eq. 4.54. We need to undertake two stages:

(i) to eliminate the corresponding velocity t', by solving for this velocity the equation saying that the corresponding momentum is constant, i.e. by solving eq. 4.51 for t';

(ii) to integrate the resulting equation for t', to find t.

As to stage (i), we use eq. 3.5 and 3.6. Recall that the latter is:

$$T = \frac{1}{2} \left(\frac{ds}{dt} \right)^2, \tag{4.56}$$

so that now using τ as independent variable,

$$T = \frac{1}{2} \left(\frac{ds}{d\tau} \right)^2 / t'^2. \tag{4.57}$$

This implies that solving the energy conservation equation, eq. 4.51, for t' yields

$$t' = \frac{1}{\sqrt{(2(E-V))}} \frac{ds}{d\tau} \tag{4.58}$$

[51] Arnold (1989: 246) joyously quotes Jacobi who says (in his *Lectures on Dynamics*, 1842-1843): 'In almost all textbooks, even the best, this principle is presented so that it is impossible to understand'. Arnold continues ironically: 'I do not choose to break with tradition. A very interesting "proof" of [the principle of least action] is in Section 44 of the mechanics textbook of Landau and Lifshitz.' Arnold's own exposition (1989: 242-248) is of course admirably lucid and rigorous. But it is abstract and hard, not least because it is cast within Hamiltonian mechanics.

Also eq. 4.57 implies that the principle of least action, eq. 4.54, can be written as:

$$\delta \int \sqrt{(2(E-V))}\frac{ds}{d\tau}d\tau = 0 \tag{4.59}$$

This form is known as *Jacobi's principle*. It determines the path in configuration space without regard to the passage of time. This completes stage (i).

NB: Though one might write Jacobi's principle as

$$\delta \int \sqrt{(2(E-V))}ds = 0, \tag{4.60}$$

beware that ds is not an exact differential, and is not the differential of the independent variable in eq. 4.59. *Some* parameter τ must be chosen as the independent variable, e.g. one of the q_j, say q_n, so that all the other q_j are functions of q_n: which clearly reduces the problem from n to $n-1$ degrees of freedom.

As to stage (ii), one integrates eq. 4.58 to get t as a function of τ. This determines how the motion through configuration space occurs in time.

I end this Subsection with four comments on the principle of least action. The first two are technical; but since they connect the principle of least action to the geometry of configuration space, they illustrate the moral (Reformulate), and the merit (Wider) of (Scheme). The third and fourth comments will return us to Section 4.1.1's treatment of the principle of least action.

(1): *The analogy with optics*:—
For a single particle, the line-element ds is just ordinary spatial distance (in arbitrary curvilinear coordinates). Jacobi's principle is then very like Fermat's principle of least time in geometric optics (mentioned in Paragraph 3.3.2.B, (2)), which determines the optical path by minimizing the integral

$$\int n\, ds \tag{4.61}$$

where n is the refractive index, which can change from point to point (cf. $\sqrt{(2(E-V))}$). This optico-mechanical analogy is deep and important: it plays a role both in Hamilton-Jacobi theory and in quantum theory. (Butterfield (2004c: Sections 7-9) gives details and references.) But here I just note that the analogy concerns the path, not how the motion occurs in time—which is different in the two cases.

(2): *Geodesics*:—
Eq. 4.60 suggests that we think of Jacobi's principle as determining the path in configuration space as the shortest path (geodesic), according to a new line-element, $d\sigma^2$ say, defined by

$$d\sigma^2 := (E-V)ds^2 \tag{4.62}$$

so that Jacobi's principle is the statement that the motion minimizes the integral $\int d\sigma$. Besides, if the system is free i.e. $V = 0$, $d\sigma^2$ and ds^2 just differ by a multiplicative constant E, and Jacobi's principle now says that the system travels a geodesic of the

original line-element ds^2 defined in terms of T. Furthermore, the conservation of energy and

$$E = T = \frac{1}{2}\left(\frac{ds}{dt}\right)^2 \tag{4.63}$$

implies that the representative point moves at constant velocity in configuration space.

(3): *Euler's and Lagrange's practice*:—
We have deduced the principle of least action, eq. 4.54, for holonomic conservative scleronomous systems, as an example of Routhian reduction applied to Hamilton's Principle. But now let us ask how Euler and Lagrange wrote down the principle of least action, and got the right Euler-Lagrange equations for such systems, without starting from Hamilton's Principle. The short answer is that Lagrange et al. regard the energy conservation equation eq. 4.58 as an auxiliary condition, and treat it by Lagrange's λ-method.

Thus recall that in essence, Jacobi's principle involves two steps:—
(A): In the kinetic energy, replace differentiation with respect to t by differentiation with respect to the parameter τ:

$$T' = \frac{1}{2}\Sigma\, a_{ik} q_i' q_k' = T t'^2 \ . \tag{4.64}$$

(B): Minimize the action integral (cf. eq. 4.54)

$$2\int \frac{T'}{t'} d\tau \tag{4.65}$$

after eliminating t' by using the energy relation (cf. eq. 4.58)

$$\frac{T'}{t'^2} + V = E. \tag{4.66}$$

Lagrange instead proposes to treat eq. 4.66 by his λ-method. So his integral to be extremized is

$$\int \left[2\frac{T'}{t'} + \lambda\left(\frac{T'}{t'^2} + V\right)\right] d\tau. \tag{4.67}$$

Since t' is one of our variables, we can find λ by minimizing with respect to t', getting

$$-\frac{2T'}{t'^2} - \frac{2\lambda T'}{t'^2} = 0; \quad \text{giving } \lambda = -t'. \tag{4.68}$$

Then the integral becomes

$$\int \left(\frac{T'}{t'^2} - V\right) t'\, d\tau = \int (T - V) t'\, d\tau. \tag{4.69}$$

But now that the variational problem is a free problem, i.e. has no auxiliary conditions, there is no reason *not* to use t as the independent variable. Doing so, we get

$$\int (T - V)\, dt. \tag{4.70}$$

Thus we are led back to Hamilton's Principle; and thus Lagrange et al. could obtain the usual, correct, equations of motion for holonomic conservative scleronomous systems from their principle of least action.

(4): *Two kinds of variation, revisited*:—
In Section 4.1.1, I introduced the principle of least action using a notion of variation different from the type I have considered throughout this Subsection. There I used a variation in which the energy H is conserved, so that the transit time varies from one path to another. Indeed, the principle of least action is often discussed using this kind of variation. But I shall not go into details, nor relate this notion of variation to comment (3) above; (for details, cf. Goldstein et al (2002: 356-362) or Arnold (1989: 242-248)).

4.7 Noether's theorem

4.7.1 Preamble: a modest plan

Any discussion of symmetry in Lagrangian mechanics must include a treatment of "Noether's theorem". The scare quotes are to indicate that there is more than one Noether's theorem. Quite apart from Noether's work in other branches of mathematics, her paper (1918) on symmetries and conservation principles (i.e. constants of the motion) in Lagrangian theories has several theorems. I will be concerned *only* with applying her first theorem to finite-dimensional systems. In short: it provides, for any symmetry of a system's Lagrangian (a notion I will define), a constant of the motion; the constant is called the 'momentum conjugate to the symmetry'.

I stress at the outset that the great majority of subsequent applications and commentaries (also for her other theorems, besides her first) are concerned with versions of the theorems for infinite (i.e. continuous) systems. In fact, the context of Noether's investigation was contemporary debate about how to understand conservation principles and symmetries in the "ultimate continuous system", viz. gravitating matter as described by Einstein's general relativity. This theory can be given a Lagrangian formulation: that is, the equations of motion, i.e. Einstein's field equations, can be deduced from a Hamilton's Principle with an appropriate Lagrangian. The contemporary debate was especially about the conservation of energy and the principle of general covariance (aka: diffeomorphism invariance). General covariance prompts one to consider how a variational principle transforms under spacetime coordinate transformations that are arbitrary, in particular varying from point to point. This leads to the idea of "local" symmetries, which since Noether's time has been immensely fruitful in both classical and quantum physics.[52]

So I agree that from the perspective of Noether's work, and its enormous later

[52] An excellent anthology of philosophical essays about symmetry is Brading and Castellani (2003): apart from its papers specifically about Noether's theorem, the papers by Wallace, Belot and Earman (2003) are closest to this paper's concerns.

development, this Section's application of the first theorem to finite-dimensional systems is, as they say, trivial. Furthermore, this application is easily understood, *without* having to adopt that perspective, or even having to consider infinite systems. In other words: its statement and proof are natural, and simple, enough that no doubt several nineteenth century masters of mechanics, like Hamilton, Jacobi and Poincaré, could recognize it in their own work—allowing of course for adjustments to modern language. In fact, versions of it for the Galilei group of Newtonian mechanics and the Lorentz group of special relativity were published a few years before Noether's paper; (Brading and Brown (2003: 90); for details, cf. Kastrup (1987)).[53]

Nevertheless, for this paper's purposes, it is worth expounding the finite-system version of Noether's first theorem. For it generalizes Section 4.5's result about cyclic coordinates (and so the elementary theorems of the conservation of momentum, angular momentum and energy which that result encompasses). There is also a pedagogic reason for expounding it. Many books (e.g. Goldstein et al 2002: 589f.) concentrate on the versions of Noether's theorems for infinite systems: for the reasons given above, that is understandable—but it can unwittingly give the impression that there is no version for finite systems. (Noether's theorem also has an important analogue in Hamiltonian mechanics.)

I should also give a warning at the outset about the sense in which Noether's theorem generalizes Section 4.5's result about cyclic coordinates. I said at the very end of Section 4.5 that (unlike Routhian reduction) the theorem does not assume we have identified cyclic coordinates.

Indeed so: but every symmetry in the Noether sense will arise from a cyclic coordinate in some system of generalized coordinates. In fact, this will follow from the Basic Theorem (what Arnold dubs the "rectification theorem') of the theory of ordinary differential equations; cf. Paragraph 2.1.3.A (i).

So the underlying point here will be the important one we have seen before. Namely: the Basic Theorem secures the existence of a coordinate system in which "locally, the problem is completely solved": i.e., n first-order ordinary differential equations have, locally, $n-1$ functionally independent first integrals. But that does not mean it is easy to find the coordinate system!

In other words: as I emphasised already in Section 2.1.4: analytical mechanics provides no "algorithmic" methods for finding the best coordinate systems for solving problems. In particular, Noether's theorem, for all its power, will not be a magic device for finding cyclic coordinates!

My own exposition of the theorem is a leisurely pedagogic expansion of Arnold's concise geometric proof (1989: 88-89).[54] This will involve, following Arnold, two main

[53] Here again, 'versions of it' needs scare-quotes. For in what follows, I shall be more limited than these proofs. I limit myself, as I did in Paragraph 3.3.2.E, both to time-independent Lagrangians and to time-independent transformations: so my discussion does not encompass boosts.

[54] Other brief expositions of Noether's theorem for finite-dimensional systems include: Desloge (1982: 581-586), Lanczos (1986: 401-405: emphasizing the variational perspective) and Johns (2005: Chapter 13).

limitations of scope:

(i): I limit myself, as I did in Paragraph 3.3.2.E, both to time-independent Lagrangians and to time-independent transformations. Formally, this will mean L is a scalar function on the $2n$-dimensional velocity phase space (aka: tangent bundle) TQ coordinatized by $q, \dot{q}$: $L : TQ \to \mathbb{R}$.

(ii): I will take a symmetry of L (or L's being invariant) to require that L be the *very same*. That is: a symmetry does not allow the addition to L of the time-derivative of a function $G(q)$ of the coordinates q—even though, as discussed in Paragraph 3.3.2.D (and Section 4.2), such a time-derivative makes no difference to the Lagrange (Euler-Lagrange) equations.

So my aims are modest. Apart from pedagogically expanding Arnold's proof, my only addition will be to contrast with (ii)'s notion of symmetry, *another* notion. Although this notion is *not* needed for my statement and proof of Noether's theorem, it is so important to this paper's theme of general schemes for integrating differential equations (from Section 2.1.1 onwards!) that I must mention it briefly.

This is the notion of a symmetry of the set of solutions of a differential equation: (aka: a dynamical symmetry). This notion applies to all sorts of differential equations, and systems of them; not just to differential equations of this paper's sort—i.e. derived, or derivable, from an variational principle. In short, this sort of symmetry is a map that sends any solution of the given differential equation (in effect: a dynamically possible history of the system—a curve in the state-space of the theory) to some other solution. Finding such symmetries, and groups of them, is a central part of the modern theory of integration of differential equations (both ordinary and partial). (This notion, and the theory based on it, were pioneered by Lie.)

It will turn out that broadly speaking, this notion is more general than that of a symmetry of L (the notion needed for Noether's theorem). Not only does it apply to many other sorts of differential equation than the Euler-Lagrange equations. Also, for the latter equations: a symmetry of L is (with one *caveat*) a symmetry of the solutions, i.e. a dynamical symmetry—but the converse is false.

An excellent account of this modern integration theory, covering both ordinary and partial differential equations, is given by Olver (2000). He also covers the Lagrangian case (Chapter 5 onwards), and gives many historical details about Lie's and others' contributions.

The plan is as follows. Starting from cyclic coordinates, I first develop the idea of the Lagrangian being invariant under a transformation (Section 4.7.2). This leads to defining:

(i): a *symmetry* as a vector field (on configuration space) that generates a family of transformations under which the Lagrangian is invariant (Section 4.7.3);

(ii): the *momentum conjugate to a vector field*, as (roughly) the rate of change of the Lagrangian with respect to the $\dot{q}$s in the direction of the vector field.

Together, these definitions lead directly to Noether's theorem (Section 4.7.5): that the momentum conjugate to a symmetry is a constant of the motion. One might guess, in the light of the rough definitions just given in (i) and (ii), that proving this statement

promises to be easy work. And so it is: after all the stage-setting, the proof in Section 4.7.5 will be a one-liner application of Lagrange's equations.[55]

4.7.2 From cyclic coordinates to the invariance of the Lagrangian

I begin by restating Section 4.5's result that the generalized momentum conjugate to a cyclic coordinate is constant, in terms of coordinate transformations. This leads to the correspondence between passive and active transformations, and between their associated definitions of 'invariance'.

The passive-active correspondence is of course entirely general: it applies to any space on which invertible differentiable coordinate transformations are defined. But it is a notoriously muddling subject and so worth expounding *slowly*! And although my exposition will be in the context of cyclic coordinates in Lagrangian mechanics, it will be clear that the correspondence is general.

So let q_n be cyclic, and consider a coordinate transformation $q \to q'$ that just shifts the cyclic coordinate q_n by an amount ϵ:

$$q'_i := q_i + \epsilon\delta_{in} \quad \dot{q}'_i = \dot{q}_i \ . \tag{4.71}$$

So to write the Lagrangian in terms of the new coordinates, we substitute, using the reverse transformation, i.e.

$$L_\epsilon(q', \dot{q}', t) = L(q(q', t),\ \dot{q}(q', t),\ t) \ . \tag{4.72}$$

That is: the Lagrangian is a *scalar* function on the space with points labelled $(q, \dot{q}, t)$. (In Paragraph 3.3.2.B, we called this the 'extended velocity phase space', or without the time argument, just 'velocity phase space'; in the geometric description of Paragraph 3.3.2.E, we called it the 'tangent bundle'.) It is just that we use L_ϵ to label its functional form in the new coordinate system.

Now we let ϵ vary, so that we have a one-parameter family of transformations. Differentiating L_ϵ with respect to ϵ, we get using the chain rule

$$\frac{\partial L_\epsilon}{\partial \epsilon} = \Sigma_i \frac{\partial L}{\partial q_i}\frac{\partial q_i}{\partial \epsilon} + \Sigma_i \frac{\partial L}{\partial \dot{q}_i}\frac{\partial \dot{q}_i}{\partial \epsilon} = \Sigma_i \ - \frac{\partial L}{\partial q_i}\delta_{in} = -\frac{\partial L}{\partial q_n} \ . \tag{4.73}$$

Now we use the fact q_n is cyclic. This implies that eq. 4.73 is equal to zero:

$$\frac{\partial L_\epsilon}{\partial \epsilon} \equiv -\frac{\partial L}{\partial q_n} = 0 \ ; \tag{4.74}$$

and thus that the Lagrangian has the same functional form in the new coordinates as in the old ones: which we can express as

$$L_\epsilon(q, \dot{q}, t) = L(q, \dot{q}, t) \ ; \quad \text{or equivalently} \quad L_\epsilon(q', \dot{q}', t) = L(q', \dot{q}', t) \ . \tag{4.75}$$

[55] If in (ii), the conjugate momentum had been defined simply as the rate of change of the Lagrangian in the direction of the vector field, then of course the theorem would be truly trivial: it would not require Lagrange's equations.

So far we have viewed the coordinate transformation in the usual way, as a *passive* transformation: cf. the comment after eq. 4.72. But for Noether's theorem, we need to express these same ideas in terms of active transformations. So I will now describe the usual correspondence between passive and active transformations, in the context of the velocity phase space.

(Again, I stress that my discussion assumes time-independent Lagrangians and transformations; so I will not work with extended velocity phase space. But some of the following discussion could be straightforwardly generalized to that context. For example, the vector fields X that in the next Subsection will represent symmetries would become time-dependent vector fields, so that their rates of change pick up a partial time-derivative $\frac{\partial X}{\partial t}$.)

I will temporarily label the velocity phase space S (for 'space'). And let us write s for a point (however coordinatized) in S, and indicate scalar functions on S (however expressed in terms of coordinates) by a *bar*. So the Lagrangian is a scalar function $\bar{L} : s \in S \mapsto \bar{L}(s) \in \mathbb{R}$.

Furthermore, in this Subsection's discussion of the corresponding active transformations, the distinction between the qs and the $\dot{q}$s will play no role. As in the discussion above, we will start with a passive coordinate transformation on S (both qs and $\dot{q}$s) that is induced by a passive coordinate transformation on just the configuration space (just qs); and then we will define a corresponding active transformation. But since the $q - \dot{q}$-distinction plays no role—the $\dot{q}$s just "carry along" throughout—it will be clearest to temporarily drop the $\dot{q}$s from the notation. So in this Subsection, when I talk of a passive $q \to q'$ coordinate transformation inducing an active one, you can think indifferently of there being:

either (i) n qs on configuration space, with the discussion "lifting" to the $2n$-dimensional S,

or (ii) $2n$ qs on S, so that the $q \to q'$ transformation need not be induced by a transformation on just the n-dimensional configuration space.

In the next Subsection, the $q - \dot{q}$-distinction will of course come back into play.

A passive coordinate transformation $q \to q'$ defines an active transformation, θ say, as follows. We will allow the coordinate transformation to be local, i.e. defined only on a patch (to be precise: an open subset) U of S. Then $\theta : U \to U$ is defined by the rule that for any $s \in U$ the coordinates of $\theta(s)$ in the q'-system are to be the same numbers as the coordinates of s in the q-system; that is

$$q'_i(\theta(s)) = q_i(s) \quad \text{for all } i \, . \tag{4.76}$$

This definition implies that θ's functional form in the q'-system is the transformation $q' \to q$, i.e. the inverse of our original coordinate transformation. That is: $\theta : s \mapsto \theta(s)$ is expressed in the q'-system by

$$q'(s) \mapsto q'(\theta(s)) = q(s) \ . \tag{4.77}$$

On the other hand, a scalar function $\bar{L} : U \to \mathbb{R}$ can be "dragged along" by composition with θ. That is, we define $\bar{L} \circ \theta : s \in U \mapsto \bar{L}(\theta(s)) \in \mathbb{R}$.

Putting these points together, we deduce that the functional forms of $\bar{L}$ and $\bar{L} \circ \theta$, in the coordinate systems q and q' respectively, *match.*

That is: let the functional form of $\bar{L}$ in the q-system be L: so $\bar{L}(s)$ is calculated in the q-system as $L(q(s))$. But eq. 4.77 implies that the functional form of $\bar{L} \circ \theta$ in the q'-system is $q' \to q \to L(q)$: which is the same. That is, this functional form is also $L(q(s)) \equiv L(q(q'(s)))$. (The occurrence of $q(q')$ here corresponds to the use of the inverse coordinate transformation under the passive view; cf. eq. 4.72.)

Furthermore, we could undertake this construction in reverse. That is, we could instead start with differentiable invertible active maps θ on S, and thereby define coordinate transformations, with the property that a scalar function and its "drag-along" have the same functional expression in the two coordinate systems. (Exercise! Fill in these details; and fill in the details of the above discussion, so as to respect the $q - \dot{q}$ distinction.)

This passive-active correspondence obviously applies to any space on which invertible differentiable coordinate transformations are defined. For our construction of the corresponding active transformation made no appeal to the three special features of Lagrangian mechanics and cyclic coordinates that we used in our passive discussion, eq. 4.71 to 4.75. Namely, the three features:

(i): the $q - \dot{q}$ distinction on S;

(ii): the use of a translation in just one coordinate to give a one-parameter family of transformations, labelled by ϵ;

(iii): the idea of a cyclic coordinate and L being invariant.

So we will now re-introduce these features. We begin with the most important one: (iii), invariance.

(We will re-introduce feature (i), the $q - \dot{q}$ distinction, in the next Subsection. And it will be clear that the main role of (ii), in both passive and active views, is to provide a differential notion of invariance; cf. how eq. 4.74 is the differential form of eq. 4.75.)

We saw that on the passive view, the invariance of the Lagrangian means that it has the same functional form in both coordinate systems; eq. 4.75.

On the active view, it is natural to define: $\bar{L}$ is *invariant* under the map $\theta : U \to U$ iff $\bar{L}$ and $\bar{L} \circ \theta$ are the *same* scalar function $U \to \mathbb{R}$.

But we also saw that on the active view, now replacing θ by ϵ in the obvious way: for *any* $\bar{L}$ and its drag-along $\bar{L}_\epsilon$, the functional forms in the q-system and q'-system respectively, match. This implies that $\bar{L}$ being invariant means that $\bar{L}$ itself has the same functional form in the q-system and q'-system.

So to sum up: the correspondence between passive and active (local) transformations implies an equivalence between two definitions of what it is for a scalar such as the Lagrangian to be invariant—where this invariance is understood as above, and not just as being a scalar on the space S! Namely: that L have the same functional form in two coordinate systems, and that it be identical with its "drag-along".

4.7.3 Vector fields and symmetries—variational and dynamical

The last Subsection used, albeit briefly, the idea of a one-parameter family of transformations on the n-dimensional configuration space Q, and how it "lifts" to the $2n$-dimensional velocity phase space that I labelled S.

I now need to state these ideas more carefully; and especially, to be explicit about the "differential version" of such a family of transformations. This is the idea of a *vector field* on Q. Here my discussion borders on the relatively abstract ideas of modern geometry: ideas which this paper has eschewed, apart from the geometric interlude Paragraph 3.3.2.E. But fortunately, in proving Noether's theorem I will be able to make do with an elementary notion of a vector field. More specifically, I need to expound four topics:

(1): the idea of a vector field on Q;

(2): how such vector fields "lift" to velocity phase space;

(3): the definition of a (variational) symmetry;

(4): the contrast between (3) and the idea of symmetry of the equations of motion (aka: a dynamical symmetry). *Warning*: the material in (4) will not be needed for Section 4.7.5.

(1): *Vector fields on Q* :—-

The intuitive idea of a vector field on configuration space Q is that it is an assignment to each point of the space, of a infinitesimal displacement, pointing to a nearby point in the space. The assignment is to be continuous in the sense that close points get similar infinitesimal displacements assigned to them, the displacements tending to each other as the points get closer. For present purposes, we will furthermore require that a vector field be differentiable; (this is defined along similar lines to its being continuous).

In this way, it is intuitively clear that a differentiable vector field on an n-dimensional configuration space is represented in a coordinate system $q = (q_1, \ldots, q_n)$ by n first-order ordinary differential equations

$$\frac{dq_i}{d\epsilon} = f_i(q_1, \ldots, q_n) \quad . \tag{4.78}$$

At each point $(q_1, \ldots, q_n)$, the assigned infinitesimal displacement has a component in the q_i direction equal to $f_i(q_1, \ldots, q_n)d\epsilon$.

Thus we return to the basic ideas of solving ordinary differential equations, expounded in Paragraph 2.1.3.A, (1). In particular, a vector field has through every point a local integral curve (solution of the differential equations). In this way, a vector field generates a one-parameter family of active transformations: that is, passage along the vector field's integral curves, by a varying parameter-difference ϵ, is such a family of transformations. The vector field is called the *infinitesimal generator* of the family. It is in this sense that a vector field is the "differential version" of such a family. (Using the last Subsection's passive-active correspondence, one could define what it is for a vector field to generate a one-parameter family of coordinate transformations. But I will not need this idea: exercise to write it down!)

I turn to a more precise definition of a vector field on Q: informal, by the standards of modern geometry, but adequate for present purposes. The idea of the definition is that a vector field X on Q is to assign the same small displacements irrespective of a choice of coordinate system on Q. So first we recall that the expressions in two coordinate systems q and q' of a small displacement are related by

$$\delta q'_i = \Sigma_j \, \frac{\partial q'_i}{\partial q_j} \delta q_j + \mathrm{O}(\epsilon^2) \quad . \tag{4.79}$$

We therefore define: a *vector field* X on Q is an assignment to each coordinate system q on Q, of a set of n real-valued functions $X_i(q)$, with the different sets meshing according to the transformation law (for the coordinate transformation $q \to q'$):

$$X'_i = \Sigma_j \, \frac{\partial q'_i}{\partial q_j} X_j. \tag{4.80}$$

The functions $X_i(q)$ are called the *components* of the vector field in the coordinate system q.

(2): *Vector fields on TQ; lifting fields from Q to TQ*:—-
Now consider the velocity phase space, the $2n$-dimensional space of configurations and generalized velocities taken together. This has a natural structure induced by Q, essentially because a coordinate system q on Q, defines a corresponding coordinate system $q, \dot{q}$ on the velocity phase space. Indeed, the velocity phase space is called the *tangent bundle* of Q', written TQ—where T stands, not for 'time', but for 'tangent'. The reason for 'tangent' lies in modern geometry; (cf. Paragraph 3.3.2.E for more detail). But in short, the reason is as follows.

Consider any smooth curve in configuration space, $\phi : I \subset \mathbb{R} \to Q$, with coordinate expression in the q-system $t \in I \mapsto q(\phi(t)) \equiv q(t) = q_i(t)$. Mathematically, t is just the parameter of the curve ϕ: but the physical interpretation is of course that ϕ is a possible motion, and t is the time, so that if we differentiate the $q_i(t)$ the dot $\dot{}$ stands for time, and the n functions $\dot{q}_i(t)$ together define the generalized velocity vector, for each time t. Besides: for each t, the values $\dot{q}_i(t)$ of these n functions together form the tangent vector to the curve ϕ where it passes through the point in Q with coordinates $q(t) \equiv q(\phi(t))$. We think of this tangent vector as attached to the space Q, at the point.

It will be helpful to have a notation for this point in Q, independent of its coordinate expression (here, $q(t)$). Let us write it as $x \in Q$. Then: considering all the various possible curves ϕ that pass through x (with various different directions and speeds), we get all the various possible generalized velocity vectors. They naturally form a n-dimensional vector space, which we call the *tangent space* T_x attached to $x \in Q$.

Then the set (space) TQ is defined by saying that an element (point) of TQ is a pair, comprising a point $x \in Q$ together with a vector in T_x. So: adopting the coordinate system q on (a patch of) Q, there is a corresponding coordinate system $q, \dot{q}$ on (a corresponding patch of) TQ.

TQ is a "smooth space"—formally, a differential manifold—so that we can define vector fields on it, on analogy with (1) above. In full generality, a vector field on TQ will be an assignment to any coordinate system $\zeta^\alpha = (\zeta^1, \ldots, \zeta^{2n})$ on TQ of a set of $2n$ real-valued functions $X_\alpha(\zeta)$, with the different sets meshing according to the analogue of eq. 4.80, i.e. for a coordinate transformation $\zeta \to \zeta'$:

$$X'_\alpha = \Sigma_{\beta=1}^{2n} \frac{\partial \zeta'_\alpha}{\partial \zeta_\beta} X_\beta. \tag{4.81}$$

But we will be interested only in vector fields on TQ that "mesh" with the structure of TQ as a tangent bundle, i.e. with vector fields on TQ that are induced by vector fields on Q—in the following natural way.

This induction has two ingredient ideas.

First, any curve in configuration space Q defines a corresponding curve in TQ—intuitively, because the functions $q_i(t)$ define the functions $\dot{q}_i(t)$. More formally: given any curve in configuration space, $\phi : I \subset \mathbb{R} \to Q$, with coordinate expression in the q-system $t \in I \mapsto q(\phi(t)) \equiv q(t) = q_i(t)$, we define its *extension* to TQ to be the curve $\Phi : I \subset \mathbb{R} \to TQ$ given in the corresponding coordinates by $q_i(t), \dot{q}_i(t)$.

Second, any vector field X on Q generates displacements in any possible state of motion, represented by a curve in Q with coordinate expression $q_i = q_i(t)$. (So here t is the parameter of the state of motion, not of the integral curves of X.) Namely: for a given value of the parameter ϵ, the displaced state of motion is represented by the curve in Q

$$q_i(t) + \epsilon X_i(q_i(t)) \quad . \tag{4.82}$$

Putting these ingredients together: we first displace a curve within Q, and then extend the result to TQ. Namely, the extension to TQ of the (curve representing) the displaced state of motion is given by the $2n$ functions, in two groups each of n functions, for the $(q, \dot{q})$ coordinate system

$$q_i(t) + \epsilon X_i(q_i(t)) \quad \text{and} \quad \dot{q}_i(t) + \epsilon Y_i(q_i(t), \dot{q}_i) \quad ; \tag{4.83}$$

where Y is defined to be the vector field on TQ that is the derivative along the original state of motion of X. That is:

$$Y_i(q, \dot{q}) := \frac{dX_i}{dt} = \Sigma_j \frac{\partial X_i}{\partial q_j} \dot{q}_j. \tag{4.84}$$

In this sense, displacements by a vector field within Q can be "lifted" to TQ. The vector field X on Q lifts to TQ as $(X, \frac{dX}{dt})$; i.e. it lifts to the vector field that sends a point $(q_i, \dot{q}_i) \in TQ$ to $(q_i + \epsilon X_i, \dot{q}_i + \epsilon \frac{dX_i}{dt})$.[56]

[56] I have discussed this in terms of some system $(q, \dot{q})$ of coordinates. But the definitions of extensions and displacements are in fact coordinate-independent. Besides, one can show that the operations of displacing a curve within Q, and extending it to TQ, commute to first order in ϵ: the result is the same for either order of the operations.

(3): *Definition of 'symmetry'* :—-
To define symmetry, I begin with the integral notion and then give the differential notion. I will also simplify, as I have often done, by speaking "globally, not locally", i.e. by writing as if the relevant scalar functions, vector fields etc. are defined on all of Q or TQ: of course, they need not be.

We return to the idea at the end of Section 4.7.2: the idea of the Lagrangian L being invariant under an active transformation θ, i.e. equal to its drag-along $L \circ \theta$. (So here L is the coordinate-independent scalar function on TQ, not a functional form. But we could use Section 4.7.2 to recast what follows in terms of a passive notion of symmetry as sameness of L's functional form in different coordinate systems: exercise!)

Now we consider an entire one-parameter family of (active) transformations θ_s : $s \in I \subset \mathbb{R}$. We define the family to be a *symmetry* of L if the Lagrangian is invariant under the transformations, i.e. $L = L \circ \theta_s$. (But see (4) below for why '*variational symmetry*' is a better word for this notion.)

For the differential notion of symmetry, we use the idea of a vector field. We define a vector field X to be a *symmetry* of L if the Lagrangian is invariant, up to first-order in ϵ, under the displacements generated by X. (But again, see (4) below for why '*variational symmetry*' is a better word.) More precisely, and now allowing for a time-dependent Lagrangian (so that for each time t, L is a scalar function on TQ): we say X is a symmetry iff

$$L(q_i + \epsilon X_i, \dot{q}_i + \epsilon Y_i, t) \; = \; L(q_i, \dot{q}_i, t) + \mathrm{O}(\epsilon^2) \quad \text{with} \;\; Y_i = \Sigma_j \, \frac{\partial X_i}{\partial q_j} \dot{q}_j \; . \tag{4.85}$$

An equivalent definition is got by explicitly setting the first derivative with respect to ϵ to zero. That is: X is a symmetry iff

$$\Sigma_i \; X_i \frac{\partial L}{\partial q_i} \; + \; \Sigma_i \; Y_i \frac{\partial L}{\partial \dot{q}_i} \quad = 0 \quad \text{with} \;\; Y_i = \Sigma_j \, \frac{\partial X_i}{\partial q_j} \dot{q}_j \tag{4.86}$$

(4): *A Contrast: symmetries of the equations of motion* :—-
Warning:— As I said at the end of Section 4.7.1, the material here is not needed for Section 4.7.5's presentation of Noether's theorem. But the notion of a symmetry of equations of motion (whether Euler-Lagrange or not) is so important that I must mention it, though only to contrast it with (3)'s notion.

The general definition is roughly as follows. Given any system of differential equations, $\mathcal{E}$ say, a *(dynamical) symmetry* of the system is an (active) transformation ζ on the system $\mathcal{E}$'s space of both independent variables, x_j say, and dependent variables y_i say, such that any solution of $\mathcal{E}$, $y_i = f_i(x_j)$ say, is carried to another solution. For a precise definition, cf. Olver (2000: Def. 2.23, p. 93), and his ensuing discussion of the induced action (called 'prolongation') of the transformation ζ on the spaces of (in general, partial) derivatives of the y's with respect to the xs (called 'jet spaces').

As I said in Section 4.7.1, groups of symmetries in this sense play a central role in the modern theory of differential equations: not just in finding new solutions, once

given a solution, but also in integrating the equations. For some main theorems stating criteria (in terms of prolongations) for groups of symmetries, cf. Olver (2000: Theorem 2.27, p. 100, Theorem 2.36, p. 110, Theorem 2.71, p. 161).

But for present purposes, it is enough to state the rough idea of a one-parameter group of dynamical symmetries (without details about prolongations!) for the Lagrangian equations of motion in the usual familiar form, eq. 3.39 or 4.8. In this simple case, there is just one independent variable $x := t$, so that we are considering ordinary, not partial, differential equations; and there are n dependent variables $y_i := q_i(t)$.

Furthermore, following the discussion in Section 3, these equations mean that the constraints are holonomic, scleronomous and ideal, and the system is monogenic with a velocity-independent and time-independent work-function. As I have often emphasised (especially Sections 4.4 and 4.6), this means that the system obeys the conservation of energy, and time is a cyclic coordinate. And *this* means that we can define dynamical symmetries ζ in terms of the familiar active transformations on the configuration space, $\theta : Q \to Q$, discussed since Section 4.7.2. In effect, we define a ζ by just adjoining to any such $\theta : Q \to Q$ the identity map on the time variable $i : t \in \mathbb{R} \mapsto t$. (More formally: $\zeta : (q, t) \in Q \times \mathbb{R} \mapsto (\theta(q), t) \in Q \times \mathbb{R}$.)

Then we define in the usual way what it is for a one-parameter family of such maps $\zeta_s : s \in I \subset \mathbb{R}$ to be a one-parameter group of dynamical symmetries (for Lagrange's equations eq. 3.39): namely, if any solution curve $q(t)$ (or equivalently: its extension $q(t), \dot{q}(t)$ to TQ) of the Lagrange equations is carried by each ζ_s to another solution curve, with the ζ_s for different s composing in the obvious way.

And finally: we also define (in a manner corresponding to (3)'s discussion) a differential, as against integral, notion of dynamical symmetry. Namely, we say a vector field X is a dynamical symmetry if it is the infinitesimal generator of such a one-parameter family ζ_s.

For us, the important point is that this notion of a dynamical symmetry is *different* from (3)'s notion of a variational symmetry. Hence it is best to use different words.[57]

Many discussions of Noether's theorem only mention (3)'s notion, variational symmetry. Arnold himself just says that any variational symmetry is a dynamical symmetry, adding in a footnote that several textbooks mistakenly assert the converse implication—but he does not give a counterexample (1989: 88). But in fact there is a subtlety also about the first implication, from variational symmetry to dynamical symmetry. Fortunately, the same simple example will serve both as a counterexample to the converse implication, and to show the subtlety about the first implication. Besides, it is an example we have seen before: viz., the two-dimensional harmonic oscillator with a single frequency (Paragraph 3.3.2.D).[58]

[57] Since the Lagrangian L is especially associated with variational principles, while the dynamics is given by equations of motion, calling (3)'s notion 'variational symmetry', and the new notion 'dynamical symmetry' is a good and widespread usage. But beware: it is not universal. Many treatments call (3)'s notion 'dynamical symmetry'—understandably enough in so far as, for the systems being considered, L determines the dynamics.

[58] All the material from here to the end of this Subsection is drawn from Brown and Holland (2004a);

Recall from eq. 3.47 and eq. 3.48 that the usual and unfamiliar Lagrangians are respectively (with cartesian coordinates written as qs):

$$L_1 = \frac{1}{2}\left[\dot{q}_1^2 + \dot{q}_2^2 \; - \; \omega^2(q_1^2 + q_2^2)\right] \;\; ; \;\; L_2 = \dot{q}_1\dot{q}_2 - \omega^2 q_1 q_2 \;\; . \tag{4.87}$$

These inequivalent Lagrangians give the same Lagrange equations eq. 3.39—or using Hamilton's Principle, the same Euler-Lagrange equations: viz.

$$\ddot{q}_i + \omega^2 q_i = 0 \;\; , i = 1, 2. \tag{4.88}$$

The rotations in the plane are of course a variational symmetry of L_1, and a dynamical symmetry of eq. 4.88. But they are *not* a variational symmetry of L_2. So a dynamical symmetry need not be a variational one. Besides, eq.s 4.87 and 4.88 contain another example to the same effect. Namely, the "squeeze" transformations

$$q_1' := e^{\eta} q_1 \; , \; q_2' := e^{-\eta} q_2 \tag{4.89}$$

are a dynamical symmetry of eq. 4.88, but not a variational symmetry of L_1. So again: a dynamical symmetry need not be a variational one.[59]

I turn to the first implication: that every variational symmetry is a dynamical symmetry. This is true: general and abstract proofs (applying also to continuous systems i.e. field theories) can be found in Olver (2000: theorem 4.14, p. 255; theorem 4.34, p. 278; theorem 5.53, p. 332).

But beware of a condition of the theorem. It requires that all the variables q (for continuous systems: all the fields ϕ) be subject to Hamilton's Principle. The need for this condition is shown by rotations in the plane, which are a variational symmetry of the harmonic oscillator's familiar Lagrangian L_1. But it is easy to show that such a rotation is a dynamical symmetry of one of Euler-Lagrange equations, say the equation for the variable q_1

$$\ddot{q}_1 + \omega^2 q_1 = 0 \;\; , \tag{4.90}$$

only if the corresponding Euler-Lagrange equation holds for q_2.

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cf. also their (2004). Many thanks to Harvey Brown for explaining these matters. The present use of the harmonic oscillator example also occurs in Morandi et al (1990: 203-204).

[59] In the light of this, you might ask about a more restricted implication: viz. must every dynamical symmetry of a set of equations of motion be a variational symmetry of *some or other Lagrangian* that yields the given equations as Euler-Lagrange equations? Again, the answer is No for the simple reason that there are many (sets of) equations of motion that are not Euler-Lagrange equations of *any* Lagrangian, and yet have dynamical symmetries in the sense discussed, i.e. transformations that move solution curves to solution curves.

Wigner (1954) gives an example. The general question of under what conditions is a set of ordinary differential equations the Euler-Lagrange equations of some Hamilton's Principle is called the *inverse problem* of Lagrangian mechanics. It is a large subject, with a long history; cf. e.g. Santilli (1979), Lopuszanski (1999).

4.7.4 The conjugate momentum of a vector field

Now we define *the momentum conjugate to a vector field* X to be the scalar function on TQ:

$$p_X : TQ \to \mathbb{R} \;\; ; \;\; p_X = \Sigma_i \; X_i \frac{\partial L}{\partial \dot{q}_i} \tag{4.91}$$

(For a time-dependent Lagrangian, p_X would be a scalar function on $TQ \times \mathbb{R}$, with $\mathbb{R}$ representing time.)

We shall see in examples below that this definition generalizes in an appropriate way our previous definition of the momentum conjugate to a coordinate q, in Section 4.4.

For the moment, I just note that it is an *improvement* in the sense that, as I said in Section 4.5.1 (footnote 49), the momentum conjugate to a coordinate q depends on the choice made for the other coordinates. But the momentum p_X conjugate to a symmetry X is independent of the coordinates chosen. I will (i) explain why, in intuitive terms (expanding Arnold 1989: 89); and then (ii) give a proof. *Warning*:— (i) and (ii) are not needed for the statement and proof of Noether's theorem in Section 4.7.5.

(i): This independence is suggested by the definition of p_X. For think of how in elementary calculus the rate of change (directional derivative) of a function $f : \mathbb{R}^3 \to \mathbb{R}$ along a line $\phi : t \mapsto \phi(t) \in \mathbb{R}^3$ is a coordinate-independent notion; and it is given by contracting the gradient of f with the line's tangent vector, like eq. 4.91. That is: Taking cartesian coordinates, so that the tangent vector of the line is $(\frac{dx_1}{dt}, \frac{dx_2}{dt}, \frac{dx_3}{dt})$, the directional derivative of f is given by

$$\frac{df}{dt} = \Sigma_i \; \frac{dx_i}{dt} \frac{\partial f}{\partial x_i} \; . \tag{4.92}$$

Then on analogy with the case in elementary calculus, we have: p_X as defined by eq. 4.91 is given by contracting the "gradient of L with respect to the $\dot{q}$s" with the vector X.

Arnold (1989: 89) makes much the same point in terms of:

(i) the one-parameter family of transformations generated by X, call it θ_s with $s = 0$ corresponding to the identity at a point $q \in Q$;

(ii) the idea introduced in (2) of Section 4.7.3, that the various possible velocity vectors $\dot{q}$ form the tangent space T_q at $q \in Q$.

Thus Arnold says p_X is the rate of change of $L(q, \dot{q})$ when the vector $\dot{q}$ 'varies inside the tangent space T_q with velocity $\frac{d}{ds} \mid_{s=0} \theta_s(q)$'.

(ii) To prove independence, we first apply the chain-rule to $L = L(q'(q), \dot{q}'(q, \dot{q}))$ and "cancellation of the dots" (i.e. eq. 3.30, but now between arbitrary coordinate systems), to get:

$$\frac{\partial L}{\partial \dot{q}_i} = \Sigma_j \; \frac{\partial L}{\partial \dot{q}'_j} \frac{\partial \dot{q}'_j}{\partial \dot{q}_i} = \Sigma_j \; \frac{\partial L}{\partial \dot{q}'_j} \frac{\partial q'_j}{\partial q_i} \tag{4.93}$$

Then using eq. 4.80, and relabelling i and j, we deduce:

$$p'_X = \Sigma_i \; X'_i \frac{\partial L}{\partial \dot{q}'_i} = \Sigma_{ij} \; X_j \frac{\partial q'_i}{\partial q_j} \frac{\partial L}{\partial \dot{q}'_i} = \Sigma_{ij} \; X_i \frac{\partial q'_j}{\partial q_i} \frac{\partial L}{\partial \dot{q}'_j} = \Sigma_i \; X_i \frac{\partial L}{\partial \dot{q}_i} \equiv p_X \; . \tag{4.94}$$

Finally, I remark incidentally that in a geometric formulation of Lagrangian mechanics, the coordinate-independence of p_X becomes, unsurprisingly, a triviality. Namely: p_X is obviously the contraction of X with the canonical one-form

$$\theta_L := \frac{\partial L}{\partial \dot{q}^i} dq^i \quad . \tag{4.95}$$

that we defined in eq. 3.51 of Paragraph 3.3.2.E (3).

4.7.5 Noether's theorem; and examples

Given just the definition of conjugate momentum, eq. 4.91, the proof of Noether's theorem is immediate. (The interpretation and properties of this momentum, discussed in the last Subsection, are not needed.) The theorem says:

> If X is a (variational) symmetry of a system with Lagrangian $L(q, v, t)$, then X's conjugate momentum is a constant of the motion.

Proof: We just calculate the derivative of the momentum eq. 4.91 along the solution curves in TQ, and apply the definitions of Y_i eq. 4.84, and of symmetry eq. 4.86:

$$\begin{aligned} \frac{dp}{dt} &= \Sigma_i \; \frac{dX_i}{dt} \frac{\partial L}{\partial \dot{q}_i} \; + \; \Sigma_i \; X_i \frac{d}{dt} \left(\frac{\partial L}{\partial \dot{q}_i} \right) \\ &= \; \Sigma_i \; Y_i \frac{\partial L}{\partial \dot{q}_i} \; + \; \Sigma_i \; X_i \frac{\partial L}{\partial q_i} \quad = 0 \quad . \end{aligned} \tag{4.96}$$

All of which, though neat, is a bit abstract! So here are two examples, both of which return us to examples we have already seen.

The first example is a shift in a cyclic coordinate q_n: i.e. the case with which our discussion of Noether's theorem began, in Section 4.7.2. So suppose q_n is cyclic, and define a vector field X by

$$X_1 = 0, \ldots, X_{n-1} = 0, \; X_n = 1 \; . \tag{4.97}$$

So the displacements generated by X are translations by an amount ϵ in the q_n-direction. Then $Y_i := \frac{dX_i}{dt}$ vanishes, and the definition of (variational) symmetry eq. 4.86 reduces to

$$\frac{\partial L}{\partial q_n} = 0 \tag{4.98}$$

So since q_n is assumed to be cyclic, X is a symmetry. And the momentum conjugate to X, which Noether's theorem tells us is a constant of the motion, is the familiar one

$$p_X := \Sigma_i \; X_i \frac{\partial L}{\partial \dot{q}_i} = \frac{\partial L}{\partial \dot{q}_n} \quad . \tag{4.99}$$

Furthermore, as mentioned in this Section's Preamble, Paragraph 4.7.0, this example is *universal*, in that it follows from the Basic Theorem about solutions of ordinary differential equations (the 'rectification theorem': Paragraph 2.3.1.A (i)) that every symmetry X arises from a cyclic coordinate in some system of coordinates.

But for good measure, let us nevertheless look at our previous example, the angular momentum of a free particle (Section 4.5.1), in the *cartesian* coordinate system, i.e. a coordinate system without cyclic coordinates. So let $q_1 := x, q_2 := y, q_3 := z$. Then a small rotation about the x-axis

$$\delta x = 0, \;\; \delta y = -\epsilon z, \;\; \delta z = \epsilon y \tag{4.100}$$

corresponds to a vector field X with components

$$X_1 = 0, \;\; X_2 = -q_3, \;\; X_3 = q_2 \tag{4.101}$$

so that the Y_i are

$$Y_1 = 0, \;\; Y_2 = -\dot{q}_3, \;\; X_3 = \dot{q}_2 \quad . \tag{4.102}$$

For the Lagrangian

$$L = \frac{1}{2} m(\dot{q}_1^2 + \dot{q}_2^2 + \dot{q}_3^2) \tag{4.103}$$

X is a (variational) symmetry since the definition of symmetry eq. 4.86 now reduces to

$$\Sigma_i \; X_i \frac{\partial L}{\partial q_i} \;+\; \Sigma_i \; Y_i \frac{\partial L}{\partial \dot{q}_i} = -\dot{q}_3 \frac{\partial L}{\partial \dot{q}_2} + \dot{q}_2 \frac{\partial L}{\partial \dot{q}_3} = 0 \; . \tag{4.104}$$

So Noether's theorem them tells us that X's conjugate momentum

$$p_X := \Sigma_i \; X_i \frac{\partial L}{\partial \dot{q}_i} = X_2 \frac{\partial L}{\partial \dot{q}_2} + X_3 \frac{\partial L}{\partial \dot{q}_3} = -mz\dot{y} + my\dot{z} \tag{4.105}$$

which is indeed the x-component of angular momentum.

5 Envoi

Two of this paper's themes have been: praise of eighteenth century mechanics; and criticism of conceiving physical theorizing as "modelling". So let me end by quoting two passages concordant with those themes. My praise is summed up by Lanczos in the Preface to his wise book:

> [T]he author ... again and again ... experienced the extraordinary elation of mind which accompanies a preoccupation with the basic principles and methods of analytical mechanics. (Lanczos 1986: vii)

And as an antidote to elation! ... My criticism is illustrated by Truesdell, famous not only as a distinguished mathematician and historian of mechanics, but also as an acerbic polemicist against all manner of shallow and fashionable ideas:

> Nowadays people who for their equations and other statements about nature claim exact and eternal verity are usually dismissed as cranks or lunatics. Nevetheless, we lose something in this surrender to lawless uncertainty: Now we must tolerate the youth who blurts out the first, untutored, and uncritical thoughts that come into his head, calls them "my model" of something, and supports them by five or ten pounds of paper he calls "my results", gotten by applying his model to some numerical instances which he has elaborated by use of the largest machine he could get hold of, and if you say to him, "Your model violates NEWTON's laws", he replies "Oh, I don't care about that, I tackle the physics directly, by computer."
> (Truesdell 1987: 74; quoted by Papastavridis 2002: 817)

Acknowledgements:— I am grateful to various audiences and friends for comments on talks; and to Harvey Brown, Tim Palmer, David Wallace and Graeme Segal for conversations. I am especially grateful for comments on previous versions, to: Harvey Brown, Anjan Chakravartty, Robert Bishop, Larry Gould, Oliver Johns, Susan Sterrett, Michael Stöltzner and Paul Teller. I am also grateful to Oliver Johns for letting me read Chapters of his forthcoming (2005).

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