Newton on Islandworld: Ontic-Driven Explanations of Scientific Method

Penultimate Version, forthcoming in Perspectives on Science

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Acknowledgements

This paper has had a long development. Adrian presented the germ of the idea at the Philosophy Seminar Series at the University of Sydney in May 2014, where Maureen O'Malley, John Matthewson and Mark Colyvan provided crucial input. In November 2014 Kirsten and Adrian presented the complete idea to the HPS working group at Penn State, thanks in particular to Michael Weisberg. We also received useful feedback on drafts from Daniel Nolan and two anonymous referees from Perspectives in Science. Both of us have also had various useful conversations over the years with too many people to fairly remember. Part of the research for this publication was made possible through the support of a Fellowship at the Institute for Research in the Humanities, University of Bucharest, and a grant from Templeton World Charity Foundation. The opinions expressed in this publication are those of the author(s) and do not necessarily reflect the views of Templeton World Charity Foundation.

Abstract

Philosophers and scientists often cite ontic factors when explaining the methods and success of scientific inquiry. That is, the adoption of a method or approach (and its subsequent success or otherwise) is explained in reference to the kind of system in which the scientist is interested: these are explanations of why scientists do what they do, that appeal to properties of their target systems. We present a framework for understanding such 'ontic-driven' explanations, and illustrate it using a toy-case, the biogeography of 'Islandworld'. We then put our view to historical work, comparing Isaac Newton's *Opticks* to his *Principia*. Newton's optical work is largely experiment-driven, while the *Principia* is primarily mathematical, so usually, each work is taken to exemplify a different kind of science. However, Newton himself often presented them in terms of a largely consistent method. We use our framework to articulate an original and plausible position: that the differences between the *Opticks* and the *Principia* are due to the kinds of systems targeted. That is, we provide an ontic-driven explanation of methodological differences. We suspect that ontic factors should have a more prominent role in historical explanations of scientific method and development.

1 Introduction

As philosophers, we are often in the business of explaining scientific method. That is, we ask why such-and-such investigation was carried out as it was, what worked and what didn't, and why. Here, we introduce a framework for understanding *ontic-driven* responses to these kinds of questions. Explanations of method are 'ontic-driven' when they appeal to properties of the systems under investigation. We shall use our framework to develop a fruitful and (at least *prima facie*) plausible hypothesis: that several methodological differences between Isaac Newton's two major contributions to natural philosophy, his work on mechanics and optics, are due to ontic differences. We'll start by providing some examples of ontic-driven explanations and characterizing them abstractly.

Ontic-driven explanations are common in scientists' own methodological reflection—whenever they chalk up a method's effectiveness or otherwise to the nature of the system targeted. Consider Diamond and Robinson:

> …the cruel reality is that manipulative experiments are impossible in many fields widely admitted to be sciences. That impossibility holds for any science concerned with the past… one cannot manipulate the past (Diamond and Robinson 2010, 1).

Diamond and Robinson point to a paradigmatic scientific capacity—controlled manipulative experiments—and argue that for some targets (historical ones) that capacity is unavailable, and unavailable because of the nature of the target system: past events are not suitable experimental targets.[1] They go on:

> One therefore has to devise other methods of "doing science": that is, of observing, describing and explaining the real world, and of setting the individual explanations within a larger framework (Diamond and Robinson 2010, 1).

That is, because some scientists target systems which are not amenable to experimental approaches, different methods are required. Another example of ontic-driven discussion is Jacob Weiner's concerns about ecology:

> Frustration with the apparent lack of progress [in ecology] has led numerous ecologists to question the way scientific research in ecology is done… Many ecologists attribute the lack of progress in ecological science to the nature of the 'beasts,' not to methodological issues… but many researchers think that the way ecology is done could be part of the problem (Weiner 1995, 153).

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[1] Although, for a different view, see (Jeffares 2008).

What explains ecology's (apparent) lack of progress? Perhaps ecologists are doing the best they can—what they should—given the systems they investigate. Or perhaps they are making some kind of methodological mistake. The former answer is ontic-driven; the latter is not.

Philosophers, too, highlight ontic features when discussing scientific method and epistemology. For instance, according to both *difference-making* and *mechanistic* accounts, scientific explanation proceeds by identifying special properties of systems. Both views take the properties in question to be variables playing privileged causal roles. The former, 'difference makers', are those which influence the target variables under certain counterfactual perturbations (Waters 2007, Woodward 2003, 2010); the latter are concerned with mechanistic components and their organisation, required in combination for the production of some phenomenon or behaviour (Levy 2013, Craver 2007, Machamer and Darden 2000). On both views, features of systems—ontic features—play a central role in how explanation proceeds. Another contemporary concern is modelling and idealisation. Scientific representation often involves omission and distortion. Philosophers have argued that these distortions are necessitated by the complexity of the systems scientists investigate (Weisberg 2007, Winsberg 2010, Cartwright 1999, Mitchell 2002). On such views, scientists construct indirect, idealised representations, *models,* which allow them to navigate the complex systems they seek to understand. So, philosophers often identify a scientific strategy—an explanatory structure, pattern of reasoning, or theoretical construct—and then justify that strategy in light of properties of the relevant target systems.

The second half of this paper will present an ontic-driven explanation of differences in Isaac Newton's methodology. And indeed, philosophers and historians studying Newton occasionally highlight ontic features in accounting for the differential success of his methodological strategies. For example, Steffen Ducheyne argues that Newton's optical phenomena are simply not amenable to the kind of rigorous causal explanations available to celestial phenomena (Ducheyne 2012, 219-222). And, George Smith argues that, despite Newton's best efforts in investigating fluid mechanics, "the empirical world did not cooperate" (Smith 2001, 249). Our account of ontic explanation, and the framework we use to understand it, allows such ontic-driven explanations to be more explicit, better situated, and better supported.

An *ontic-driven* explanation, then, accounts for an investigative strategy by pointing to properties of the system targeted. We can get somewhat clearer on this. Explanation is fruitfully understood in contrastive terms (Lipton 1990, van Fraassen 1980). That is, when we desire an explanation, we seek features in virtue of which the *explanandum* diverges from relevant (actual or counterfactual) contrasts. For instance, Weiner is after the factors which distinguish ecology from more (apparently) progressive sciences (in his case, molecular biology). Is the relevant difference between ecology and molecular biology a methodological deficiency on ecology's part, or does it come down to ontic features—the 'nature of the beasts'? In other words, are ecological systems simply different from molecular systems in a way which necessitates differing epistemic strategies? If so, we have an ontic-driven explanation.

And so, an explanation of scientific method is ontic-driven when the explanatory target and the relevant contrasts diverge in virtue of properties of the systems investigated. Of course, ontic features are not all that matters. Investigative approaches are also explained by appeal to technological and theoretical circumstances, to scientific aims, to sociological and institutional factors, or even to the psychologies of individual researchers.[2] Historians and sociologists of science, especially, often emphasise intellectual and cultural context. It is sometimes argued that we cannot understand how science works divorced from the traditions, beliefs, and influences of the time. Pitt, for instance, argues that analyses of 'observation' can only be made in reference to specific time periods (Pitt 2001). Jim Lennox claims that the best approach to understanding contemporary (and presumably past) scientific theoretical confusion is by studying "…the historical origins and development of those problems" (Lennox 2001, 657). As he says, "the foundations of a particular scientific field are shaped by its history, and to a much greater degree than many of the practitioners of a science realize" (Lennox 2001, 657). Burian recommends grouping scientific practice either in terms of the evolution of a particular scientific problem, or in terms of contexts: "…such studies need to take account of the multiple settings within which scientific work takes place—theoretical, technical, instrumental, institutional, political, financial, national..." (Burian 2001, 387). On such views, why scientists do what they do is necessarily tied to the rich details of a particular time and place.

Ontic-driven explanations differ from, but potentially complement, those preferred by some sociologists of science—particularly those associated with the Edinburgh Strong Programme (Barnes and Bloor 1982, Shapin and Schaffer 2011) and empirical relativism (Collins 1975). Here, 'rational' or 'internal' explanations favoured by philosophers of science are deemphasized in favour of explanations which appeal to the role of social

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[2] For example, Hacking's work (Hacking 1983) emphasises the role of technology and background theory in driving scientific method; explanatory pluralists such as Sterelny (e.g. Sterelny 1996) and Jackson & Pettit (Jackson and Pettit 1992) appeal to the explanatory program (the aims) of scientists; Kuhnian (e.g. Kuhn 1996) approaches to scientific change tend to be institutional, or at least pedagogical; Nersessian (Nersessian 1999) appeals to psychologies ('mental models') in explaining conceptual change in science.

factors: the political, the personal, and the highly contingent. Generally speaking, in explaining scientific change, and why scientists take the approaches that they do, we need not choose between an abstract analysis of the logic of science and the social and political context involved in the construction of knowledge.[3] Science is complex, and explaining its features involves a multitude of factors, from the abstract to the social. And, we suggest, the ontic. How scientific disciplines develop depends both on how scientists reason, their social context, and the kinds of systems they are trying to understand. Often, good explanations will involve a diverse buffet, not a homogenous, single-course meal.

In short, our framework isn't in conflict with these other explanatory resources. It captures the aspects of scientific methodology which are determined ontically, but fuller explanations will often draw on non-ontic factors. Having said this, we do suspect that important ontic details are sometimes obscured by over-emphasising social/historical factors. Newton's differing approaches to optics and mechanics may, in large part, be explained by the nature of optical and mechanical systems, and this has potentially been obscured.

Note that we focus on just one aspect of scientific investigation: uncovering regularities, generalisations and laws. That is, recurrent features of the world which ground scientific explanation and prediction. As we shall see, Newton explained planetary motions by appeal to his theory of universal gravitation, and explained optical phenomena by appeal to the composition of white light. We are interested in how such regularities are established. This doesn't require taking a stance on the status of such

[3] Helen Longino (Longino 2002, chapter 2) has argued convincingly that such positions rely upon a problematic dichotomy between 'internal' and 'external' explanations of science. Against this dichotomy, she argues that scientific rationality plays out in and through the social environment that scientists find themselves in.

regularities (whether they be '*ceteris paribus*' or 'law-like', and so forth), we are only interested in how scientists come to know about them. Nor do we take a stance on the nature of confirmation: suffice to say, the kinds of tests we discuss will do epistemic work on any plausible view.

We begin with a toy case, the biogeography of 'Islandworld'. The 'laws' of Islandworld bear a superficial resemblance to actual biogeographical theory, but we use it as a tool for concretizing the ideas in our framework, rather than as a serious analysis of ecology or biogeography. We then articulate our framework via discussion of macroevolutionary theory. With this in place, we apply it to Newton's work in the *Opticks* and the *Principia*. Our examples, then, are varied: from ecology, to paleontology, to mechanics and optics. This is a feature, not a bug: we argue that reflections on scientific method frequently employ a general kind of explanation—ontic-driven explanation—and that our framework grants traction on the nature of such explanation. Given the generality of our purpose, wide-ranging examples are appropriate.

2 Islandworld

Islandworld is a globe, its geography consisting of a single continent (the mainland), and many islands of varying sizes and positions relative to both the equator and the mainland. Islandworld is similar to Earth in that the biogeography of each island—here understood in terms of species number—is related to that island's spatial properties. Islandworld species-richness is determined by three 'laws':[4]

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[4] Islandworld differs from Earth by the sheer simplicity of its biogeography. The 'laws' of Islandworld have sophisticated cousins on Earth. Earthling species richness on islands (and similar locales) is taken to be broadly determined by two factors: the distance effect (how far the island is from sources of colonists affects how likely it is that new species will turn up on the island (L3)); and the size effect (the

L1: Larger islands are more species-rich than smaller islands;

L2: Islands closer to the equator are more species-rich than islands further from the equator; and

L3: Islands closer to the mainland are more species-rich than islands further from the mainland.

For our purposes, the precise mathematical relationship between island-property and species-richness is irrelevant: they could be linear or logarithmic, deterministic or probabilistic. Islandworld, then, lacks both the complexity of the real world, and the subtlety of actual biogeographical theory (see footnote 8). The point of doing this is two-fold. First, the simple nature of Islandworld allows us to articulate our framework: the model is a pedagogical tool. Second, Islandworld is similar enough to real ecological theory to provide a partial defence of our framework's scope. Potentially, it could be developed to understand the nature of method in ecology. But that is not our purpose. What matters here is the advice we have for the biogeographers of Islandworld: how might they go about establishing biogeographical theory, and what would determine the success of that method?

The most obvious way of establishing a law is by holding the effects of other influences fixed. That is, we should seek islands which differ in only one relevant property. In establishing, say, L1, ensure you only examine islands which are equidistant from the equator and the mainland. If it turns out that species richness tracks island size

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species-area curve connects species richness to island size, typically logarithmically (L1)). In addition to this, many other factors are taken to influence species richness on Earth's islands. These include the climate: hotter, wetter climates tend to encourage species richness (hence L2). The founding document of modern island biogeography is taken to be (MacArthur and Wilson 1967).

on equidistant islands, then L1 is (to some extent, at least) established. Call this method *isolation*.[5] According to isolation, to establish a regularity, we should seek tests which differ only in terms governed by that regularity.

However, it may be that the scientists of Islandworld are unable to test via isolation: there may not be enough (or any!) equidistant islands, and the Islandworlders may lack the technology and theory to fabricate them. If so, isolated tests are impossible as each law's influence cannot be examined in isolation from the others. Call the influence of laws on one another *interference*. In such cases, there are no objects with the relevant sets of properties required for isolated testing.

Isolation does not exhaust the means by which we can establish laws. The Islandworlders can instead test their theories in *combination*. Do L1, L2 and L3 in tandem determine species richness? If so, the laws are confirmed. If sufficient islands of differing sizes and distances from the equator and mainland are available, the laws can be established.

And so, we have two methods for establishing Islandworld theories of biogeography. By the first, we isolate the factor we are interested in by holding fixed other relevant factors across test cases. By the second, we use the laws in combination. What determines the appropriate method? The amount of interference. We ask, is there, or can we fabricate, a situation where one law alone determines the property of interest?

Interference is ontic, it is a property of the system under investigation, and so appealing to it provides an ontic-driven explanation of Islandworlder methodology. Whether they establish their biogeographical laws via isolation or via combination

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[5] Isolation is most clearly carried out in experimental contexts. Here, our capacity to actively manipulate systems allows us to bring about test cases of causal regularities. See (Okasha 2011).

depends on Islandworld's geographical layout. Assuming that the technological and theoretical capacities are held fixed, the relative position and sizes of islands makes the difference.

Let's complicate things. Islandworld is occasionally shaken by underwater earthquakes. These spawn tsunamis which in turn sink parts of islands. Let's imagine the earthquakes and their effects are stochastic and independent of L1, L2 and L3. The result is that some islands decrease in size without corresponding decreases in biodiversity. That is, earthquakes undermine L1. Let's call this kind of affect *noise*.[6] The law still holds, let us imagine, but only *ceteris paribus*—when not effected by earthquakes. How do earthquakes effect Islandworld biogeographical theory?

If earthquakes are frequent on Islandworld, it doesn't follow that scientists will be unable to establish laws by isolation or combination. However, there will be two consequences. Firstly, epistemic confidence could be undermined by non-systematic exceptions. Secondly, and more interestingly, the applicability of Islandworld biogeographical theory will be restricted, as the laws together are not the final word on species richness. Even in the right circumstances for isolated tests—an abundance of islands of the right size and distance from equator and mainland—because the target system is noisy, the applicability of subsequent theories is limited. Islandworld laws will not predict species number reliably outside of their tests. And so Islandworlders in noisy circumstances will be less successful than those in quiet circumstances. Again, this

[6] Our use of the term 'noise' is somewhat idiosyncratic, as it picks out exogenous factors affecting a system's behaviour, as opposed to the more usual epistemic sense of noise, which contrasts it with 'signal'.

difference in success requires an ontic-driven explanation. The nature of Islandworld, the frequency of earthquakes, makes a difference to the biogeographers' success.[7]

And so, Islandworlder method and success depends first, on the amount of interference between the biogeographical laws and second, on the amount of noise from earthquakes. High interference undermines the Islandworlders' capacity to conduct isolated tests, while high noise lowers both the efficiency of tests and the applicability of resulting theory. In the next section, we will characterise these dimensions in more detail and present our framework.

3 Interference & Noise

The amount of interference and noise in a system partly governs the appropriate scientific method for its investigation, and this generates ontic-driven explanations. We will illustrate this claim in section 4. Before getting there, however, we shall explain the central distinction of the framework, and present it schematically.

3.1 The distinction

The difference between interference and noise depends on a distinction between inter- and intra-system factors. That is, with respect to some system, some factors are endogenous, i.e. 'part' of the system, while others are exogenous, i.e. 'outside' of the system. In Islandworld, this was stipulated: island size and position were a proper part of Islandworld biogeography, while earthquakes were not. But on what basis might we

[7] Our discussion of islandworld might strike ecologists as odd, given that changes to Islandworld—the sinking of islands, for instance—would ultimately affect the species composition of other islands as well. The dynamical nature of ecological systems, and ecologists' focus on those dynamics, are not well-captured by our discussion of Islandworld. But again, the point of the example is not directly to understand the work of ecologists or the nature of ecological systems, rather, it is to provide an illustration of our framework. Interference and noise, we think, are useful conceptual tools for understanding ecology's ontic difficulties in practice, but we leave that task for later work.

draw such a distinction? We shan't provide an *analysis* here; indeed, we are sceptical that this can be done. One reason for scepticism is that whether or not a variable falls within some domain—and in particular why it does—is an empirical question. Scientific investigation, rather than *a priori* analysis, determines what counts as interference or noise for some domain. Moreover, as we shall see, interference and noise are generated by a variety of sources, undermining generic characterisation. Nonetheless, we can point to related distinctions and provide illustrations. Moreover, that scientists themselves often rely on such distinctions partly vindicates our using them.

Marc Lange's account of laws relies on a distinction akin to ours. For him, a law is part of a *stable* set. That is, it is logically consistent with the possibilities set by the other laws in that relevant domain. If L1 is a law on Lange's view, then it will remain true across the range of possibilities described by L2 and L3. But how do we determine each domain's laws? Here, (following Mill) Lange distinguishes between *greater causes* and "…petty, local, idiosyncratic influences that must be ascertained on a case-by-case basis" (Lange 2005, 398). By Lange's account, a necessary condition of being a law *vis-à-vis* some domain is capturing a greater cause relevant to that domain.[8] In Islandworld, for instance, island size is a greater cause of species richness. In contrast, although earthquakes can affect species richness, they are not greater causes. What makes something a greater cause is in part whether we can say something systematic about that factor's effect in the relevant domain. On Islandworld, island size has a specific and systematic effect on species richness. Moreover, the effect can be captured using relatively simple dynamics. Earthquakes are not like this: their effect on species richness depends on local, idiosyncratic factors—where the earthquake struck, its magnitude, and

[8] This is merely an aspect of Lange's account.

so on. Debate in macroevolution provides an illustration of greater and lesser causes, which we'll connect to interference and noise.

Macro-evolutionary theory targets large-scale patterns in morphological evolution, explaining the 'shape of life' in terms of radiation, speciation, mass-extinction, and so on. Reductionists hold that evolution via natural selection, which explains the evolution of particular traits on the 'micro-scale', is sufficient to explain patterns of the 'macro-scale': macro-evolution is micro-evolution scaled up. Anti-reductionists, in contrast, believe that micro-evolutionary forces alone are insufficient to explain important macro-evolutionary features. They often point to the contingent 'historicity' (Beatty 2006, Beatty and Carrera 2012) of macro-evolutionary processes such as speciation (Gould and Eldredge 1977) and extinction (Gould 1989) to argue that, for instance, natural selection is not the only 'force' bearing on macro-evolution.[9]

One anti-reductionist argument highlights mass extinctions (see McConwell and Currie 2017, for example). At least sometimes, mass extinctions are caused by 'external' disruptions, most dramatically by extra-terrestrial impacts, but also changes wrought by continental drift and atmospheric shift. It is thought that these factors are not within the purview of evolution by natural selection—organisms are not 'designed' by natural selection to survive mass extinctions—and so a different theory is required to understand macro-evolution. In essence, the debate is about the role or otherwise of both 'drift', non-selective, often stochastic, forces which influence genotypic and phenotypic evolution, and macro-evolutionary analogues such as species-sorting (Turner 2011). We are not interested here in entering the debate about the nature of drift and natural

[9] See (Sterelny 2003) for a general comparison of the two views through an analysis of Gould's work, and (Dennett 1995) for a classic reductionist rejoinder.

selection,[10] rather, we think it nicely illustrates the intra/inter-domain distinction we are after.

One might understand anti-reductionism about macro-evolution as the view that, on a macro-evolutionary scale, natural selection is not the only major cause, or fails to even count as a major cause. Let's imagine that mass extinctions are sometimes caused by extra-terrestrial impacts, and that mass extinctions make a real difference to macro-evolutionary patterns. Moreover, let's say that such events have the features of major causes: they are relatively systematic and can be understood with relatively simple theory. If so, natural selection is likely not the only major cause of macro-evolution. If the role of chance and outside disruptions are such that natural selection does not have a systematic effect on macro-evolutionary patterns, then it is not even a major cause. Note that (usually at least) anti-reductionists in no way deny that natural selection is important on the micro-scale—they are not anti-Darwinian. Rather, *vis-à-vis* macro-evolution, natural selection is either insufficient (only one of the major causes), or irrelevant (a lesser cause).

Let's connect the distinction between interference and noise to greater and lesser causes. Recall the two anti-reductionist views about macro-evolution. The weaker view includes other greater causes: to accommodate macro-evolutionary patterns, extra theoretical machinery covering drift, species sorting, and so forth, must be added to our arsenal. Here, natural selection is a greater cause, but interferes with drift, and is itself interfered with. The stronger anti-reductionist denies that natural selection is a greater cause; if anything, it is noise from a macro-evolutionary perspective. By contrast, the reductionist considers drift to be a lesser cause, merely generating noise. And so,

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[10] Philosophers of biology differ on whether 'drift' constitutes an actual process or force, or whether it should be considered statistical 'error' (see (Plutynski 2007) for a general overview).

interference is the relationship between greater causes, while noise is from lesser causes. The distinction between greater and lesser causes, and the kinds of distinctions those in the debate about macroevolution draw, turn crucially on the idea that *vis-à-vis* some system, some causal factors are endogenous and others are exogenous.

To reiterate, this is no analysis of the difference between interference and noise, but rather an elucidation. The following three points are important. Firstly, the difference between interference and noise depends upon the properties of systems. That is, the difference is ontic. Thus, disagreement between reductionists and anti-reductionists about macro-evolution centres on the nature of macro-evolutionary systems and processes. Secondly, what counts as interference or noise is system-relative. If anti-reductionists about macroevolution are right, then drift interferes with natural selection on the macro-scale. Yet they may happily concur that it is a greater cause on the micro-scale. Thirdly, and relatedly, whether something is a source of noise or interference *vis-à-vis* some system is decided by scientific investigation: we doubt there is an *a priori* analysis available.

This third point deserves further discussion, in service of which we'll look at the sources of noise and interference. These are multiply realisable and scale-dependent. Any worldly factor which makes isolation testing troublesome could be seen as a source of interference. On Islandworld, for instance, island position is a source of interference: if no equidistant islands are available, Islandworlders lack the appropriate objects to establish L1 via isolation. John Matthewson (Matthewson 2011) and Alkistis Elliott Graves' (2016) distinction between 'complexity' and 'heterogeneity' is a useful way of thinking about sources of noise and interference. We can understand a system's 'complexity' in terms of the number of components, and their interaction. An electric sewing machine is a fairly complex system. Its operation relies on sensitive interaction between needles, motor, foot pedal, and so forth. Isolated tests on any particular part of the machine become difficult. A system's complexity can generate interference: modularity begins to break down, intervening on and tracing causal relationships becomes more difficult, and so forth. Although sewing machines are complex, they are often not *heterogeneous*. 'Heterogeneity' relates to the different *types* of components within and between systems. Sewing machines, particularly those of the same make and model, tend to operate in the same way, using the same components. This homogeneity means that knowledge of one sewing machine will be easily generalised across many. If Tolstoy is right, unhappy families are heterogeneous, as each generates unhappiness in a different way. Due to this, understanding the dynamics of unhappiness in one family will not tell us much about unhappiness in another family. Heterogeneity, then, generates noise. As there are many ways of being complex and being heterogeneous, there are many sources of interference and noise.

The enormous range of sources of interference and noise prevents a precise analysis of these concepts. However, as we see in macro-evolutionary debate, scientists do distinguish between endogenous and exogenous factors pertaining to some system. That the distinction plays a central role in empirical disputes justifies our taking it seriously despite imprecision.

Two more points are required. First, although the framework is primarily ontic, it is necessarily indexed to theoretical and technological capacities (hence our term 'ontic-driven'). To see this, consider various ways in which interference could occur. Some interference might be unsolvable in principle—two processes might be necessarily entangled, for instance. Let's add a fourth law to Islandworld: any island that is both equidistant from the equator and the mainland is barren. In this scenario, it is nomologically impossible to test L1 in isolation. This seems extraordinarily unlikely for biogeography, but analogues might occur in, say, particle physics. More realistically, most cases of high interference are not so due to nomological or logical restrictions, but due to (sometimes extremely challenging) technological or practical ones. For instance, if Islandworlders are not lucky enough to find equidistant islands, the technology required to construct them, and the time required for the requisite species to turn up or evolve, would most likely make isolation tests prohibitive. As we'll see in Newton's case, in such circumstances scientists often do conduct isolated studies, but these employ proxies to supplement combination testing.

Finally, you might worry that our distinction is held hostage to the decisions scientists make about whether or not something counts as a system,[11] or that it requires a strong view about the relationship between social, pragmatic and epistemic context on one hand, and ontic factors on the other hand. Such worries are misplaced. All that is required for our account to gain purchase is for ontic factors to play *some* role in determining which systems scientists investigate, how they conceive of them, and whether such investigations succeed. The distinction between interference and noise can be accommodated by strong views which place stringent restrictions on what the valid systems are, but can fit just as happily with more relaxed—even 'promiscuous' (Dupré 1993)—views which allow scientists bountiful wiggle-room *vis-à-vis* the systems they target and how they divide them. Only views claiming that ontic features place no restrictions on scientists' capacities to identify, delineate, and investigate systems will make trouble for the ontic status of the distinction between noise and interference. And such views are implausible.

With the basic distinction in hand, we can now turn to the framework.

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[11] We're grateful to an anonymous referee for highlighting this issue.

3.2 The Framework

Recall our aim: to understand ontic-driven explanations of scientific method. Our thought is that how scientists go about establishing regularities will depend on the nature of the system they target; specifically, on the amount of noise and interference within that system. We can envision noise and interference using a two-dimensional space (see figure 1, below). Where systems fall within that space determines the kind of access scientists have, and thus the appropriate methodology (that is, whether isolation or combination tests are available, and their effectiveness). Further, recall that we understand ontic-driven explanation in contrastive terms. Systems don't have *tout court* 'amounts' of interference or noise, rather, the measure is relative. For instance, when we come to compare Newton's *Principia* to his *Opticks*, we shall argue that optical systems have less interference than astronomical ones, but that astronomical systems are less noisy. The relativity of the measure allows our account to apply across a vast range of systems despite the differences between them. With the understanding that our claims are system-relative, let's identify some locations and indicate some possible examples.

Let's begin with systems of low interference and low noise: *tame* systems. These can be tested via isolation, and behaviour beyond tests is to a large extent determined by the relevant greater causes. If Islandworld biogeography is tame, then scientists are able to establish their theories both via isolation and in combination. Moreover, in virtue of the lack of noise from earthquakes, their theories will be projectable. That is, they will be a reliable guide to species richness throughout Islandworld. Possible candidates for tame systems might be physical or chemical systems. For instance, not only can chemical interactions be isolated and studied in the lab, but those interactions often occur similarly in 'the wild' (although see (Cartwright 1983, 1999) for scepticism about physics, and (Havstad 2017) for chemistry).

As we increase a system's interference, we reach circumstances where isolated tests are difficult or impossible, but behaviour is still regular. These are '*quiet* systems'.[12] Although isolatable tests are unavailable, combinatory tests are—and the established theories will be happily projectable. Islandworlders in this situation would not be able to hold fixed L2 and L3 in order to test L1, but they would find that, by combining the three laws, reliable and accurate predictions about species number are achievable. Plausible examples of such cases might be fluid mechanics and astrophysics. Although isolating planetary systems is infeasible, relatively trustworthy predictions can be made by applying physical laws in combination.

Holding interference low, and increasing noise, we find *noisy* systems. These are amenable to isolated tests, if we control for noise. However, the adequacy of subsequent theory will be limited. If Islandworld biogeography is noisy, then although equidistant islands are available to test L1 (or the Islandworlders have the technology and patience required to fabricate tests), the common occurrence of earthquakes renders the laws a poor guide to actual species richness. Some plausible systems of this nature are biological and behavioural systems. Although, for instance, animal behaviour can be isolated and examined in the lab, wild and laboratory circumstances diverge, making it difficult to export results.

Finally, turning both interference and noise to full, we find *wild* systems. These are both unamenable to isolated tests *and* are riddled with exceptions. Scientists facing such systems might find themselves questioning whether their targets are really 'systems' at all. Some tentative examples might be evolutionary, ecological and economical systems.

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[12] These are what Nancy Cartwright has called 'natural nomological machines' (Cartwright 1999).

Although such systems are sometimes taken to be amenable to experimental study or something analogous,[13] typically, such systems are considered to be extremely difficult to investigate in isolation, and the results of such studies only apply across limited cases.

This framework, we argue, can be used to generate and understand ontic-driven explanations of scientific methodology. That is, when asking why some scientific investigation proceeds as it does, we should attend to the target system's interference and noise relative to the explanatory contrasts. We do not pretend that this is always the best explanation. Rather, when an ontic-driven explanation is called for, this framework has the goods.

Our aim for the remainder is to harness the framework in a real-world case.

4 An ontic-driven explanation of Newton's Methodology

The methodological differences between Isaac Newton's two great works of Natural Philosophy, the *Opticks* and the *Principia,* are often thought to reflect developments in Newton's methodology, varying aims of investigation, differences in tradition, or differences in technology or theory. But we shall argue that a case can be made for taking the primary difference to be the subjects of the investigations. Newton's two great works target different kinds of systems, which require different methodological approaches. And so we argue that this aspect of Newton's methodology is amenable to an ontic-driven explanation. This discussion of Newton serves two purposes. On the one hand, his work illustrates our framework and demonstrates its potential. And on the other hand, we use our framework to generate a novel, *prima facie* plausible and likely

[13] For instance, sometimes convergent evolution is taken to be a 'natural experiment' which tests evolutionary theories (Currie 2013), and ecological theories are sometimes studied using model organisms in so-called 'bottle-experiments' (Odenbaugh 2006).

fruitful hypothesis about why Newton claims to have a consistent methodological approach, but employs different methods in different situations

Our treatment is somewhat simplified: given our purpose, a discussion which did justice to the rich, sophisticated state of contemporary Newton scholarship would be prohibitive. Having said this, we take our claims about Newton seriously; and consider the picture we paint broadly accurate. Newton is a startling and influential example of scientific success and as such getting the details right is important, but it would be a shame if the demand for detail was such that Newton's example couldn't inform more general projects such as the one at hand.

Here's an odd feature of Newton's career. He is taken to have spawned two important, but different, sciences: an experimental science exemplified in the *Opticks*, and the mathematical science exemplified in the *Principia*. And yet, his methodological reflections remained remarkably consistent throughout his life.

Consider this representative quote from Cohen and Smith:

> There is, perhaps, no greater tribute to the genius of Isaac Newton than that he could thus engender two related but rather different traditions of doing science (Cohen and Smith 2002, 31).

On this view Newton is the father of *two* scientific traditions. First, the austere, formal mathematism of his celestial mechanics. And second, the complex and sophisticated experimentalism of his optics. When considering the motions of bodies and the forces that produce them, Newton was a mathematician; when considering the properties of light and colours, Newton was an experimentalist. Although he draws the distinction very differently to Cohen and Smith, Kuhn also argued that Newton's two major contributions represent two profoundly different traditions. For him, the *Principia* was a 'classical' science, unified with other mathematical inquiries such as astronomy and harmonics. In contrast, the O*pticks* was a 'Baconian' science, to be lumped with studies of magnetism, electricity and heat.[14] The large divergence between Kuhn and Cohen & Smith notwithstanding, the *Opticks* and the *Principia* are often taken to exemplify very different methodologies.

However, Newton's own writing about the methodology of the *Opticks* and the *Principia* suggests a different story. Consider the following, a passage from Newton's 'anonymous' report from the Royal Society's 1715 examination of the calculus dispute with Leibniz:

> The Philosophy which Mr. *Newton* in his *Principles* and *Optiques* has pursued is Experimental; and it is not the Business of Experimental Philosophy to teach the Causes of things any further than they can be proved by Experiments (Newton 1715, 222).

Here, Newton unified his two works under the label 'experimental philosophy', suggesting that, in stark (and undeniably snarky) contrast to Leibniz, both works are grounded in observation and experiment, rather than speculative hypotheses. And so apparently Newton took himself to have a distinct methodology, exhibited in both of his great works.

Commentators have often interpreted this and other such statements as purely rhetorical: in his dispute with Leibniz, Newton made a political decision to align himself with the Royal Society and, by extension, to identify his methodology with experimental philosophy. However, while we lack the space to make a satisfactory case for it here, we think this professed unity is not a mere rhetorical flourish on Newton's part, but a serious philosophical claim.[15] For instance, we think Newton's experimentalism can be seen in the reasoning in book 3 of the *Principia*, which is based on the 'phenomena' of

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[14] More recently, Kuhn's influential account has been called into question by, for example, (Anstey 2014, Domski 2013).

[15] For an influential account of this position, see (Shapiro 2004). And for replies see (Walsh 2012a, b).

observed planetary motions, and in the scholia in books 1 and 2 of the *Principia*, which are littered with suggestions for future experimental programs. Moreover, in both works, Newton makes his stance on hypotheses very clear. In the *Principia*, he (in)famously declares *Hypotheses non fingo* ("I do not feign hypotheses" (Newton 1999, 943)). And in the *Opticks*, he begins, "My Design in this Book is not to explain the Properties of Light by Hypotheses, but to propose and prove them by Reason and Experiments" (Newton 1952, 1).[16] But the unity runs deeper than this. Niccolò Guicciardini suggests that the two works are unified by their use of the method of analysis. He writes:

> In the *Principia* analysis is a deduction of forces from motions; in the *Opticks* it is a deduction from compositions to ingredients… In both cases one has a deduction of causes from effects (Guicciardini 2011, 316-317).

He continues:

> Further, the procedure of deduction from experiments (in the *Opticks*) and from phenomena or observations (in the *Principia*) has the tentative, heuristic, and complex structure of the analytical heuristic method of the mathematicians. Newton could draw a comparison between the experimental method adopted in natural philosophy and the method of analysis of the mathematicians because he placed experimentation within a deductive mathematical procedure… (Guicciardini 2011, 317)

In short, the unity of Newton's methodology, that endures over time and across his scientific work, is grounded in the use of mathematical reasoning to derive theoretical

[16] The distinction between theories and hypotheses is central to Newton's methodology. For Newton, theories are on epistemically surer footing than hypotheses because they are grounded on phenomena, whereas the latter are grounded in speculations (Walsh 2017). For a discussion of the distinction between theories and hypotheses in early modern philosophy more generally, see (Ducheyne 2013).

propositions from experiments and phenomena—deducing causes from effects. Below, we'll sketch this as his 'mathematico-experimental' method.

The following passage from Newton's first published paper (1672) is an early statement of this methodology:

> A naturalist would scearce expect to see ye science of [colours] become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not a hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all Phænomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt (Newton 1959-1977, Vol. 1, 96-97).

Here, Newton claimed that his science of optics is *experimental*, in that it is grounded in observation and experiment, and *mathematical*, in that theoretical propositions are deduced from experiment, in the style of mathematicians, without recourse to speculative hypotheses. These two aspects constitute the methodology which remained consistent throughout his life.[17]

Now consider another representative passage. This one's from book 1 of the *Principia* (1687):

> Mathematics requires an investigation of those quantities of forces and their proportions that follow from any conditions that may be supposed. Then, coming down to physics, these

[17] Commentators generally view the quoted passage as extreme, and there is some disagreement *vis-à-vis* the extent to which it is representative of Newton's epistemological commitments. Shapiro and Guicciardini, for example, have argued that Newton quickly retracted his extreme claims of certainty and instead expressed a more moderate epistemic attitude towards his theory of light (Guicciardini 2011, 20, Shapiro 1989, 225, 1993, 14) (against this position, see Walsh Forthcoming). Here, however, we introduce this passage to demonstrate that, well before his difficulties with Leibniz, Newton was presenting his optical work as mathematico-experimental. We can make this point without taking a position on the debate about Newton's notion of certainty.

proportions must be compared with the phenomena, so that it may be found out which conditions [or laws] of forces apply to each kind of attracting bodies. And then, finally, it will be possible to argue more securely concerning the physical species, physical causes, and physical proportions of these forces (Newton 1999, 588-589—brackets indicate translators' comment).

Here, Newton claimed that mathematical reasoning can carry us from causes to effects—or from forces to the motions they produce. But to do 'physics', that is, to harness the power of mathematics to explain actual physical systems, we must reason in the other direction—from motions to their forces.[18] This passage is best understood contextually: it leads us into sections 12 and 13 of book 1, where the focus is on centripetal forces directed toward individual particles of matter. Immediately following this passage is a sequence of mathematically derived conditional propositions relating centripetal forces directed toward whole bodies and centripetal forces directed toward their individual parts composing those forces. Just as the above quotation says, these mathematical propositions investigate "those quantities of forces and their proportions that follow from [a subset of] any conditions that may be supposed". Newton finally arrives at Proposition 92 (Newton 1999, 618), which describes an experimental program to (1) establish whether the forces toward whole bodies are composed of forces toward the parts of those bodies and (2) to measure how the latter forces vary with distance. In short, what we find is an experimental program akin to the one investigating optical

[18] In the above passage, Newton might have been appealing to the well-known distinction between 'pure mathematics' (i.e. arithmetic and geometry) and 'mixed mathematics' (i.e. astronomy, optics, harmonics, etc.). Traditionally, pure mathematics was said to deal with mathematics as it applied to *intelligible* things, that is, those things that can be apprehended by the intellect or imagination. In contrast, mixed mathematics was concerned with the application of mathematics to natural phenomena. So it was said to deal with mathematics as it applied to *sensible* things, that is, those things that can be apprehended by the senses. See (Dunlop 2012, 77) for a discussion of this distinction.

phenomena. However, Newton evidently realised how weak gravitational forces toward everyday bodies are, and saw no way to carry out this program at the time.

And so, Newton's methodology has two aspects: the *mathematical* and the *experimental*. Let's make these more explicit.

Mathematical: According to Newton, just as mathematical inferences could be performed without epistemic loss, so could philosophical inferences (i.e. inferences about physics). That is, it is possible (with the help of mathematical tools such as geometry and calculus) to deduce new physical propositions from principles and laws. And these new propositions will have the same epistemic status as the original principles and laws.

Experimental: According to Newton, theories must be inferred from experimentally-established principles: i.e. principles deduced from phenomena, acquired via observation and experiment. Furthermore, Newton seems to have been rather inflationary about what such empirical enquiry—even a single '*experimentum crucis*'—could achieve. In his more extreme statements, claiming they could yield truth "without any suspicion of doubt".[19]

These features in combination make sense of Newton's claims about a 'mathematical science'. From principles or laws, one may deduce propositions or theorems without epistemic loss. The challenge, then, is to establish those principles. Newton thought

[19] This notion can be characterised as 'compelled assent': the evidence compelled Newton undeniably to his conclusion, and he expected others to draw the same conclusion in the same context (See Walsh Forthcoming). For example, in his correspondence with Lucas (August 1676), Newton wrote: "[Let Lucas examine the experiments given.] For if any of those be demonstrative, they will need no assistants nor leave room for further disputing about what they demonstrate. The main thing he goes about to examin is *the different refrangibility of light*. And this I demonstrated by *the Experimentum Crucis*. Now if this demonstration be good, there needs no further examination of the thing; if not good the fault of it is to be shewn, for the only way to examin a demonstrated proposition is to examin the demonstration" (Newton 1959-1977, Vol. 2, 79-80).

that this was best achieved experimentally. And so Newton's methodology was 'mathematico-experimental'. To drive the point home, consider this passage from book 2 part II of the *Opticks*, written later in Newton's career:

> Now as all these things follow from the properties of Light by a mathematical way of reasoning, so the truth of them may be manifested by Experiments (Newton 1952, 240).

In arguing that Newton's methodology is much more unified and consistent than usually recognised, we are not suggesting that it was static and unvarying. Indeed, over the course of his working life, Newton was deeply engaged with methodological questions, working to improve, develop and nuance his investigative and theorising skills. Moreover, different contexts and research programs presented Newton with different methodological challenges to overcome. So, Newton's methodology was dynamic and progressive. We think it is plausible, however, that the mathematico-experimental procedure sketched above was an enduring methodological commitment. Development and variation took place within the parameters of this fundamental feature of Newton's methodology. The analysis we offer in the following sections highlights one aspect of this variation: the ontic features of different target systems.

In sum, Newton apparently spawned two very different sciences, the experimental optics and the mathematical mechanics, and yet his methodological reflection betrays a unity, his mathematico-experimental method. Newton's methodological reflections describe two works unified by one methodological approach; but the two works seem to exemplify different ways of investigating natural systems. And so, here, we think an ontic-driven explanation will be illuminating. Our first task, then, is to ask what was different about Newton's targets which necessitated these differences. Our second (and brief) task is to inquire into their differing success: the mechanical project is taken to have outperformed the optical one. We will present ontic-driven answers to both questions. In a nutshell, on our view, Newton's optical system exhibited less interference than his celestial system, and thus, where the former was amenable to (experimental) isolation tests, the latter was not. This goes some way towards explaining the difference in how Newton approached these systems. However, we argue, optical systems are much noisier than celestial ones, and so the regularities which Newton established were much more widely applicable in the case of mechanics than optics—which explains the differing success of those projects.

Before we examine the two sciences in more depth, we should say something about how our ontic-driven explanation fits with other explanations of Newton's methodology. As implied in the introduction, we are ecumenicists about explanation. That is, we do not think there is one single privileged way of explaining an historical episode. Explanations are contrastive: they serve to explain why one thing happened rather than another. With respect to a given historical episode, there might be multiple sets of contrasts that interest us, leading to multiple legitimate explanations of a single historical episode. Newton's methodology is a case in point. Depending on our primary concerns, explanations of Newton's methodology could appeal to multiple factors: for instance, Newton's position in the Royal Society (Feingold 2001), his education at Cambridge (Dunlop 2012), his primary interest in alchemy (Newman 2016), and so on, could each form the basis of an explanation of his method. Although these explanations are not equivalent, they are also not necessarily in competition. Rather, each highlights a different aspect of Newton's methodology. And we think this is valuable. Different kinds of contrastive explanations allow us to bring different features of Newton's work into focus, and explore different hypotheses. One feature of Newton's methodology which, we think, has been largely overlooked is its relationship to the system being investigated. And we think our ontic-driven explanation sheds important light on this feature.

4.1 *How to Establish Principles*

Let's compare Newton's 'experimental' approach to optics with his 'mathematical' approach to mechanics. As we shall see, both are examples of his mathematico-experimental method: propositions are deduced from principles, which are established by experiment. Why, then, does the method look so different in the two cases? Drawing on our account of ontic-driven explanations, we'll tentatively suggest that the two works differ in the method by which principles are established. The *Opticks* contains paradigm instances of isolation-testing. We attribute this approach to the low interference in such systems. The *Principia*, by contrast, relies on combination-testing. This is because, as we'll see, the target system has high interference. We argue, then, that an ontic-driven explanation of this methodological difference is available. Establishing such a thesis requires more work, but for our purpose of illustrating our framework and establishing its utility, availability and plausibility are sufficient.

4.1.1 *Isolation-testing in the* Opticks

Newton's *Opticks* follows the quasi-geometrical style of the *Principia*: it opens with a set of definitions and axioms, before the theory is developed in a series of propositions (labelled as either theorems or problems). Newton establishes each proposition experimentally, calling the procedure 'proof by experiments'. He begins by stating the proposition, then discussing a sequence of experiments which are supposed to establish the proposition beyond doubt. A discussion follows explaining how the experiments support the proposition.[20]

[20] These discussions also contain detailed instructions and illustrations, which suggests that the reader was supposed to work through the experiments, perhaps even attempting to replicate them, in order to grasp the truth behind the demonstrations.

Here we shall focus on the first proposition of Book 1 part I.[21] Proposition 1 theorem 1 states that "*Lights which differ in Colour, differ also in Degrees of Refrangibility*[22]" (Newton 1952, 20). This proposition is an important first step in establishing one of Newton's key claims about light: that there is a one-to-one correspondence between refrangibility and spectral colour.[23] Establishing this correspondence enabled Newton to make use of geometrical properties of light (rectilinear propagation, regular refraction and reflection, and so on) to make inferences about the colour-producing properties of light.[24] The inference might be reconstructed as follows:[25]

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[21] Of course, by focusing on a single proposition, we do not do justice to the sophisticated design of Newton's experimental program, in which combinations of experiments accumulate to establish a sequence of theoretical propositions. In this paper, however, we are interested in Newton's *Opticks* insofar as it offers paradigm cases of isolation-testing. The experiments supporting proposition 1 provide a particularly clear and accessible example.

[22] 'Refrangibility' refers to the disposition of light to refract when passing from one medium into another, or a "predisposition, which every particular Ray hath to suffer a particular degree of Refraction" (Newton 1959-1977, Vol. 1, 96).

[23] The proposition which gets him the rest of the way to the one-to-one correspondence is in Book 1 part II: proposition 2 theorem 2, "*All homogeneal Light has its proper Colour answering to its Degree of Refrangibility, and that Colour cannot be changed by Reflexions and Refractions*" (Newton 1952, 122).

[24] It is significant that, in both the *Opticks* and the early paper on which it is based (Newton 1672), Newton establishes geometrical properties of white light before considering colour. (Certainly this fact has been lost on many of Newton's critics, both then and now.) There are at least two good reasons for this. Firstly, a technological limitation: it was one thing to isolate and manipulate small rays of light, but it was much more difficult to isolate and manipulate a single colour, which required extraordinary precision and very good eyesight. Secondly, as we've already seen, Newton's aim was to develop a rigorous mathematical science of optics: angles, sizes, shapes and positions were things that he could talk about mathematically; hue, saturation and brightness were not.

[25] This inference can be seen more explicitly in a letter to Huygens (23 June 1673). Here, Newton sets out his account in five definitions and ten propositions. Proposition 1 corresponds to my P2, propositions 2 and 3 together correspond to my P1, and propositions 4 and 5 together correspond to my C (Newton 1959-1977, Vol. 1, 293). (Propositions 6-10 offer an account of coloured bodies.) Newton claims that proposition 4 is derived from definitions 1 and 3 and proposition 1, and that proposition 5 is derived from

P1. There is a one-to-one correspondence between refrangibility and spectral colour.

P2. White light is composed of rays of many refrangibilities.

C. White light is composed of rays of many spectral colours.

Newton provides two experiments in support of proposition 1. In experiment 1 Newton draws a line down the centre of a piece of black card, and paints one half red and the other half blue. Then he uses sunlight to illuminate the card, and peers at the card through a prism, which he holds close to his eyes (see figure 2 below). When he tilts the prism upwards, the card appears to move upwards, the blue half (*dg*) appearing higher than the red half (*fe*). When he tilts the prism downwards, the card appears to move downwards, the blue half (*δν*) appearing lower than the red half (φε). From this experiment, Newton concludes that the blue light refracts to a greater degree than the red light, and hence the blue light is more refrangible than the red light:

> Wherefore in both Cases the Light which comes from the blue half of the Paper through the Prism to the Eye, does in like Circumstances suffer a greater Refraction than the Light which comes from the red half, and by consequences is more refrangible (Newton 1952, 21).

In experiment 2, Newton takes the same piece of card and winds "a slender Thred of very black Silk" (Newton 1952, 23) around it, so that several horizontal black lines pass across the colours. He stands the card upright against a wall, so that the colours stand vertically, side-by-side, and illuminates it with a candle. He places a glass lens at a distance of six feet from the card, and uses it to project the light coming from the illuminated card onto a piece of white paper which is at the same distance from the lens

definitions 1 and 3 propositions 2 and 3. It is a remarkable feature of Newton's theorising strategy that he frequently provided two lines of support for his propositions: (1) demonstration via deduction from established propositions and (2) demonstration via experiment.

on the other side (see figure 3 below). He moves the piece of white paper to and fro, taking precise note of where and when the red and blue parts of the image are most distinct (the purpose of the black thread is to indicate distinctness: the image is most distinct when the lines created by the thread are sharpest). He finds that when the red part of the image appears most distinct, the blue part is faint and blurred; and when the blue part of the image is most distinct, the red part is faint and blurred. And that, in order to obtain a distinct red image, the paper has to be held 1½ inches further away than it is to obtain a distinct blue image. He concludes:

> In like Incidences therefore of the blue and red upon the Lens, the blue was refracted more by the Lens than the red, so as to converge sooner by an Inch and a half, and therefore is more refrangible (Newton 1952, 25).

Newton takes these experiments to establish proposition 1: that rays of different colours are differently refrangible. He does this by isolating red and blue light, and then projecting the light through glass to observe its geometrical properties. In the first experiment, for instance, the coloured card serves to isolate the coloured light and the prism projects the light, causing it to bend.

The one-to-one correspondence between refrangibility and spectral colour was important for at least two reasons. Firstly, it established that refrangibility and colour are connected. Newton didn't think that one was the cause of the other—that, say, refrangibility caused the ray to be a certain colour, or vice versa. Rather, he thought both refrangibility and colour were original properties, primary properties, of light. Hence, when white light is projected through a prism, the rays separate (via refraction) to reveal their individual colours. Secondly, it established that there is no interference between the two properties. In other words, Newton could neither change the original colour of a ray of light by refraction, nor change the refrangibility of a ray of light by changing its colour (e.g. by mixing it with another colour). Refrangibility and spectral colour were, according to Newton, fixed and immutable properties of light.

Although this case is a particularly clear example of Newton's reasoning from experiment to proposition, the discussion thus far is insufficient to argue that the method of Newton's *Opticks* (or even just the method of Book 1) followed the mathematico-experimentalist approach we discussed above. If space was no concern and our primary purpose was establishing such a claim, we might be tempted to work through all the experiments that contribute to establishing P1. But here we're interested in showing that a plausible, ontic-driven, explanation of the differences between Newton's approaches to different systems is available—thus highlighting the utility of our account of ontic-driven explanations, and its suitability to historical contexts. We take these experiments to be paradigm cases of isolation-testing, analogous to Islandworlders examining equidistant islands to establish L1, and examining islands of equal size to establish L2 and L3. The successful use of isolation-testing suggests a system of low interference.

4.1.2 Combination-testing in the Principia

Newton's *Principia* deals with the motions of bodies and the forces that produce them—books 1 and 2, with the motions of bodies *qua* abstract mathematical objects; book 3, with the motions of celestial bodies as revealed via observation. In book 3, Newton puts forward his theory of universal gravitation, which states that any two bodies in the universe attract each other with a force that is directly proportional to the product of their masses and inversely proportional to the square of the distance between them. On Islandworld, we sought to establish each law separately—in establishing L1, we went hunting for islands which were equidistant from the mainland and the equator. In the case of universal gravitation, such isolation testing is impossible. Why? Because, as we shall see, bodies *interfere* with one another. And so, Newton's strategy in the *Principia* departed from that of the *Opticks*: using his three laws of motion, he employed a creative mixture of combination-testing and a kind of proxy isolation-testing to establish his laws. Let's examine this in more detail.

Newton established his theory of universal gravitation in two stages. First (in books 1 and 2), he modelled the laws. He started with a one-body system, in which a single body orbited a central point. In a one-body system, and in the absence of external forces, the body would remain either at rest or in uniform rectilinear motion—in accordance with law 1 (the law of inertia). So Newton conceived of circular motion as a combination of two things: uniform rectilinear motion and a centrally directed force that draws the body away from its rectilinear motion. These together cause the body to orbit the central point. From law 2 (the net force law), and its corollaries, Newton was able to calculate the motions produced by various forces, and vice versa. Newton found that, if the centrally-directed force diminishes with the square of the distance between the body and the central point (i.e. an *inverse-square centripetal force*), then the body would display Keplerian motion.[26]

Having demonstrated the results of laws 1 and 2 in a one-body system, Newton added a complexity. In the one-body system, the body orbited a central point; now, he made that central point a body. In a two-body system, law 3 comes into play. In this case, law 3 amounted to mutual attraction: the central body acted on the orbiting body and vice versa. The result was that both bodies were now in motion, orbiting their combined centre of gravity. Newton demonstrated that, with a small correction to the

[26] Keplerian motion can be defined by three rules now known as 'Kepler's laws': (1) the orbit of a planet is an ellipse, with the sun at one of the two foci; (2) a line segment joining a planet and the Sun sweeps out equal areas in equal times; and (3) the square of the orbital period of a planet is proportional to the cube of the semi-major axis of its orbit. See (Wilson 2000) for an account of how these propositions came to be regarded as 'laws'.

harmonic law, both bodies would display Keplerian motion, both with respect to one another and with respect to their common centre of gravity.

Newton then proceeded to add further complexity in a step-by-step fashion. So far, he had treated the bodies as mass points; now he considered them as objects with shape and dimension. He added a third body orbiting the original one (as the moon orbits the Earth which orbits the Sun[27]), and so on. He considered orbits of different shapes, forces of different strengths, and even complex situations involving the motions of multiple bodies on different planes. This modelling stage was akin to a series of isolation tests on a mathematical system. By carefully adding complexity, one step at a time, Newton was able to establish the consequences of his laws and to learn how they interact in complex systems. We submit that this can be illuminatively understood as a series of proxy isolation tests, with Newton's mathematical system acting in place of real celestial objects. (As mentioned in the introduction, the complexity of target systems is sometimes appealed to in explaining modelling practises in science—considering such work as 'proxy isolation testing' strikes us as a promising thought, which we leave for later development.)

In the second stage (book 3), Newton established the resemblance between his models and his target system: the motions of celestial bodies. He started by treating each of the five primary planets as a one-body system; noting that, generalised and idealised, the five primary planets approximate Keplerian motion with respect to the Sun. He also noted that the moons of Jupiter and the moons of Saturn, orbiting their respective planets, approximate Keplerian motion, and that the Moon approximates Keplerian motion with respect to the Earth. Now, Newton had already established that, in a one-

[27] Although, he did not draw this explicit connection until book 3.

body system, a body displays Keplerian motion if and only if it is maintained by an inverse square centripetal force. So idealised, the planets and moons also display Keplerian motion. Hence, Newton was able to infer that the planets and moons maintain their motions by an inverse square centripetal force. He concluded that this force was gravity—i.e. the force that causes an apple to fall to the ground.

Then, in a series of de-idealisations, Newton established that, as the Sun exerts a gravitational pull on each of the planets, so the planets exert a gravitational pull on the Sun. Similarly, the moons exert a gravitational pull on their planets. And finally, the planets and moons exert gravitational pull on each other. In short, he conceived of the solar system as a complex series of pair-wise interactions. Newton argued firstly, that each pair (i.e. each planet-moon, sun-planet and sun-moon system) in isolation would display Keplerian motion (both with respect to one another and with respect to their common centre of gravity). And secondly, that every deviation from Keplerian motion could, in principle, be accounted for by identifying every pair-wise interaction. To demonstrate this, he used observational data to show that Jupiter and Saturn sensibly perturb one another to precisely the degree predicted by the law.[28] And so Newton was able to explain why the planets and moons only approximate Keplerian motion: every body attracts every other body with a force that is proportional to its mass and diminishes with the square of the distance between them.

In sum, in the *Principia*, Newton sought to establish his theory of universal gravitation. The challenge he faced was to establish and measure centripetal forces acting on orbiting celestial bodies. He was unable to measure these forces directly, because it

[28] George Smith refers to this process as detection of second-order phenomena. For his discussion of the role of perturbation-detection in Newtonian mechanics see (Smith 2014). And for a detailed discussion of Newton's reasoning to universal gravitation (Book 3 proposition 7 and its corollaries) see (Harper 2011, 291-299).

was not possible to observe celestial bodies in isolated one- and two-body systems. However, by modelling the laws of motion in increasingly complex systems, Newton was able to learn how bodies behave, when acted upon by inverse-square forces from multiple sources. Only in the abstract world of Newton's mathematics could bodies be studied as isolated one- and two-body systems. In the real world, each body—each planet, moon, sun and comet—mutually attracts every other body. So, in examining his mathematical system, Newton engaged in a kind of proxy isolation testing; in establishing universal gravitation, he relied on combination testing. It was only by taking into account each pair-wise interaction that he was able to produce highly accurate explanations of celestial motion. By analogy, Islandworlders without access to isolation-testing could establish laws 1-3 by showing how they each together predicted species richness across Islandworld's islands.

4.2 Comparing the **Opticks** *and the* **Principia**

We have characterized Newton's methodology as 'mathematico-experimental'. Newton's propositions were deduced from experimentally established principles. We have examined the strategies by which Newton established his principles in the *Opticks* and the *Principia*. In the former case, he proceeded via isolation; in the latter, via combination. In light of our framework for ontic-driven explanations of scientific method, we submit that this difference in method can be explained by the nature of the systems he investigated. Newton's optical system was amenable to isolation-testing: he could gather together rays of a single colour and manipulate them as a single beam to establish geometrical properties. An analogous test in the *Principia* would involve isolating a single planet to observe its motion around an empty centre. But even if Newton could do this, the experiment would not yield results, because gravity is a relational property: there is no gravity in a one-body system, because there is nothing to attract the body. And if Newton could isolate a two-body system, he would have to deal with perturbations caused by mutual attraction. In other words, in Newton's celestial system, the pair-wise interactions of bodies are not independent of one another. In response to these ontic challenges, we saw Newton adopt two complementary epistemic strategies. Firstly, he represented the bodies abstractly—via a mathematical model—and carried out something like an isolation test on that proxy. Secondly, he tested via combination: a complex series of pair-wise interactions could produce the approximate Keplerian motion he was after to support his theory of universal gravitation.

Let's return to our question: how can we reconcile Newton's unified methodological statements with his disparate approaches to optical and celestial systems? One promising answer, we submit, is ontic-driven. Newton's celestial target exhibits much higher interference than his optical target. The source of the interference in mechanical systems is the *universality* of gravity—as each body effects the motion of every other body, physical isolation cannot be achieved. On Islandworld, the various islands do not interact with one another in ways which generate interference—and nor does the refrangibility and spectral colour of sunlight in the *Opticks*. And so Newton took different routes to establishing the principles of his celestial and optical systems: the former via combination (and proxy-isolation), the latter via isolation. With the principles established, the mathematical arm of Newton's method came into play. The central difference in method, then, is the approach to the experimental arm. Faced with two systems exhibiting different amounts of interference, Newton needed to employ different strategies for experimentally establishing the principles. Thus, an ontic-driven explanation of the difference not only fits, but also brings certain features of Newton's investigations into sharper focus: namely, the parallel roles played by isolation- and combination-testing. This ontic focus, we think, complements the focus on traditions, biography and social factors of many of Newton's commentators.

But what can we say about the varying success of the two projects? As we mentioned earlier, Newton's theory of universal gravitation was undoubtedly an enormous success—consequently, the *Principia* is usually considered to be the crowning achievement of seventeenth-century natural philosophy. And yet, his attempts to mathematize and explain optical phenomena were ultimately considered, no more so than by himself, a failure—and a failure specifically of his method.[29] Space forbids detailed discussion, but we submit that the answer, again, may be ontic-driven. Where we emphasised interference to explain the difference in method, we suspect that noise has something to tell us about success. Noise, you will recall, undermines the applicability of theory. Exogenous factors muck up our system's tidy, regular behaviour.

Newton's theory of universal gravitation applied both to terrestrial objects and celestial ones—including comets. And so it had enormous power to unify, explain and predict.[30] His optical theory, however, did not fare so well once it left the confines of the experimental system of book 1. Firstly, despite his best efforts in book 3,[31] Newton failed to construct a mathematical model of diffraction. The properties of this system

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[29] Ducheyne follows Cohen in arguing that the *Opticks* is an 'incomplete treatise', in that Newton fails to offer a completed account of diffraction phenomena (what Newton calls 'inflexion') and, instead, concludes the treatise with a series of queries (Ducheyne 2012, 181-184, Cohen 2001, 18-23). Ducheyne moreover points out that "Optical phenomena did not easily lend themselves to a *Principia*-style physico-mathematical treatment" (Ducheyne 2012, 219). He attributes this difficulty to problems of epistemic access: in mechanics Newton had a good understanding of the entities that constitute the *explananda,* but in optics, he did not. Indeed, any ideas he had regarding the nature of light were hypotheses, and therefore, best avoided by his own methodological decree. Thus, Ducheyne's explanation is ontic-driven in part. We take ourselves to be offering a more nuanced ontic-driven explanation; one which engages with particular features of the interaction between methodology and the target system at hand.

[30] In fact, on Earth, noise entered the system in the form of air resistance. In the scholium to the laws, Newton described some experiments which allowed him to deal with this (Newton 1999, 424).

[31] These are well-documented by Shapiro (Shapiro 2001).

interfered with, i.e. caused exceptions to, his putative 'laws' of optics—in particular, his assumption of the rectilinear propagation of light. The principles Newton employed in book 1 were no match for the complexities of such phenomena, and he never came close to matching the scope and rigour of his model of universal gravitation. And so, he was unable to draw from his observations any conclusions that he was willing to assert as propositions. Secondly, in contrast to the clear and forceful statement of achievements and conclusions that we find in the *Principia*, Newton concluded his *Opticks* with a list of increasingly speculative questions.[32] Thus, admitting openly that this conclusion could only pose questions, and not answers.[33]

Based on this discussion, we can contrast Newton's two systems in terms of the dimensions we presented in figure 1. Newton's celestial mechanics targets a relatively quiet system: there is high interference, but low noise. Newton's optical systems are comparably noisy: interference is low, but noise is high. This ontic contrast accounts for both the differing methodologies and the differing successes of the two projects.

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[32] Even Newton's theory of fits in Book 2 was, at best, a mixed success. While he eventually succeeded in producing a mathematical account of the colours of thin plates, it was poorly received, and the properties of fits (i.e. the action occurred in the direction of motion) were incompatible with diffraction (which needed a cause that could act transverse to that direction) (Shapiro 2001). And yet, despite these failures, Book 2 was extremely important and influential. For example, Newton's experiments on thin plates in book 2 offered tools for investigating matter at extremely small dimensions (on the order of 1/100,000 of an inch) and extremely great distances (e.g. the planets and even distant stars) (Sepper 1994, 128-129).

[33] While Newton's queries are usually taken to represent a failure of the Newtonian method, we cannot ignore the historical fact that they were extremely influential, forming the basis of the experimental science of electricity (Cohen 1956),

4.3 A wild system: book 2 of the Principia

We would be remiss if our discussion of Newton's successes (and failures) did not (at least briefly[34]) consider book 2 of the *Principia*, which studies the forces of resistance to motion in various types of fluids—and is widely considered to be a failure.[35] We'll highlight two relevant aspects: (1) the mixture of combination-testing and proxy isolation that was so successful for celestial mechanics fails in the case of fluid resistance; (2) this, we submit, is due to the system under investigation being a relatively wild system.

The aim of book 2 was to understand the forces operating on bodies in fluids. Newton understood total resistance as a function of several kinds of resistance and properties of the fluid (such density and viscosity), represented by the following equation:

$$R_{total} = a + bv + cv^2$$

Where a, b and c are coefficients that demanded empirical determination. Newton applied the same methodology here as he did in his study of universal gravitation, aiming (1) to find a mathematical solution for the problem and (2) to isolate the physical mechanisms in order to understand their separate contributions to the motion. And so the goal of Newton's work on fluid resistance was to disaggregate the coefficients and determine their individual magnitudes.

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[34] For the details of Newton's work on fluid resistance, see (Smith 2001, Smith 2005).

[35] At least partly for this reason, book 2 has been largely ignored by scholars. Indeed, Clifford Truesdell remarked that this book is, to all intents and purposes, "the part of the *Principia* that historians and philosophers, apparently, tear out of their personal copies" (Truesdell 1970). Notwithstanding its failures, book 2 has been important and influential to the development of a science of fluid resistance (this is discussed by Smith in the introduction to (Newton 1999, 188-194).

Newton carried out two experimental programs concerned with fluid resistance. The first is a program of pendulum experiments, in which a pendulum bob moved through air, water and mercury. Newton inferred resistance forces from the rate of decay of the pendulum motion in these different fluids. The second is a program of vertical fall experiments. These involved dropping spheres of different sizes and different materials in various fluids—some of these experiments were conducted inside the newly- or almost-completed St Paul's Cathedral in London. Newton inferred resistance forces from the time it took the bodies to fall a certain distance.

We have already argued that the success of the theory of universal gravitation can be attributed to two factors. First, the phenomena were such that the contributions of individual bodies could be disaggregated. Newton conceived of the inverse-square centripetal force as the primary cause of celestial motion. Deviations from Keplerian motion could then be treated as second-order phenomena: evidence of additional sources of attraction. Second, the system was relatively free from noise. In the context of celestial motion, gravitational forces swamp all other forces, and so the observational data was very regular and predictable. We now suggest that Newton's study of fluid resistance failed because the system in question was problematic with respect to both factors: it was wild, exhibiting both comparatively high noise and interference.

Firstly, Newton was unable to disaggregate the contributions of different kinds of resistance. There were both theoretical and experimental reasons for this. The problem was that the data contained too much variation to serve as the basis for investigating secondary factors, i.e. fluid friction and tenacity. This suggested that he hadn't properly isolated the inertial contribution. In his vertical fall experiments, he treats inertial resistance as a greater cause, assuming that it swamps the other resistance forces. Hence, he treats the other components as lesser causes, i.e. as noise. This led him to attempt to *control* for those features, that is, not include them in his theoretical understanding of the system in question. It is now understood, however, that these components are better thought of as interference: they affect inertia in ways that cannot be disaggregated. In other words, inertia can neither be theoretically nor physically isolated from other contributions. For example, forces arising directly from viscosity are often negligible, but even then, indirect effects of viscosity govern the forces arising from fluid inertia. (In the first instance, Newton tried to treat viscosity as noise, but it was really interference.)

Secondly, Newton was unable to anticipate the sources of noise in his system, such as surface finish, local small irregularities and even rotation (and other irregular motions of the bodies). In fact, the shape of the body was much more crucial than he realised. Newton knew that his results didn't hold for shapes other than spheres (e.g. disks). But the small protuberance at the top of a spherical pendulum bob, where it attaches to the string, had a much larger effect than he realised.[36]

It is notable that we are still unable to calculate the forces of resistance acting on spheres, not to mention any other shapes, from theory alone. Testing in wind tunnels and so on is required. Interference is so high that it is difficult even to establish what's part of the system and what isn't. The source of the interference is the complexity of fluid resistance. On Matthewson's account (discussed in section 3.1), complexity is the result of a large number of components interacting in a complicated way. (Recall that complexity does not entail heterogeneity across systems—the same components may still be involved in every case). In fluid resistance, friction, tenacity and other features such as surface finish interact in such complex ways that they forbid disaggregation. As we noted

[36] In fact, faced with this (at the time) unknown source of resistance, and in the absence of any other likely explanation, Newton suggested that the additional resistance was an artefact of the experiment: the to-and-fro motion of the pendulum created a current in the fluid, which was, in effect, another source of resistance.

earlier, sometimes interference is unsolvable in principle: two components are necessarily entangled. This is such a case.

Newton tried to use the same methodology to understand fluid resistance as he had to understand celestial mechanics. But the system wasn't amenable. We submit that it is the ontic aspects which draw apart the success of Newton's celestial mechanics, and the failure of his fluid dynamics.[37] In the face of such a wild system, it's not clear what he could have done differently.

5 Conclusion

Our discussion of Newton had two purposes. The first was to illustrate and apply the machinery we developed in sections 2 and 3. We have argued that contrasting 'interference' and 'noise' offers a framework for understanding the ontic-driven explanations of scientific method that scientists and philosophers of science often provide. The second was to articulate a novel claim about Newton's methodology. The difference between his 'experimental' and 'mathematical' work, we suspect, was *not* that one was experimental and the other, mathematical. Rather, he established his principles using different—but both ultimately experimental—methods. We think it is illuminating and fruitful to explain this in terms of ontic differences between the systems Newton examined. One was quiet; the other, noisy. Just as the epistemic strategies and successes of Islandworld biogeography can be understood in the ontic-driven terms we have presented, so (at least potentially) can Newton's actual scientific achievements.

As we discussed in the introduction, philosophers, historians and sociologists of science often emphasise socio-historical factors. And rightly so—we often cannot understand Newton (or ecological debate, or macroevolution) *in vacuo*. We must take

[37] Smith draws a similar conclusion in (Smith 2001).

into account the influences, intellectual climate and traditions of the time, as well as technological and theoretical capacity. However, in all this rich detail, an important aspect which can be missed—or at least left underemphasised—is the nature of the system under investigation. In Newton's case, we saw that an ontic-driven explanation potentially defuses the tension between his apparently unified methodological statements and his apparently disunified approach to actual science. There is a place—even in history—for ontic-driven explanation.

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Figures

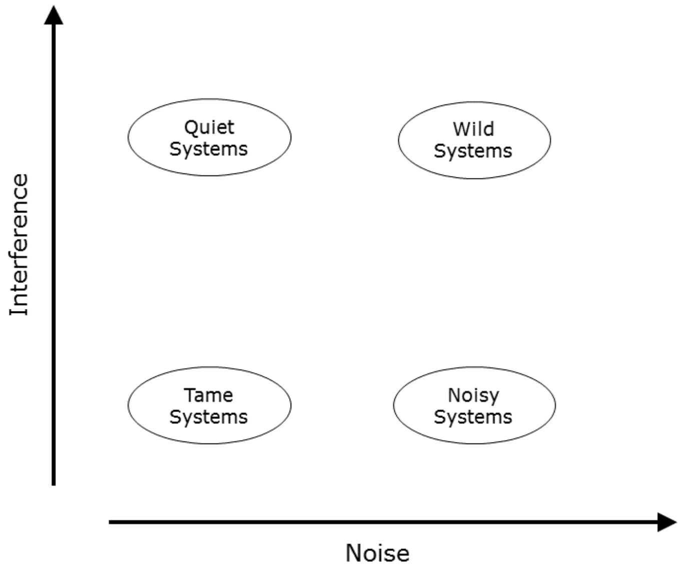

Figure 1 The framework

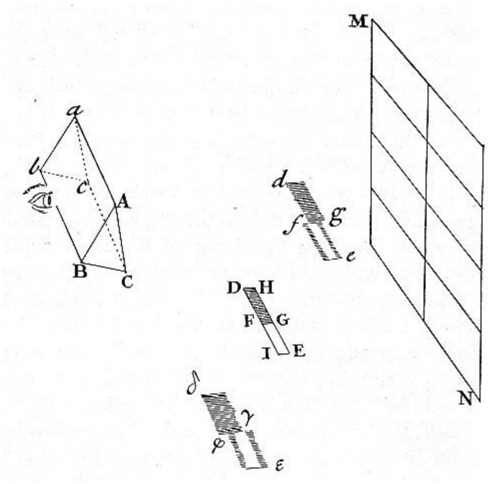

Figure 2 The *Opticks*, Book 1 part I, figure 11 (Newton 1952, 22)

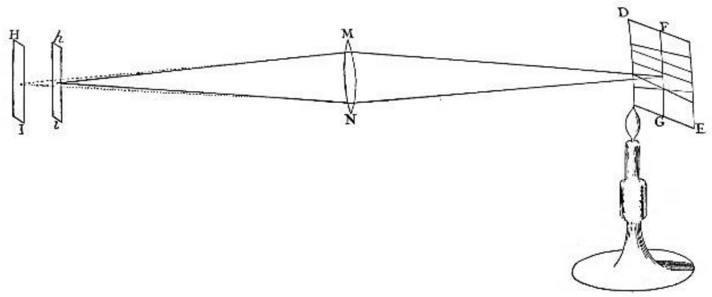

Figure 3 The *Opticks*, Book 1 part I, figure 12 (Newton 1952, 27)