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Forces and Facts: Yet Another Fragment of the Explanation for Late Eighteenth-Century Dynamism

1. Introduction: A Problem and its Context

From the very beginning of his career, Volta was an enthusiastic advocate of action-at-a-distance, and his youthful advice to Beccaria and Nollet, included the recommendation that electricity be studied in the manner of Newton.[1] We historians are however remarkably divided on the question of Newton's ontology, undecided whether he personally believed in what Volta advocated – in such things as the reality of action-at-a-distance or the intrinsic activity of matter. But we have no doubt that many of his contemporaries found these notions dubious, and remained faithful to the Cartesian project of explaining the natural world "mechanically" – with no devices beyond the shapes and motions of homogenous and inert matter, interacting through contact alone. So a contrasting "dynamical" world-view (in which intrinsic activity was re-attached to matter, that then interacted via forces)[2] could not be widely endorsed until this early opposition had been dispersed. That dispersion (we further agree) occurred well after the *Principia* – in Volta's own era (1745-1827) – and the prime purpose of this essay is to add to existing explanations of this process. Its function then is to help explain the genesis of an intellectual climate, one in which Volta's preferences could flourish.

To understand the story, we need to go back to Newton however. For whatever his personal ontology, Newton certainly took the view that celestial mechanics was

[1] J.L. HEILBRON, *Electricity in the 17th and 18th Centuries: A Study of Early Modern Physics*, (Berkeley, 1979), pp. 412-3, see also p. 414, pp. 422-5.
[2] For the purposes of our discussion, a very general characterisation of dynamism seems adequate. In reality, of course it came in various grades: Leibniz opposed action-at-a-distance, but insisted on the activity of matter; Kant seemed to deny any matter separate from force; Boscovich similarly denied matter all extension, yet retained matter as the subject of dynamic action; Aepinus made heavy use of action-at-a-distance, without requiring any ontological decision; Priestley dissolved the distinction between matter and spirit; Herschel fully accepted the reality of action-at-a-distance, without any abandonment of extended matter.

entitled to treat matter *as if* it exerted a force; and it could do this before the cause of the apparent force had been ascertained. Attraction could be deployed "nominalistically" then, as the name for the detectable effect of some unknown agency, but to do this acceptably, certain important criteria had to be met: the laws that governed the "attraction" had to be known; the evidence for the existence of the effect it described had to be good; and the effect had to be of wide scope. Gravity met all these criteria so could be well deployed in natural philosophy – even by a scientist who passionately believed that matter was utterly inert, exactly as Descartes had described it. The plausibility of the first two of these criteria is quite straightforward, and we believe that Newton adopted them for much the same reasons as we would use to defend them today. The rationale for the third (that requiring wide scope) is quite different however, and Newton's motives here are none too clear. We readily empathize with the rule, but we can imagine too many defences of it, some of which overlap with the other criteria. The result is that we do not well understand Newton's own motivation for it,[3] though it is evident that the rule had much to do with its immediate philosophic context. For it is precisely the fact that Newton's "attraction" is universal that distinguishes it from a similarly nominalistic interpretation of the disreputable *qualitates* of peripatetic matter-theory. Coining a special name for the unknown power specific to opium which makes humans fall asleep can seem pointless, even if we have no doubt that the effect of opium is real, and know (as well) the precise laws of its operation. Such practices (claimed Newton)[4] "put a stop to the Improvement of natural Philosophy". For:

> … to tell us that every species of Thing is endow'd with [a] ... specifick Quality by which its acts and produces ... effects, is to tell us nothing: But to derive two or three general Principles of Motion ... would be a very great step ..., though the Causes of those

[3] The big ambiguity here is the extent to which Newton regarded the rule 'realistically' (i.e. as a guide to truth), or 'instrumentally' (as a guide to good quality theories). His general outlook would surely suggest the first interpretation, though he presumably took the view that good theories are more likely to be true ones. Yet it is clear that one reason Newton does not explain the rule more fully is the fact that he inherits it from his philosophical environment, where his more sceptical predecessors did not impose a realist interpretation. So at the end of the *Principles* Descartes equivocates on this very issue, swinging wildly one way and then the other (R. DESCARTES, *Principles of Philosophy* [*Principia philosophiae*, 1644], V.R. and R.P. MILLER, trans., (Dordrecht, 1983), pp. 285-8); while Boyle seems to give up on the search for truth in favour of coherent (etc.) theories (e.g., R. BOYLE, *Selected Philosophical Papers of Robert Boyle*, M.A. STEWART, ed., (Manchester, 1979), p. 119).

[4] I. NEWTON, *Opticks: Or a Treatise on the Reflections, Refractions, Inflections & Colours of Light*, (New York, 1952), based on 4th ed. (1730), p. 401 [Query 31, from 1717, but based on the 1706 Query 23]. It should be noted that I do not endorse all Newton's claims about the character of peripatetic explanation: see K. HUTCHISON, "Dormitive Virtues, Scholastic Qualities and the New Philosophy", *History of Science*, 29 (1991), pp. 245-78, passim. I do however accept his suggestion that dormitive virtues were attributed great specificity: see K. HUTCHISON, "What happened to Occult Qualities in the Scientific Revolution?", *Isis* 73 (1982), pp. 233-253, on pp. 240-1.

> principles were not yet discover'd. And therefore I scruple not to propose [my attractions and repulsions], they being of very general Extent ...

It is not (in other words) idle to coin a special name – "gravitational attraction" – for the single unknown cause of a vast range of heavenly motions. One reason that such practices are not idle is surely that they are not easy to implement. For to declare that the moon's orbit results from the gravity of the Earth carries with it a far-reaching empirical claim, lacking from the seemingly analogous claim about opium. The latter reflects only the immediate facts, but the former declares that the moon responds to its environment in exactly the same quantitative way as does Mercury to its rather different environment, and so on for each moon of Jupiter and every terrestrial projectile, etc. There are (in short) harsh empirical obstacles to Newton's practice, which do not restrain Molière's doctor.

It is important to see that the empirical content here is quite independent of the true character of the "attraction" invoked. So the third criterion enabled Newton to develop a potent system of natural philosophy without providing compelling reasons for others to share his suspicions about the nature of force. We will see later that it also supported the possibility of accounting for long-range forces, via a pseudo-mechanical aether, though this is not obvious for the moment. So on two quite different grounds, the criterion functioned to provide a shelter for Newton's ontological hesitations. It allowed (and it was urged in order to allow) his audiences to adopt a moderately dynamical physics without abandoning their mechanical ontologies.

The criterion played a vital role in Enlightenment science, then. Yet Newton was only able to appeal to it, because of another well-known feature of Early Modern thought. Its audiences widely agreed that nature operated with great uniformity, shunning the multiplicity and variety that would have undermined our "Third Criterion". Copernicus's complaint about Ptolemaic astronomy being "monstrous", because of the multiplicity of its planetary mechanisms, is a familiar example of this phenomenon; and so is Descartes's analogy between doing philosophy, and the activities of a 17th-century town-planner.[5] Newton's positivist accommodation of apparent actions-at-a-distance worked, in other words, because contemporaries shared what might be called an "aesthetic prejudice" with him.

It will hardly be doubted that this prejudice was a complex matter, with origins spreading well beyond the narrow confines of natural philosophy.[6] It seems furthermore that this faith was not shared by the philosophers of late medieval Europe, as Newton's reference to the doctrine of *qualitates occultae* serves to

[5] For details, see K. HUTCHISON, "Idiosyncrasy, Achromatic Lenses and Early Romanticism", *Centaurus* 14 (1991), pp. 125-71, on pp. 130-4.

[6] I have, for example, argued for a political connection in K. HUTCHISON, "Harmony and Authority: The Political Symbolism of Copernicus' Personal Seal", in R. MAZZOLINI, ed., *Non-verbal Communication in Science prior to1900*, (Firenze, 1993), pp. 115-68, passim.

suggest – but we can forbear defending this difficult claim here. What is more important for our purposes is something else: the apparent fact that the tastes of the elite in Volta's Europe were also inclined to drift away from Early Modern uniformitarianism – and acknowledge (even celebrate) diversity. A brief remark of Goethe's vividly illustrates the contrast. "In New York", he observed:

> there are ninety different Christian sects, each acknowledging God and our Lord in its own way without interference. In scientific research – indeed in any kind of research – we need to reach for this goal.[7]

Commentators who recognize this aesthetic drift often associate it with Romanticism, though that awkward attachment is also a red herring here, serving only to illustrate the obvious: that the alleged drift is a big affair, not some minor theological quibble, etc. The question that is to occupy us here is much narrower, and develops out of the suggestion above that Newton's uniformitarianism functioned to delay commitment on the nature of action-at-a-distance. For if this were so, we should expect the decision (between mechanism and dynamism) to become more pressing for scientists who participated in our putatively "Romantic" aesthetic drift. Those then, who accepted, identified or stressed the variety and diversity of nature, should be found promoting the new dynamic outlook, interpretations of nature that emphasised (in various ways: see note 2 above) the role of force at the expense of matter. If this suggestion can be supported, then we will have found a further explanation for the new acceptability of action-at-a-distance – beyond those already identified.[8] (We will also have deepened the linkages between our aesthetic drift and Romanticism – because of the Romantics' special fondness for dynamism, as indicated in their great enthusiasm for Kant. But again that is a side issue here, serving only to locate our discussion in broader contexts.

What is to occupy us is more focussed, just the following threefold task:

- to identify the presence of our aesthetic drift within natural philosophy;
- to associate some of it with the expansion of empirical knowledge;
- to argue that this drift is one reason (among others) for the increasing acceptability of action-at-a-distance physics for late 18th-century philosophical audiences.

[7] J.W, VON GOETHE, *Scientific Studies* [Collected works], 12 vols., D. MILLER, ed. and trans., (New York, 1988), XII, p. 312.

[8] E.g.: gradual acclimatisation to action-at-a-distance in the light of the success of Newtonian celestial mechanics (used as a partial explanation in E. AITON, *The Vortex Theory of Planetary Motions*, (London, 1972), pp. 202-5); metaphysical arguments (as in primary literature cited in note 51) to the effect that mechanical contact is an illusion (as used in M. HESSE, *Forces and Fields: The Concept of Action at a Distance in the History of Physics*, (London, 1961), pp. 170-80); sceptical attitudes which tolerated incomplete understandings (ibid., pp. 163-70).

2. A Preliminary Example: Aepinus (1724-1802)

A conveniently simple example of the phenomenon that interests us is provided by a short argument in Aepinus's *Essay on the theory of electricity and magnetism* (1759), where the possibility of an aethereal explanation of electrical and magnetic attractions is set aside. For Aepinus does this by citing our precise target, the variability of nature, as exemplified in the differences between electric and magnetic attractions, familiar since the time of Gilbert. The two agencies behave so differently, says Aepinus, that a purely mechanical account of both electricity and magnetism becomes implausible, since two separate aethers would be needed to explain their forces – and this he quickly judges unacceptable, to himself and to his reader. For proponents of aethereal reduction had always imagined themselves using a single aether. Indeed, to do otherwise would be to commit the very peripatetic sin they prided themselves on avoiding.[9]

In consequence of this shunning of mechanism, Aepinus totally abandons the traditional focus of magnetic researches, the search for an explanation of the startling attractions. Instead, he develops a prime example of the new dynamical physics, one applauded by Volta. For his theory starts with the forces, deploying them as the elements of its explanation. To do this he introduces a hypothetical magnetic fluid, analogous to (but separate from) the hypothetical electric fluid he adapted from Franklin. Each of these fluids is elastic and each repels itself via apparent actions-at-a-distance, and similarly attracts (some, or all) "ordinary" matter. Aepinus offers no account of these attractions and repulsions, and, admitting this will cause some offence, defends his practice via the reduction it yields of complicated activity to a few simple causes. "I think", he says (*Essay* p. 240), "that considerable progress can be made in the analysis of the operations of nature by the scholar who reduces rather complicated phenomena to their proximate causes and primitive forces, even though the causes of those causes have not yet been detected."

This defence is an obvious echo of the Third Criterion, and like Newton, Aepinus does not tell us precisely why simplicity is a virtue. Yet there is an important difference between the two physicists here, a difference which forces Aepinus in the direction of dynamism. For Aepinus's theory compromises its simplicity slightly, in not complying with our Third Criterion to the same extent as celestial mechanics. Indeed, as Aepinus admits, the very facts which prevent mechanical explanation of electric and magnetic forces require him to introduce *two* hypothetical fluids and *two* separate systems of unexplained forces. So Aepinus acknowledges (p. 243) that his theory seems to depart "from the harmony which nature is constantly accustomed to observe it its own operations"; and fails fully to gain "many ends with few means". It thus suffers the same weakness (slight indeed, but still real enough to be cited by

[9] Fifty years later – after earlier confidence in the uniformity of nature had been more thoroughly disrupted – a reluctant Young found himself doing exactly what Aepinus opposes here: see below.

Euler as an objection to Aepinus)[10] as would weigh against a premature celestial mechanics that lazily attributed terrestrial weight to a local force separate from the celestial one generating planetary motions. But Aepinus claims (p. 243) he is "not really afraid of [this] objection":

> For since the effects of the magnet can only be happily explained if properties are attributed to the magnetic fluid which differ completely from those with which electric matter is endowed, it is not without reason, but guided by the contemplation of nature itself, that I propose here the total diversity of the two fluids.

The explanatory defects in the theory, then, are redeemed by something else: fidelity to nature. Aepinus is compelled to place rhetorical stress upon his theory's truth, the concordance between its hypotheses and the real structure of nature – precisely because he is accommodating the complexities of nature, and not just its uniformities. Yet the earlier argument against aethereal mechanism had also developed out of a recognition of variety in the universe. So for two quite different reasons, the assimilation of diversity has led us in the direction of a realist dynamism. The forces, we conclude, are more than just names for hidden mechanism. Perhaps (Aepinus's analysis tempts us to suspect) they exist in nature itself?

3. Friction: Euler (1707-1783), Coulomb (1736-1806) and Prony (1755-1839)

Though the Aepinus example illustrates my alleged link between diversity and dynamism, it is blemished in a number of ways. Firstly, because it was presented as an isolated example, but that (of course) is no genuine defect. More significantly, it gives no sense at all that a major aesthetic drift is involved, for it hinges on an example of diversity familiar well before Newton – Gilbert's distinction between electricity and magnetism. I have rectified this defect elsewhere, however, in an argument to be adduced in the section below.

And finally, the Aepinus example does not illuminate the alliance between mechanism and uniformitarianism brightly enough: for though it is easy to empathize with Aepinus's suggestion that a second aether is required to give a mechanical account of both electric and magnetic forces, the strict necessity of dual aethers is not established by Aepinus's passing remark. In fact (one suspects) Aepinus's message here is simpler: all aethers so far devised are incompatible with the diversity observed by Gilbert. A stubborn opponent of dynamism could well retreat behind this fact, and insist the search go on, recalling Newton's suggestion

[10] R.W. HOME, "Introduction", in R.W. HOME, ed., and P.J. CONNOR, trans., *Aepinus's Essay on the Theory of Electricity and Magnetism* [*Tentamen theoriae electricitatis et magnetismi*, 1759], (Princeton, NJ, 1979), pp. 3-224, on p. 116.

(via the Third Criterion) that an explanation is all the more acceptable, the more difficult it is to devise. So this opponent might insist that Aepinus just try harder.

But in the end, repeated failures to solve a puzzle must count against a programme, even if an undetected solution does in fact "exist". So examples of mechanism that collapse in the face of phenomenal diversity will continue to provide an explanation of the growing accommodation of dynamism, even if a literal reading of Aepinus's claim goes too far.

An excellent illustration of mechanism's vulnerability is provided by someone extremely committed (as we later demonstrate) to the regularity of nature, and with it to Newton's Third Criterion, Leonhard Euler – the mathematician who criticised Aepinus's theory of magnetism because of its compromise here. In a discussion (from the late 1740s) of the friction of solid bodies, Euler sketches a hypothetical mechanism to account for the friction between solids.[11] His mechanism, however, requires extreme uniformity in the behaviour of sliding surfaces, so once Coulomb's systematic measurement showed that friction was not so well-behaved, Euler's attempt to eliminate a very humble dynamism had to collapse.

In developing his model, Euler realises that he has few hard facts about the magnitude of friction at his disposal, though he is aware, from the beginning, that different surfaces generate different frictional forces. But a few things are known to him, and some of these surprise him: the friction exerted on a solid by the surface it rests upon is independent of the surface area ("Sur le frottement" p. 55); and friction seems incompatible with "the law of continuity".[12] He presents his theory as a resolution of this second puzzle, a simple mechanism (figure 1) capable of generating similar discontinuity. But he also observes (ibid., p. 58) that his model indicates that surface area makes no difference, so removes the first puzzle as well. If Euler's task were now complete, there would be no need for him to ask whether he has found the true cause of friction or not, since the paradoxes would be adequately resolved by any indisputably mechanical device that vividly displayed the puzzling behaviour.

[11] L. EULER, "Sur le frottement des corps solides", in *Opera omnia*, 3 series, F. RUDIO et AL., eds., (Berlin-Göttingen-Leipzig-Heidelberg, 1911+), ser. 2, VIII, pp. 54-63, C. BLANC, ed., (1965). E143, in *Histoire de l'Académie royale des sciences de Berlin*, 4 (1748), pp. 122-32 (published in 1750). The mechanism is set out on pp. 57-8. Yet like many earlier mechanists (see A. CHALMERS, "The Lack of Excellency of Boyle's Mechanical Philosophy", *Studies in History and Philosophy of Science*, 24 (1993), pp. 541-64, on pp. 555-61), Euler explicitly allows weight (or some more general 'load') as one of the elements of his reduction of friction (e.g. p. 56).

[12] EULER, cit. 11, p. 56. Euler's precise point here is not clear to me, but the issue certainly does not matter below. Euler is observing something like the following. Suppose a variable external horizontal force is applied to, say, a cubic object supported on a plane surface. As the magnitude of that force increases, there is no visible effect, until the limiting friction is reached, when 'suddenly' a new effect, acceleration, is produced. This 'effect-saltation' is the discontinuity in question.

But it is clear that Euler wishes to go a little further and cast light on the real nature of friction. Indeed, he seems to regard the fact that he has devised a model which resolves two difficulties as evidence that he has got close to the real cause of friction. "A quite perfect resemblance", he says (p. 58), has emerged "between [his] case and that of friction".[13] He speaks of the practical importance (p. 54) of "discover[ing] the nature of friction" (p. 56); and of his ability to render visible "the true cause of friction".

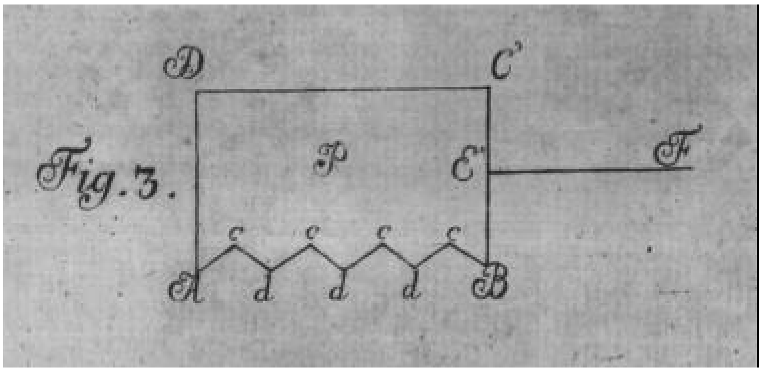

Figure 1 Euler's mechanical explanation of friction: a pair of "hilly" surfaces in contact. For one to slide over the other, vertical motion, (sufficient to separate the hills) is required and force is needed to generate this (From EULER, "Sur le frottement", p. 58).

Yet the theory is only plausible in a uniformitarian's universe, for although Euler's allows frictional forces to vary individually, his theory imposes an important restriction upon that variability (pp. 58-9): "kinetic friction" (that between surfaces in relative motion) will be exactly half the static friction (that between the same surfaces before the motion begins). Because to start motion against friction, requires pushing a load uphill, as in figure 1; but subsequent motion is half uphill and half downhill. So Euler urges an experimental test, to supplement common experience, (already familiar with a vague reduction along the lines he predicts). Euler expects that moving friction will be found to be "considerably less" than stationary friction – and spends the remainder of his memoir discussing phenomena associated with this reduction. Of course, the fact that Euler is not here predicting a reduction of *exactly* 50% shows that he is not committed to the absolute accuracy of this model. But it remains true that the mechanism Euler proposes requires this regularity.

The first systematic study of friction, Coulomb's "most celebrated" work in engineering, was published some thirty years later. "Important for a century and a

[13] "Il en paroit une assez parfaite ressemblance entre ce cas et celui du frottement", ibid., p. 58.

half", this classic memoir won the Paris Academy's 1779-81 competition, for analysis of the effects of friction and stiffness on the operation of naval machinery.[14] On perusing Coulomb's results, it is immediately obvious that Euler's simplistic characterization of friction is definitively refuted. Coulomb finds, for instance, that when an oak surface slides, without lubrication, on another oak surface, the kinetic friction reduces to 25% of the maximal static one (cit., p. 99) – half Euler's estimate. More precisely (and far more seriously), one of Coulomb's principal findings (p. 100) is that there is often no such thing as THE static friction, for the friction depends on how long the loaded surfaces have been in contact, before an attempt is made to start them sliding, and this increases with time. When one surface is metal and the other wood, it can take days of contact before the static friction reaches its maximum! There is no hint of this idea in Euler, and no possibility of reconciling it with Euler's unsophisticated mechanism. Worse still, this phenomenon is verging on the erratic: for when metal slides on dry metal, there is (p. 100) neither time delay nor significant difference between static and kinetic friction. So in this case, Euler has underestimated the kinetic friction to the same extent as he overestimated it for oak.

Yet Coulomb does manage to impose a little order on his results, for he implies that categorising the interacting surfaces as wood or metal, will indicate the broad character of the frictional effects. So his phenomena are not truly erratic, yet they are far from the uniformitarian vision presumed in Euler's analysis. The contrast between the two treatments thus illustrates the aesthetic drift at the centre of our present discussion – a growing sensitivity to the fact that the world is a complicated place, a retreat from earlier simplicities. That the change in attitude apparently associated with this event is real (and not just some coincidental illusion, given undue, spurious and forced prominence here) is indicated by one of the terms of the Academy competition: contestants had not just to base their analysis on new measurements, but new measurements carried out on full scale.[15] It was, then, explicitly recognised that one reason earlier measurements had been unreliable is that too much confidence had been placed on scale models. Yet such trust implies more than just carelessness, it implies a presumption that the phenomenon is so uniform that huge extrapolation is reasonable.

But what does this episode tell us about dynamism, the outlook I claim to be supported by such retreats from uniformitarianism? Does Coulomb's work provide us with more than just another example of premature mechanism being refuted by

[14] CH.A. COULOMB, "Théorie des machines simples en ayant égard au frottement de leurs parties et à la roideur des cordages", *Mémoires de mathématique et de physique présentés à l'Académie royale des sciences par divers sçavans*; 10 (1785), pp. 161-332; repr., (Paris, 1821), references to the Coulomb memoir below are to the 1821 reprint. C.S. GILLMOR, *Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France*, (Princeton, NJ, 1971), chap. 4, pp. 118-38, esp. on pp. 118, 127-8, 136-7. The 1781 competition was on the same problem as that of 1779.
[15] GILLMOR, ibid., p. 128; COULOMB, ibid. p. 2.

experiment? My answer to this rhetorical question is obviously yes, but I am certainly not going to claim that Coulomb's work generated action-at-a-distance explanations of friction. What it does, rather, is illustrate and support an attitude to physics which made action-at-a-distance more acceptable elsewhere. It does this in two quite different ways.

Firstly, Coulomb's results clearly indicated that friction depends "on the nature of the materials in contact, and of their coatings" (as he puts it), for "the variety in the phenomena can only be bound to some essential difference in the nature of the constitutive parts of wood and metals".[16] Coulomb is here using the language of peripatetic physics, and doing so in a way that would once have been highly controversial.[17] Yet he continues to favour mechanical explanations of friction, which he sketches immediately after the words just quoted – so Coulomb certainly does not interpret his results as indicating internal natures to be real things. Yet the variability in his phenomena certainly served to emphasise the difference between matters, and (as Coulomb's own language vividly indicates) help the mind adjust to an outlook exemplified by Dalton (1766-1844) a few decades later, in which the homogeneity of matter is utterly abandoned. The really classic example of this historical phenomenon is provided by the doctrine of elective affinities, slowly growing in strength over the 18th century, and increasingly interpreted dynamically – in terms of microscopic attraction and repulsion between chemical corpuscles.[18] Yet once the homogeneity of matter was finally abandoned, so too was mechanism in the strict, anti-Newtonian sense – and peripatetic natures were of course back in. And once matter was thus activated, action-at-a-distance was no longer so fearful. That is one link between Coulomb's work and quite radical dynamism. There is of course nothing special about Coulomb's work here, and numerous alternative examples of such departures from uniformitarianism could readily have been used to illustrate the dynamic connection – as my brief mention of affinities indicates. We will indeed meet another of the possibilities soon – in our discussion of a similar variety begrudgingly acknowledged by Euler in refractive indices.

The second way in which Coulomb's detection of variety here supported a dynamic outlook, develops out of the wedge it inserts (so to speak) between forces and possible mechanical reductions of them. To see the point here, it is worth

[16] COULOMB, ibid., p. 4 ("de la nature des matières en contact, et de leurs enduits"); p. 101 ("la variété des phénomènes ne peut tenir qu'à quelque différence essentielle dans la nature des parties constitutives des bois et des métaux").

[17] Would it, however, have been reasonable for Molière to have made fun of an engineer who said that friction was the reason why objects in motion tended to slow down?

[18] See, e.g.: A. DUNCAN, *Laws and Order in Eighteenth-Century Chemistry*, (Oxford, 1996), pp. 96-104; A. THACKRAY, *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry*, (Cambridge, Mass., 1970), pp. 199-204. The development of an overtly dynamic interpretation of elective affinities presumably provides a prime example of the thesis of this essay, but my attempts to find concrete illustrations of an associated argument from variability to forces have so far failed.

recalling Newton's proposal (discussed above) that the word "attraction" be used in physics to refer to the well-identified effects of some unknown, but possibly mechanical, cause. Exactly why this suggestion was opposed on the Continent is a complex matter, but some of the reasons are fairly clear: Newton's practice was deemed superficial and even lazy (in the sense articulated in our discussion of the Third Criterion above); and following on from this, it must have been believed that it would be relatively easy to discover a plausible mechanical cause for gravity. Implicit in this second reason for opposing Newton is the uniformitarian confidence that nature is reasonably simple. Given such beliefs, opposition to Newton's nominalistic forces becomes rational: in a simple universe, Newton might reasonably be expected to do more.

But discoveries like Coulomb's – again surely not unique here – remove such grounds for rejecting dynamism. The complexity of the phenomenon investigated meant that patient exploration of the forces acting was not to be deemed a trite thing, even if (as is true of Coulomb) those forces were treated in the most superficially nominalistic sense, without more than passing attention being given to their true nature or causes. It is Euler, not Coulomb, who takes the easy way out here – for he is able to ignore the difficulties later imposed by the newly-discovered variability in the phenomena. The fact that Coulomb won a competition demonstrates clearly that the magnitude of his enhanced task was identified by contemporaries – if this is not evident from the mere fact of the competition having occurred.

Furthermore, the fact that the phenomena were so complex suggested that a mechanical account of them was going to be difficult, so postponement of the search for causes becomes reasonable. Coulomb himself reveals little interest in the causes of his forces, discussing them very briefly indeed, yet he says enough to make it clear he does not accept a single simple cause. His explanation is similar to Euler's, but he allows things like flexibility and elasticity (qualities quintessentially non-mechanical) in some of Euler's asperities (the tiny hills on the surface of wood), for instance. But he makes the metallic asperities "hard and inflexible"; and in addition, follows Desaguliers to allow a small contribution from cohesion.[19] In avoiding a sustained discussion of causes, Coulomb may well have been responding to the practical naval context of the competition,[20] so we cannot judge his personal attitudes by this evidence.[21] But this does not matter; it is enough for us that the Academy's judges rated a non-causal discussion of forces so highly, clearly deeming

[19] See COULOMB, cit., 14, chap. 3 ("Essai sur la théorie du frottement"), pp. 99-105, on pp. 100-5 (and associated figures). For Desaguliers' suggestion that cohesion is a source of friction, see GILLMOR, cit., 14, pp. 124-6, 130-2: Coulomb argues against Desaguliers here, insisting (pp. 100-1) his measurements show the contribution of cohesion to be very small.

[20] Cf. COULOMB, ibid., p. 105.

[21] For Coulomb's contemporary advocacy of action-at-a-distance, see HEILBRON, cit. 1, p. 474. Heilbron observes that Coulomb's opinions here did not offend the "mathematicians in the Academy".

it adequate for practical purposes. For one of the sources of Newton's own nominalistic dynamics, had been the mitigated scepticism of Gassendi, Glanvill etc. – who insisted that it did not matter that deep knowledge of causes was impossible, since such knowledge was of no practical urgency.[22] The net effect of Coulomb's findings then, is to make a search for the causes of friction less promising and less urgent. In other words, it gives prominence to the forces themselves: it was a dynamical rather than mechanical memoir.

This attitude is very clear in Prony's, *Nouvelle architecture hydraulique* of 1790, one of the first works to make extensive use of Coulomb's results, for Prony is quite insistent about the fact that practice does not require deep causal analysis.[23] Furthermore, the structure of Prony's mechanics is often dynamical in the same way as Aepinus's tract was. Instead of seeking to account for the existence of forces, Prony does something that we regard as quite routine: he takes the forces in a mechanical system as given, then deploys the "algorithms" provided by theory to determine the resulting effects. As Vandermonde and Monge put it in a contemporary review, Prony "deduces ... the general formula for the motion of a body acted on by any powers whatever".[24]

Like many other dynamical texts of the era, his discussion contains echoes of the hostility to forces, and he sets space aside to defend his approach. "The measure of motion is ... independent ... of the cause which produces it", he observes, before bluntly posing the big question: "What is the cause of motion?". But his purpose here is purely rhetorical, for he does not in fact think his question can be answered:

> It does not take much reflection to see that the answer is above the power of the human mind, and outside the class of notions that can be attained by natural means, without ascending immediately to the action of a first cause, and [to do] that is to cut off the difficulty, without resolving it ... [In both mechanical collisions and non-mechanical attractions] the cause which produces movement is perfectly unknown, and ... the law governing the effects is the only thing which gives our researches any substance. Happily,

[22] R. POPKIN, *The History of Scepticism from Erasmus to Spinoza*, rev. ed., (Berkeley, 1979), pp. 129-50. Cf. DESCARTES, cit. 3, p. 286; J. GLANVILL, *Essays on Several Important Subjects in Philosophy and Religion*, (London, 1676); rep., (New York, 1970), essay 2, pp. 47-50.

[23] GILLMOR, cit. 14, p. 137. Prony includes a long paraphrase of Coulomb's memoir: see G.F.C.M.R. DE PRONY, *Nouvelle architecture hydraulique* ..., 2 vols., (Paris, 1790), I, pp. 427f. For Prony's scepticism, see ibid., pp. 13-4, 17, 43.

[24] PRONY, ibid., pp. 169-70, 177; A.T. VANDERMONDE and G. MONGE, introductory review ("Extrait des registres de l'Académie royale des sciences [23.XII.1789]"), in PRONY, pp. ix-xii, on p. x: "[Prony] déduit ... les formules générales du mouvement de translation d'un corps, sollicité par des puissances quelconques". I do not mean to suggest here that Prony is the first person to treat mechanics in this dynamical fashion: I do not know anything about the evolution of this attitude, and seek merely to note its dynamical character.

> that is all that interests us in mechanics, and however nice it would be to discover the cause of movement, we can abandon this without regret.[25]

It is unreasonable then, to use our ignorance of the nature of force as a reason for objecting to its use by physicists. For Prony, the notion is appropriately used whenever we know "the law governing [its]effects". Though this rule for accepting a force into physics overlaps that triple criterion attributed to Newton at the very beginning of this paper, the Prony test of propriety falls far short of Newton's, for it abandons the uniformitarian requirement that a force be of extremely general action. Prony has modified the Newtonian test, so that relatively specific actions (like those reported by Coulomb) can be accommodated. All that needs to be known is the law of action. Accordingly, Prony's physics is far more dynamic than that of Newton: as variety gets acknowledged, we see the barriers against accommodating forces being lowered.

Despite his dynamic preferences, and a relaxed acceptance of action-at-a-distance, William Herschel (1738-1822) suggested that physics might be getting into trouble as a result of this retreat from earlier purity.[26] "The very terms of *elective* attraction contain something that either revolts against common sense or at least is very much involved in obscurity and mystery, and may truly be called *occult*", he suggested in 1781, urging an attempt to clear physics of such "arbitrary hypothesis". "To explain polarity by election or choice is the worst of all refuges". What Herschel preferred was to assume four, five (or, elsewhere, ten) general actions-at-a-distance, "each of them acting under very different Laws", but each also a uniformly attractive or uniformly repulsive force, acting "without cessation or interruption, by one constant law". To change the algebraic sign of a force, from attraction to repulsion, at the whim of some observed phenomenon ("as often as there shall be occasion") seemed to him the lazy way out. Far better to explain "the variety of effects" using a relatively small number of regular powers. Herschel retreats then, in the direction of the Third Criterion, but it is his reason for retreating that most interests us: for it again illustrates the claim that accommodation of experimental variety was promoting a tolerance of forces. This very variety indeed was the reason

[25] Ibid, pp. 12-4 (quoting pp. 13-4: "La mesure du mouvement est donc indépendant de sa nature ou de la cause qui le produit ... *Quelle est la cause du mouvement*? ... Il ne faut pas de longues réflexions pour s'apercevoir que sa solution est au-dessus des forces de l'esprit humain, ou est hors de la classe des notions auxquelles il peut parvenir naturellement, à moins qu'on ne remonte immédiatement à l'action d'une première cause; ce qui est trancher la difficulté sans la résoudre ... [L]a cause qui produit le mouvement nous est parfaitement inconnue, et ... la loi suivant laquelle s'opèrent les effets est la seule chose qui donne prise à nos recherches. Heureusement c'est là tout ce qui nous intéresse en mécanique, et, quelque curieuse que fût la découverte de la cause du mouvement, nous pouvons y renoncer sans regrets").

[26] The remarks in this paragraph are based on 3 papers of Herschel from 1780-1,and an undated expansion of one of these (of the same name): see W. HERSCHEL, *The Scientific Papers of Sir William Herschel including Early Papers hitherto Unpublished*, 2 vols., (London, 1912), I, pp. lxix-lxx, lxxv-vii, ciii, cxi.

that Herschel's own dynamics allowed many more forces than the "two or three" of Newton's dreams. For when accused himself of multiplying causes beyond necessity, it was to the differences revealed in the phenomena that Herschel, like Aepinus, appealed: "This one experiment ... shows that ... [the "two repulsive powers"] must be very different powers. Moreover, from ... experiment I have ... proved, that between [them] at least one attraction prevails". "So far from retracting, I confess that I am partly sure that I have not named *all* the central powers ... [and] perhaps ... may soon prove the existence of *four more*".

4. Achromatic Lenses: The Euler-Dollond Debate

Although the above discussion of friction certainly supported our story (mainly by illustrating the difficulty of reconciling empirically discovered variability with mechanism) it was defective in two important respects. It did not involve any commitment to action-at-a-distance; and it did not reveal significant hostility to the notion that nature might be full of variety. Indeed Coulomb's text gives the impressions that his audience would have no difficulty accepting that friction between metals might be very different from that between timbers, and this impression may well be accurate. Yet there is good evidence that such toleration to variety had not always been displayed. For some thirty years earlier, around 1750, a major controversy had erupted over something analogous, the variability of refractive indices, a controversy that also strengthened the case for genuine (though primarily short-range) action-at-a-distance. The debate here centred on the exploitation of optical irregularity by the English instrument-maker John Dollond (1706-1761) to produce achromatic lenses, patented in 1757. More particularly, it centred on Euler's "paradoxical" refusal to acknowledge Dollond's success – "paradoxical" because it was Euler's earlier heretical prediction that such lenses were feasible that had prompted Dollond's hesitant search.

I have however analysed this controversy in much detail elsewhere,[27] using it mainly to reveal the uniformitarian commitments of Euler and his contemporaries. It demonstrates (with exceptional clarity) active hostility on Euler's part to the possibility that nature might be erratic, far stronger than the purely passive uniformitarianism displayed in the account of friction. And similar presumptions can be detected in the contributions of others, though many of these are less explicitly made (and hence less readily seen). Yet in that earlier discussion, I did not emphasise the dynamic connection that matters to us here. So let us review the debate briefly, repeating some of my earlier analysis to expand this somewhat new emphasis.

Behind the whole story lies another of Newton's dreams, a project comparable to that which is the target of much software research today. Newton hoped to produce a

[27] HUTCHISON, cit. 5.

wide-ranging mathematical optics, capable of predicting the visual effects produced when a specified distribution of media reflected and transmitted the light produced by a given array of light sources.[28] Newton recognized that such a theory would need empirical input, but he believed the inputs would be relatively small: a few measurements of critical refractive indices, supplemented by systematic density measurements (enhanced by some determinations of sulphur-content[29] – which seemed to be inconveniently linked with refraction). So Newton's algorithm was not to require the input we would expect modern ray-tracing software to demand – where the user would surely have to list the optical properties of the refracting media; and to do so systematically, over and over again, for each surface in the system being modelled. If we ignore the inconvenient (but initially minor) problem of sulphur-content, all that Newton required (by contrast) was a purely mechanical parameter, the density. For since Newton's media were deprived of all Aristotelian natures, they simply did not possess separate "optical properties". Just as the Third Criterion forbids celestial mechanics from attributing individual gravitational properties to the planets, the Newtonian optician could acknowledge no especially optical characteristics in his individual pellucid media. Indeed, in both cases, Newton toyed with the idea that the observed effects were not even produced by their apparent causes, the media – but by an associated aether instead, and if that was so, elaborate details of the gross matter involved were patently irrelevant: all that mattered was its ability to retain aether.

Yet just as Newton's uniformitarianism had (in the case of gravity) the empirical consequences we glimpsed above, so too in the case of light, were interrelations required between the optical parameters of various media. In particular, a quantitative connection had to exist between the four refractive indices (r, b, R, B) that Newton had found to be involved when rays of two colours (say, red and blue) each passed across different interfaces (say air-glass, and air-GLASS, using capitals to distinguish one type of glass from another).[30] Various such relationships are articulated among Newton's papers, and one of them is announced in the *Opticks*, viz. $(R - 1)/(B - 1) = (r - 1)/(b - 1)$, a result there interpreted as indicating that

[28] NEWTON, cit. 4, pp.131-2; HUTCHISON, cit. 5, p. 138, (citing: esp. A. SHAPIRO, "Newton's 'Achromatic' Dispersion Law: Theoretical Background and Experimental Evidence", *Archive for History of Exact Sciences*, 21 (1979), pp. 91-128, on pp.102-5; Z. BECHLER, "Newton's Law of Forces which are Inversely as the Mass: A Suggested Interpretation of his later Efforts to Normalise a Mechanistic Model of Optical Dispersion", *Centaurus*, 18 (1973-4), pp. 184-222 on p. 193).

[29] In ignoring the sulphur-problem, we are simply following much contemporary practice: see HUTCHISON, cit. 5, p. 149.

[30] Newton is very explicit about the fact that his two interfaces involve only three media, and does not discuss cases where rays passing from air to glass are compared with others passing from (say) water to GLASS. This restriction became an important component of the debate, as discussed below; see also HUTCHISON, cit. 5, pp. 146-8.

optical deviation always generates dispersion, even with multiple refractions.[31] The result then, appears to assert a sufficient condition for the impossibility of eliminating chromatic aberration by using composite lenses.[32] This conclusion of Newton's remained the orthodoxy for another half-century, until it was challenged in a purely theoretical 1748 discussion by Euler.[33] Without explaining himself, Euler used a slightly different relationship, $\log r/\log b = \log R/\log B$ to determine one of the refractive indices involved in estimating the performance of a composite lens, and equipped with such a discordant index was able to design a lens allegedly free of the distortion imposed by Newton's different regularity. Initially, Euler's seemingly *a priori* speculations were scorned by the Newtonian establishment, and by Dollond in particular, but a debate was initiated which soon cast unexpectedly compelling doubt on Newton's reliability here. So Dollond promptly ate his words and, checking the matter empirically, had to withdraw his support for Newton: the lenses were indeed possible – though ironically it turned out that the successful designs were not Eulerian. Indeed, they were just as incompatible with the logarithmic law as with Newton's.

On receiving this news, Euler dug his heels in, refusing to admit that his former protagonist Dollond really had constructed a lens that minimised chromatic distortion. Normal human pride must surely have tempted Euler to claim victory here, so whatever distanced Euler from Dollond's discovery must have meant a lot to him. Fortunately, we can easily see what mattered, for Euler articulates his motives in the course of debate, gradually disclosing the source of the 1748 logarithmic relationship. That law, he observes, is a simple consequence of several uncontroversial principles, only one of which matters here: the radically uniformitarian rule (designated the "Fundamental Hypothesis" by Euler) which asserts that whenever two refracting interfaces are such as to make $r = R$, it must also follow that $b = B$. In other words: if two interfaces have the same optical effect on red light, they must have the same effect on blue light; or, alternatively, the refractive index for blue light is a function of the index for red light.

Such a Fundamental Hypothesis is clearly a prerequisite for any coherent ranking of media in terms of their "optical density", and virtually everyone that took part in the debate accepted something very similar to this hypothesis, even when they did

31 For Newton's (somewhat different) statement, see NEWTON, cit. 4, p. 130 ('Theorem 1'). Newton's statement of this result is superficially different from my own here, for I state the result in the form of an equation standardly used during the Euler-Dollond debates. There is no difficulty reconciling the two.

32 More precisely, the sufficient condition is only approximate, but it is a reasonable approximation. See: Z. BECHLER, "The Disagreeable Case of Newton and Achromatic Refraction", *British Journal for the History of Science*, 8 (1975), pp. 101-126, on p. 124; SHAPIRO, cit. 28, pp. 99-102.

33 L. EULER, "Sur la perfection des verres objectifs des lunettes", in *Opera omnia*, cit. 11, ser. 3, VI, pp., 1-21, E. CHERBULIEZ and A. SPEISER, eds., (1962). E118, in *Histoire de l'Académie royale des sciences de Berlin*, 3 (1747), pp. 274-96 (presented 27.IX.1748, read in 1748, published in 1749).

not realise it, or even as they endorsed opinions inconsistent with it. The uniformitarianism (so clearly visible in Euler) was widely shared then, though to detect it elsewhere we need to see that it lay hidden in related presumptions. So just as Newton had declared $(R - 1)/(B - 1) = (r - 1)/(b - 1)$, without seeing that this entails the Fundamental Hypothesis, and hence also Euler's incompatible logarithmic relationship, others invoked alternative rules that had much the same effect;[34] or thought in terms of optical density; or endorsed Newton's claim that the spectrum was musically divided; or traced the path of some "median" ray – without realising (the not too obvious fact) that all such devices depending on nature displaying the very regularity insisted upon by Euler. Initially, it was only Euler who saw that such presumptions led to the logarithmic law; so, given that Dollond claimed to use lens-elements whose indices were not interrelated logarithmically, it followed that Euler could not acknowledge the success of the new lenses without abandoning all these widely-accepted beliefs. In short, Dollond's erratic refracting indices were "bizarre and revolting", and "the greatest of paradoxes" as Euler put it, in an attempt to warn his contemporaries.

By the late 1760s the controversy thus initiated had died out – presumably because the success of the new lenses had become too familiar for the shocking facts they depended upon to be denied – theoretical prejudice had to yield to crude fact. D'Alembert (1718-1783) had urged just such a resolution in 1764, when he insisted that experiment was the only way of determining the issue:[35] "One can neither establish, nor soundly attack, either the equation of M. Newton or that of M. Euler *theoretically*", he observes. "Experience is the only perfectly reliable means of determining, not only the quantities [R, B, and b], but also the quantity [r] when one knows the others". "Some great geometers" have appealed to the "uniformity of the laws of nature" here, he adds (in a 1773 sequel to this discussion),[36] but such reasoning is inconclusive. For "we do not adequately know where the true uniformity of the laws of nature resides". The alleged uniformity "can lead us into error, since, by relying on this principle, one would be able to assign various laws of refraction, quite different from each other, and all equally plausible". Both Newton's refractive relation, and Euler's logarithmic alternative, can be defended via uniformitarianism, so that is not the way to decide the issue.

The sort of target d'Alembert had in mind was an argument of Euler's (from the earlier 1750s) which sought to discredit Newton's relationship by showing it to

[34] More precisely, what Newton is committed to is a three-media-only restriction of the Fundamental Hypothesis: see above, note 30. Cf. HUTCHISON, cit. 5, pp. 148-9.

[35] J. D'ALEMBERT, "Essais sur les moyens de perfectionner les verres optiques", *Opuscules mathématiques*, 8 vols., (Paris, 1761-80), [16th-20th Mémoires], III (1764), pp. 1-420, on pp. 342-3. Cf. ANON., [Review of D'ALEMBERT, "Essais sur les moyens …"], *Histoire de l'Académie royale des sciences*, (1764), pp. 92-6, on p. 94 (published late in 1767).

[36] J. D'ALEMBERT, "Eclaircissemens sur une prétendue loi de la réfraction de la lumière & Supplement", in *Opuscules*, cit. 35, [49th Mémoire], VI (1773), pp. 260-303, on pp. 301-2.

contradict another of the *Opticks* theorems – one never subject to doubt. But Euler's conclusion depended on a uniformitarian misreading of the Newtonian claim, adapting it to four media instead of just the three specified in the original text – as elaborated in my note 30. The presumption made here (perhaps unconsciously in Euler's case) is that nature is so uniform that a law which applies to restricted circumstances will also hold in a wider context; and d'Alembert tells us (in the 1773 piece just quoted) that some participants in the debate took this view deliberately, citing uniformity to justify the misreading in question.[37]

Another of d'Alembert's targets was a 1761 discussion by his rival Clairaut (1713-1765), which sought to refute Euler's logarithmic law without making any decision about the true cause of refraction.[38] But Clairaut does indeed introduce a presumption about that cause – for he assumes (e.g., cit., pp. 383, 422-3) that the different refractions associated with different colours derive from velocity differences alone – so the matter of his refracting medium interacts with blue light in the same way as it affects red light. Without this uniformitarian assumption, observes d'Alembert (cit., p. 361), the refutation fails – and though Dollond's success had empirically refuted the *Opticks*'s relationship, it was still not clear if measurements contradicted Euler's alternative. More studies, he said (p. 367) were needed.

D'Alembert's discussion here also reveals the way the breach with uniformitarianism that is central to this episode made action-at-a-distance more palatable. It did this via the same two methods as we have already met elsewhere: it undermined (firstly) the preferred mechanical explanation of refraction; and it undermined (secondly) the whole ontology of mechanism, by indicating that matter was not qualitatively homogeneous. To see this, we must recall Newton's preferred

37 For d'Alembert's diagnosis of Euler's error, see D'ALEMBERT, cit. 35, p. 356ff. For the discussion of Euler's being attacked, see L. EULER, "Examen d'une controverse sur la loi de réfraction des rayons de différentes couleurs par rapport a la diversité des milieux transparens par lesquels ils sont transmis", in *Opera omnia*, cit. 11, ser. 3, V, pp. 172-84, on p. 176, D. SPEISER, ed., (1962). E216, in *Histoire de l'Académie royale des sciences de Berlin*, 9 (1753), pp. 294-309 (published in 1755), further discussed in HUTCHISON, cit. 5, pp. 143-4, 152). For d'Alembert's claim about contemporaries citing uniformity, see D'ALEMBERT, cit. 36, pp. 301-2.

38 For d'Alembert's identification of Clairaut as a target, see D'ALEMBERT, cit. 35, p. 361. For the target, see A.C. CLAIRAUT, "Sur les moyens de perfectionner les lunettes d'approche [part I]", *Mémoires de mathématique et de physique tirés des registres de l'Académie royales des sciences* (1756), pp. 380-437, on pp. 383, 403-5 (read in 1761, published late in 1762). D'Alembert in fact misunderstands Clairaut here, as he later admits (D'ALEMBERT, cit. 36, p. 302). For Clairaut is claiming less that the logarithmic relationship is false than that it has not been proven. Euler had claimed (e.g. EULER, cit. 37, p. 183) his law was a necessary truth, a direct consequence of Newton's discovery that differently colours rays are differently coloured (and he is perhaps right, if enough uniformity is granted). Clairaut replies with a plausible model of this dispersion, incompatible with the logarithm law. But d'Alembert is right to detect a significant measure of uniformitarianism in Clairaut's discussion, and Clairaut himself recognises (on pp. 422-3) that the empirical facts require more variability than accommodated in his attack on Euler.

explanation of light, as a stream of particles subjected to the attractions and repulsions associated with the media through which the light passed. Such forces changed their magnitude near optical interfaces, as the density of the nearby media changed. Light corpuscles in the immediate vicinity of an interface thus experienced a net, unbalanced force, and so ceased briefly to move inertially. This was the source of the familiar deviations and dispersions.

Widely accepted by 18th-century physicists (with the notable exception of Euler), this theory did not initially have unequivocal dynamic overtones, for much the same reason as celestial mechanics lacked them: it was presumed that aether could be harnessed to give a suitably mechanical account of the forces involved.[39] Like gravitational attraction, the refracting forces were just names for actions which (perhaps) were really mechanical. So refraction was widely believed to be explicable in terms of some change in the distribution of aether near the interface involved, perhaps a mere change in density (like that envisaged by Newton to explain gravity). The resulting elasticity gradient would then exert temporarily unbalanced forces on light particles transiting from one medium to the other, and a short curvature would thus be inserted into their otherwise rectilinear inertial trajectories.

But such a means of avoiding a dynamic explanation made it highly like that the Fundamental Hypothesis (or at least some similarly uniformitarian variant thereof)[40] would be observed. For if a pair of interfaces satisfied the equality $r = R$, the two interfaces would be associated with an equally potent aether-modification, so all light corpuscles crossing them would experience the same change-in-force and hence be similarly modified. If that were so, the two interfaces would seemingly have similar effect upon blue rays, so the equality $b = B$ could be expected to hold. Thus the Hypothesis (or perhaps just some close relative) would be exemplified. The aether mechanism, in other words, imposed extreme uniformity upon refraction.

In consequence, the fact that nothing like the Fundamental Hypothesis was true – the fact that refractive indices were too variable for this – meant that an aethereal explanation of the refractive forces was implausible. So the mechanical alternative to dynamism collapsed here too, and far more compellingly than in the friction case, or in the case of electricity and magnetism, for (as we soon see) the microscopic forces revealed by macroscopic refraction were so variable that an extraordinarily large number of different mechanisms would be needed to avoid the appeal to force, one per colour and interface perhaps. In consequence, Aepinus's argument was in effect magnified a thousand-fold!

Furthermore, the forces associated with refraction could not now comply with Newton's Third Criterion, for the essence of Dollond's success lay in the fact that

[39] For some examples, see HUTCHISON, cit. 5, p. 150.

[40] The 'Fundamental Hypothesis' does not, in fact, embrace a very popular mid-18th-century theory of colour, according to which different colours have different speeds. But this theory does allow a simple (and equally uniformitarian) variant of the hypothesis. See HUTCHISON, cit. 5, pp. 148-9, 171 (note 12).

the forces acting on the blue rays were radically different from those affecting the red. So Newton's refuge was closed and a multiplication-of-entities, another thousand times worse than that imposed (to Euler's chagrin) by Aepinus's theory, was required. But again, there was a good excuse: reality. The theory was forced to break with philosophical etiquette, because physicists could find no other way of accommodating the phenomena. The forces seemed to be part of nature. And unlike the friction case, the forces in question here certainly acted at-a-distance – the theory had never proposed that light corpuscles deviated as a result of contact forces. Indeed, refraction and dispersion were widely cited as prime evidence for the existence of short-range actions-at-a-distance, to supplement the long-range force well demonstrated by Newton.[41]

D'Alembert's 1764 review illustrates all this for us. For in order to reveal the uniformitarian presumptions that plague the analyses of his contemporaries, d'Alembert harnesses the Newtonian explanation of refraction set out above, and presents a routine calculation of the refractive index of an interface, in terms of (a) the incoming velocity of the light corpuscle and (b) the unbalanced force exerted on it in the vicinity of the interface. From this, he derives a formula linking the four critical indices (r, b, R, B above), a generalisation of both the *Opticks* result and Euler's alternative. This more general relationship is rather complicated, but becomes greatly simplified by doing what d'Alembert observes is "ordinarily" done, and adopting the uniformitarian assumption that "the same medium acts in the same manner on the rays of different colour".[42] For this simplified formula to be reconciled with dioptric observations, very great differences in the incoming velocities of different colours of light need to be assumed. Yet such velocity differences are quite incompatible with what is seen when the moons of Jupiter emerge from eclipse. So the uniformitarian presumption has to be wrong. So wrong indeed, that we must presume that the corpuscles associated with different light colours must be composed of different types of matter, exerting different types of action-at-a-distance forces.

Mechanism, then, is quite implausible: for even if the action-at-a-distance forces (presumed here) really result from hidden mechanism, that mechanism will be different for the different matters involved. So homogeneity is greatly challenged. There is "nothing revolting" about this "fundamentally", d'Alembert pleads, for it is already accepted that light corpuscles differ in speed, mass and figure, so why not add "the nature of the material which composes them". For "if the rays differ in species", there is no trouble accommodating the refractive behaviour. It becomes

[41] E.g. R.J. BOSCOVICH, *A Theory of Natural Philosophy* [*Theoria philosophiae naturalis redacta ad unicam legem virium in natura existentium*, 1763], J. CHILD, trans., (Chicago, 1922), p. 163.
[42] D'ALEMBERT, cit. 35, p. 344.

quite plausible "that the action which the refracting medium exerts on light corpuscles should have a different intensity for different kinds of rays".[43]

Clairaut had earlier reached a similar conclusion, after struggling somewhat to resist it – to avoid the resulting obligation "to multiply the properties of matter" in endowing "the particles of light ... with tendencies different from one another" (as he puts it). Elsewhere, he articulated a converse of this concern: Dollond's results show that refraction depends on the nature of the material composing the refracting medium; and his reviewer noted the same point.[44] From the context it is clear that Clairaut was not here tritely citing the familiar fact that different media have different refractive indices. What he was saying is stronger: the matter of the refracting medium plays an active role in the process. Clairaut sees such a conclusion as slightly dangerous, but like Coulomb on friction, does not pursue the question far enough to insist that different media really do have different natures. Quietly though, both investigators move in that direction. D'Alembert does it more noisily.

We can see a similar shift documented in the somewhat later *Lectures* of Thomas Young (1773-1829). For though Young presents us with a dynamical physics, tolerant of action-at-a-distance,[45] he remains (with Herschel's accuser) loyal to the mechanical tradition that matter is "originally of one kind, owing its different appearances only to the form and arrangement of its parts" (cit., p. 606). In defence of this supposition, he challenges (p. 613) the specific forces presumed by Dalton to account for the law of partial pressures; and complains (p. 615) about a new fashion for ignoring Newton's Third Criterion, in deriving the phenomena from a "complicated labyrinth" of forces, "varying according to certain intricate laws, which are supposed to be primary qualities, and for which it is a kind of sacrilege to attempt to assign any ulterior cause". Yet when Young himself sought such causes

[43] Ibid., pp. 346-7: "Cette supposition ... n'a au fond rien de révoltant; car les corpuscules de lumière de différente réfrangibilité peuvent différer entr'eux ... par la nature de la matière qui les compose; or n'est-il pas possible que si les rayons diffèrent de la sorte, l'action que le milieu réfringent exerce sur les corpuscules de lumière, ait une intensité d'action différente pour les différentes sortes des rayons".

[44] A.C. CLAIRAUT, "A Letter ... to ... Birch ... Concerning the Difference of Refrangibility of the Rays of Light", *Philosophical Transactions*, 48 (1753-4), pp. 776-80, on p. 777; and CLAIRAUT, cit. 38, p. 423 (reports a measured ratio of refractive indices "qui détruit ... la possibilité de rapporter la différence de réfrangibilité ... à la différence de vitesse des rayons colorés, ou à une différence de tendance qui dépende uniquement de la nature des corpuscules mêmes, & non de la constitution des parties du corps réfringent"). ANON., [review of CLAIRAUT, cit. 38], *Histoire de l'Académie royale des sciences*, (1756), pp. 112-26, on p. 124, (published late in 1762): "c'est à la texture du corps réfringent & non à la différente vitesse des particules de lumière, que la réfraction peut être attribuée".

[45] For examples of Young's acceptance of action-at-a-distance, see T. YOUNG, *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, 2 vols., (London, 1807), repr., (New York, 1971), I, pp. 76, 612.

(via a hypothetical aether), he found himself confronted by a magnified version of the problem that faced Aepinus, a pressure to invoke a hierarchical multiplicity of such media.[46] "The grand scheme of the universe", he pleads (pp. 614-5), "must surely, amidst all the stupendous diversity of parts, preserve a more dignified simplicity of plan and principles". The key to this ultimate simplicity may, however, lie outside the realm of the purely material (pp. 606, 610-1).

Yet despite such ontological preferences (and his complaint about Dalton), Young himself is sometimes willing to invoke specific forces. So, to explain refraction according to "the system of emanation" (which, as a wave-theorist, Young personally abandons), he insists "it is necessary to suppose that ... refractive mediums have an elective attraction".[47] Yet this (he continues, p. 463) is "a property foreign to mechanical philosophy, and when we use the term ... we only confess our incapacity to assign a mechanical cause for the effect, and refer to an analogy with other facts, of which the intimate nature is perfectly unknown to us". For (p. 617) "the whole of our enquiries, respecting [such ultimate questions], must be considered as speculative amusements, which are of no further utility than as they make our views more general and assist our experimental investigations". In using such agents, we are really only declaring our extreme ignorance – and action-at-a-distance forces themselves can be tolerated only because we cannot expect the universe to be fully intelligible.[48] Indeed, Young's main discussion of such questions is placed very late in his *Lectures*, because answers to them (he indicates, p. 606) make so little difference to physics.

So Young introduces forces into his physics in the same sceptical fashion as we have already met with Prony, and while both abandon Newton's Third Criterion, Young has indicated something important to our case here, but not overt in Prony – the fact that phenomenal variety drives the extreme dynamism of his more radical contemporaries.

In his 1795 *Encyclopaedia Britannica* entry on optics, an enthusiastic John Robison (1739-1805) expands d'Alembert's anti-uniformitarian interpretation of the Dollond-Euler controversy, and cites the great variability of dioptric phenomena as evidence of a full-blooded dynamism.[49] Although Robison has quite different reason

[46] YOUNG, ibid., pp. 610, 616; G.N. CANTOR, "The Changing Role of Young's Ether", *British Journal for the History of Science*, 3 (1970), pp. 44-62, esp. on pp. 45, 59-61.

[47] YOUNG, ibid., p. 463. (Young does not mention achromatic lenses in this immediate context). For another example of Young using specific forces, see T. YOUNG, *Miscellaneous Works*, G. PEACOCK, ed., (London, 1855), I, pp. 563-74. I do not understand why Young chooses to attack Dalton's forces (see nn. 44-5), when here he seems happy with such devices. The objection to Dalton does however occur in a section of the *Lectures*, (cit. 45), that directly confronts the essential properties of matter (p. 613).

[48] For the unintelligibility etc. of action-at-a-distance, see YOUNG, cit. 45, pp. 615, 617, 630.

[49] For Robison's discussion of the achromatic lens controversy, see J. ROBISON, "Optics", in *Encyclopaedia Britannica*, 3rd ed., (Dublin, 1795), XIII, pp. 231-364, on pp. 285-6 (from which the quotations immediately below are taken).

for believing that ordinary bodies exert forces on nearby light-corpuscles, achromatic lenses remind him how specific these actions are:

> We are quite unacquainted with the law of action [of such forces] ... All that we can say is, that ... light is deflected ... attracted ... and repelled ... with a variable intensity. The action may be extremely different, both in extent and force, in different bodies, and change by a very different law ... But, amidst all this variety, there [are a few regularities].

So the different kinds of light are in effect different chemicals, with different natures, and part of the complicated schema of elective attractions. "Not knowing any cause to the contrary" (he claims), Newton presumed otherwise, supposing "that the action of all bodies was similar on the different kinds of light". This, however, "was gratuitous", and:

> … might have been doubted by him who had observed [in, e.g., Q. 31] the analogy between the chemical actions of bodies by elective attractions and repulsions, and the similar actions of light ... [From this analogy] we might expect not only that some bodies would attract light in general more than others, but also might differ in the proportion of their actions on the different kinds of light, and this so much that some might even attract the red more than the violet. The late discoveries in chemistry show us some very distinct proofs, that light is not exempted from the laws of chemical action, ... and [there] are strong proofs of its chemical affinities ...

5. Robison and Boscovich (1711-1787)

Robison was an ardent supporter of the dynamical outlook in physics, yet, even at the dawn of the new century, it is clear he feels a measure of sensitivity about the respectability of forces. For, like Newton, he sometimes claims to take no stand on the nature of such agents, using them merely as a condensed description of the phenomena.[50] Yet his own opinion is clear: he opposes aethereal explanation, and (recalling a long tradition of criticism)[51] insists that the contact actions essential to the original mechanical philosophy are not in themselves intelligible, so must be further reduced – to forces acting at-a-distance.[52] So, despite the sporadic apologies, action-at-a-distance is also deployed by him routinely, without hesitation, and in the

[50] See, e.g., J. ROBISON, *A system of Mechanical Philosophy*, D. BREWSTER, annot., (Edinburgh, 1822), I, pp. 17-8, 208-11, 264, 297-8. This work apparently contains several of Robison's wonderful *Encyclopaedia Britannica* entries. In his preface however, Brewster suggests that some of these entries may have been modified for the reprint. I have not yet been able to check my citations against original *Encyclopaedia* entries.
[51] See, e.g.: BOSCOVICH, cit. 41, pp. 77, 95; I. KANT, *Metaphysical Foundations of Natural Science* [*Die metaphysische Anfangsgründe der Naturwissenschaften*, 1786], J. ELLINGTON, trans. etc., (Indianapolis, IN, 1970), chap. 2 ("Metaphysical Foundations of Dynamics"), passim, e.g. pp. 41, 56, 59; HUTCHISON, cit. 4 (1982), p. 253, esp. n. 49.
[52] For such an attack on mechanism, see: ROBISON, cit. 49, pp. 262-4.

language of blunt realism:[53] "There really exist in nature mechanical or moving forces" he insists – in a passage that (conveniently for us) goes on to attach this realism to variability. For these forces act "like gravitation, at a distance, but [are] clearly distinguishable from it, by their law of variation by a change of distance ... [T]hese ... forces seem limited in their exertion to a small fraction of an inch ... and in this narrow bounds we observe great diversity in the intensity".

At first, it seems that the pressure of facts has forced Robison to abandon the Third Criterion, for he frankly (and self-consciously) uses specific forces attached to particular matters. "*Specific mechanical* affinity analogous to chemical affinities or elective attractions ... different in different substances" are required to account for erratic phenomena like those displayed in capillarity, he says,[54] contrasting his own data here with the far more uniformitarian accounts earlier offered by Clairaut, and then Lalande (1732-1807). But the phenomena here are far more erratic, Robison declares (cit., pp. 260-1): "The modifications of cohesion are innumerable, producing an endless variety of sensible forms ... in each of which the law of action ... is probably different. Also in each these forms we have subordinate varieties ... All and each of these are ultimately ... forces". Furthermore, they indicate that different materials have different natures. For "the specific attraction [depends] on the constitution of the particles both of the solid and the fluid" he explains (p. 236) – before turning to optical forces in illustration, optimistically claiming that Newton's brief reference to the sulphur-anomaly (mentioned above) indicates that the master himself had acknowledged the forces here were not general.

But on closer examination, the retreat from Newton is not so great. Robison's *physics* certainly uses many more forces than are used by Newton, but forces are greatly reduced in Robison's preferred *ontology*. For (like Volta) Robison is a great admirer of Boscovich, and what he particularly likes about the Jesuit's elegant theory is the clever way it can be harnessed to deal with erratic actions.

Boscovich's hypothesis is that the physical world consists of a very large number of dimensionless point-masses, all identical. Each individual Boscovichean "particle" is endowed with inertia; and each pair of them interacts via a central force, whose law of action is complicated, with numerous changes from repulsion to attraction and vice versa. But the force is completely uniform, in that the same law applies to every pair of particles. To get a variable force, Boscovich invokes what we might term a "molecule" – the composite created out of *several* particles, with each pair separated by one of those many distances that generate a stable bond (as the basic force changes from repulsion to attraction).

The force exerted externally by such a molecule turns out to be dramatically different (at short and medium ranges) to that associated with an individual mass-

[53] For examples of realistic action-at-a-distance explanations, see: ROBISON, ibid., pp. 263, 279, 284, 286, 304; and ROBISON, cit. 50, pp. 318-9. The quoted passage is from ibid., p. 253 (and a similar connection with variability is made at ibid., p. 258).
[54] ROBISON, cit. 50, pp. 229-30.

point. At long ranges however, the composite force is purely gravitational in character – as celestial mechanics requires. Furthermore, the character of the short-range forces is highly sensitive to the distance between the components of the molecule. So by varying this distance, a great variety of forces can be created. And by adding even more extra mass-points to the molecule, the family of forces can be further increased – hugely so. Boscovich himself explored this virtue of the theory at some length, but did not give it an especial emphasis, and certainly did not commend his theory because of any special ability to handle variety.[55]

But Robison, by contrast, treats this feature of the theory as the answer to Young's "prayer" (above) – a single homogeneous matter, capable of eliminating the multiplication of entities that experimental variety was seeming to impose upon their less puritanical contemporaries. Moreover, while Boscovich buries these facts about "molecules" in the middle of his treatise, Robison focuses on them, placing them right at the beginning of his discussion of Boscovich – immediately after expressing the hope that "it is highly probable that a steady and judicious [study] would bring to light some *general* laws".[56] It is therefore clear that he regards them as something especially important about Boscovich. So here is an exceptionally clear case of Volta's dynamic outlook being urged because of its special ability to accommodate variety.

[55] BOSCOVICH, cit. 41, pp. 149-89.
[56] ROBISON, cit. 50, p. 267. The discussion of Boscovich here begins on p. 267, sets out the foundational ideas on p. 268, then immediately turns (briefly, at p. 269) to the theory's ability to reconcile variety with uniformity. The extended discussion of how this is done is on pp. 273-82.