Response to Belot's "Whose Devil? Which Details?"

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[1] I would like to thank Gordon Belot for writing his thought provoking paper and for discussions about this reply. Needless to say, I am sure he is not convinced by my response.

1 Introduction: Belot's Argument

Belot [5] presents a fair and detailed discussion of one of the themes I develop in my book. This is the idea that fundamental theories that are related to less fundamental "theories emeritus" by singular asymptotic limits, will very likely, themselves, turn out to be explanatorily inadequate. Belot's strategy in arguing against this claim is to demonstrate that the mathematics of the fundamental theory contains, in an appropriate sense, the mathematics of the emeritus theory. And that, therefore, my appeal to the physically interpreted mathematics of the emeritus theory in explanation is eliminable in favor of the physical interpretation of the mathematics of the pure fundamental theory. If this is right, and if such explanation provides understanding, then fundamental theories (at least those considered in the book and by Belot) are perfectly explanatorily adequate. Belot's discussion is clear, but perhaps it is worth rehearsing briefly the context once again for the case of the rainbow.

From the point of view developed in my book, the idea is that the wave theory of light—the fundamental theory—is asymptotically related to the ray theory of light—the emeritus theory—in the limit as the ratio of the wavelength of light to the other relevant length scales (raindrop radius, for example) approaches zero. My claim, in a nutshell, is that one cannot explain various features of the rainbow (in particular, the universal patterns of intensities and fringe spacings) without ultimately having to appeal to the structural stability of ray theoretic structures called caustics—focal properties of families of rays. Belot, on the contrary, attempts to show that it *is* possible to speak about these structures and their stability without mentioning the concept of a ray of light, or of a family of such rays, by sticking purely to the language of wave theory where such old fashioned concepts make no appearance. Thus, I am being accused of improperly reifying the mathematical structures of the superseded emeritus ray theory when I claim that such structures are required for genuine physical understanding. This objection has also been raised by Michael Redhead in his review of my book forthcoming in *Studies in History and Philosophy of Modern Physics.* Redhead says

> What Batterman is effectively doing is to *reify* this auxiliary mathematics so that the ray structure becomes part of the physical ontology of a new third theory inhabiting what he calls the "no man's land" between the wave and ray theories. But why

can't we leave the asymptotic analysis of universality at the level of a purely mathematical exercise? This would be in line with other developments in theoretical physics where surplus mathematical structure with arguably no physical reference is used to explain or "control" what is going on in a physical theory. Modern gauge theories are an obvious example of this sort of thing. [8]

2 Response

2.1 Asymptotic Explanation

Let me begin my response by emphasizing one point about explanation that, I believe, has been overlooked in these criticisms. I intended the arguments in the book in part to demonstrate how very different the kind of explanation required for an understanding of various universal features of, say, the rainbow is from the types of explanations talked about in the extant philosophical literature. That is to say, most contemporary philosophical accounts of explanation simply do not have the resources to incorporate the types of asymptotic analysis that Belot presents in his explication of the explanation of the rainbow. As I said, and as Belot quotes, "... [t]hese emergent phenomena are not derivable in any straightforward sense from the underlying wave theory. They are not, as it were, from-first-principle solutions to the wave equation. ... They are deeply encoded in that equation but are apparent only through its asymptotic analysis."[4, p. 118]

The nature of this asymptotic explanation is really quite different (as Belot's extended technical discussion ably demonstrates) from the "standard" (D-N type) accounts whereby one plugs the appropriate initial conditions and boundary conditions into the fundamental (wave) equation, and then deterministically/deductively grinds out the appropriate wave function for the system's state at some later time. The asymptotic investigation, involving the principle of stationary phase, the projection of the bicharacteristic strips from the appropriate Lagrangian manifold onto configuration space, etc., really does not much resemble standard accounts of "deriving the explanandum from the explanans."

Yet, Belot's footnote, [5, p. 18] suggests that there is no genuine fundamental distinction between the type of asymptotic explanation I highlight and the D-N type accounts. He takes me to be providing a "friendly amendment to the D-N account." He cites Hempel, regarding the D-N model: "given this notion of explaining a particular occurrence of a solar eclipse or of a rainbow, etc., one can speak *derivatively* of a theoretical explanation of solar eclipses or rainbows in general: such an explanation is then one that accounts for any instance of an eclipse or rainbow." [7, p. 423] Belot continues:

> Clearly constructing an explanation of the rainbow in general ought not to require providing a D-N explanation for each occurrence of the rainbow—one cannot consider infinitely many sets of possible initial and boundary conditions, and show that each leads to a rainbow. So charity would appear to require us to read Hempel here as (perhaps imperfectly) perceiving the necessity of [requiring some account of stability of the phenomenon under physically realistic perturbations]. [5, p. 18]

In the book and in an earlier article [1] I emphasized a distinction between kinds of explanatory why-questions. On the one hand, there are what I called "type (i) questions." These ask for the explanation of why a given instance of a pattern obtained. Type (ii) why-questions, on the other hand, ask why patterns of a given type can be expected to obtain. They are, in effect, question about the existence of universal behavior. I argued that it is far from obvious how D-N based models are capable of answering type (ii) why-questions. In the present context, this distinction just is the distinction between explaining each occurrence of the rainbow (type (i)) and explaining the rainbow in general (type (ii)).

The question now concerns how to understand Hempel's idea that we may "speak *derivatively* of a theoretical explanation of solar eclipses or rainbows in general." The D-N model provides an account whereby we can explain the particular occurrence of a solar eclipse or of a rainbow. It is unclear how such an account (or even a collection of such accounts) can allow us to speak "derivatively" of a general account that would answer the corresponding type (ii) question. The point here, as I have emphasized in the book, is that each such D-N account of a particular occurrence will be remarkably different from all of the others. As Belot himself notes, each account will involve different initial and boundary conditions—different shapes of the raindrops, for example. Had we an explanation that answers the type (ii) question, then it seems that, yes, we may very well speak "derivatively" of explaining any given instance. We will have an account that tells us why many/most of those individual details can be ignored. That is just what asymptotic explanation, as I have presented it, provides. Hempel and Belot, it appears, get things backwards. The individual accounts—answers to type (i) why-questions—are (in part anyway) dependent upon there being an answer to the type (ii) question.[1]

I think that is fair to say that asymptotic explanation as I have presented it is, at the very least, quite different from standard philosophical accounts. Furthermore, it seems fair to say that Belot endorses the idea that explanation often does involve asymptotic analysis and that, in particular, such analysis is often involved in the demonstration of important stability results required to understand universal behavior. As a consequence, I believe he ought to be convinced that we really are discussing and employing a philosophically new (or at least widely ignored) approach to explanation. The question now concerns how radically different this approach really is. How are we to understand the kind of asymptotic decoding of information contained in the fundamental theory? Are we in any way forced to say that despite this "containment" the fundamental theory is explanatorily inadequate?

2.2 Explanatory Inadequacy of Fundamental Theory

As Belot notes, the cases I consider in the book are cases in which a certain partial differential equation depending upon some parameter needs to be investigated in an asymptotic limit in which that parameter approaches a limiting value (0 or ∞). This investigation proceeds via an auxiliary problem relating a set of curves in the cotangent bundle T^*X of the manifold X upon which the original equation is defined. These curves are the integral curves of a vector field generated by the so-called "principal symbol" of the original partial differential equation. One is interested in the projection of these curves (bicharacteristic strips) in T^*X down onto the manifold X. Such projections yield so-called "bicharacteristic curves" and we primarily care about various lines of singularities that appear in the projection—intersections of bicharacteristic curves in X projected down from the bicharacteristic strips

[1] I say "in part" here because I do not want to underestimate the value of a theory's being able to answer questions about why a particular instance of a general pattern obtains. Rather, such an account is usually not the complete story of the phenomenon, as it seems that relevant explanatory information is provided by being told that the instance is an instance of a general pattern. See [4, Chapter 3].

inhabiting the appropriate Lagrangian manifold in T^*X.

Belot is completely correct that all of this is purely mathematical—in fact, it is the precise description of the apparatus involved in the asymptotic decoding of a partial differential equation depending upon some parameter.[2] So if we set a mathematician, with no knowledge of the physical interpretation of the partial differential equation, the task of performing this asymptotic investigation, she will be able to construct a function characterizing the asymptotic approximate solutions to the original equation. She will even be able to show how such a solution is relatively stable under perturbations of certain kinds—that is, she will be able to show that the structure of the corresponding bicharacteristic curves on X will be diffeomorphically related to each other under such perturbations.

But so what? Well, Belot argues that if we now tell the mathematician that she has been investigating the wave equation and that she has "derived" Airy's integral, she will see that she has explained the fringe spacings and intensities of rainbows. This requires that we

> (i) . . . impart to her the standard sense of "the intensity of light"; (ii) . . . explain why the given initial and boundary conditions correspond to a situation in which a cloud of spherical water droplets is illuminated by white light; and (iii) . . . explain why the perturbations studied correspond to changes in the shape of the drops. [5, p. 25]

Furthermore, "none of this would appear to require reference to the concepts of geometrical optics [or ray theory]." [5, p. 25] Belot thinks this last claim is particularly obvious with respect to (i) and (iii). Let's consider (iii).

What is the relationship between the perturbation in the shape of a macroscopic object like a raindrop say of radius r and the stability of patterns of intensity of light in the asymptotic limit of the wave equation as $(\lambda/r) \to 0$?[3] One sees that when (λ/r) approaches zero, the contributions to the integral describing the intensity of light become concentrated near certain

[2] At least *his* discussion provides the precise description. I, of course, am being sloppy and leaving out many details.

[3] λ is the wavelength of the light—the parameter upon which the partial differential equation (the wave equation) depends. Physically, one is investigating the regime in which λ is small compared with the other lengths that figure in the problem, such as the radius of the drop. This is important, otherwise the results of the limiting process with depend upon a particular set of units chosen for the wavelength.

lines (these are the bicharacteristic curves) that exit the raindrop at certain angles. (This is what the principle of stationary phase tells us.) Now some families of these lines of concentrated intensity intersect to form an envelope (or curve) outside the raindrop. How do we fulfill (iii)? We must assert that changing the shape of the raindrop (in effect, letting r vary in certain ways) doesn't effect the general "shape" of the envelope of the family of lines.

But now I *am* speaking the language of geometrical optics. The lines of concentrated intensity leaving the raindrop are the rays and the envelope of a family of such lines is a caustic. If I am going to say how the perturbation of the shape of a macroscopic object, characterized by variations in r, figures in the stability of the fringe spacings and intensities, then it seems I need to refer to the relationship between that macroscopic object and the lines of concentrated intensity exiting the object as its shape is altered.

Furthermore, note that the investigation of the shortwave limit of the wave equation presents an interesting kind of problem. When $(\lambda/r) = 0$ the wave equation blows up. That is to say that the limiting behavior as $(\lambda/r) \to 0$ is qualitatively distinct from the behavior at the limit, when $(\lambda/r) = 0$. What is the motivation for studying this singular limit? One way to think about this is to note that we are investigating a kind of boundary problem—we are looking at a place where the fundamental theory breaks down completely. One attitude toward this might be to say that the fact that the law breaks down at this boundary (in this limit) just shows that the boundary is unimportant to the physics. After all the mathematics of the fundamental theory is incapable of saying what goes on *at* the limit—the wave equation makes no sense when there are no waves!

But boundary problems such as this crop up all over the place in physics. One example, discussed by Mark Wilson [9] concerns the development of shocks. Suppose we are interested in understanding the behavior of a gas as it moves through a tube. If a collection of the molecules are given a push (say by blowing into the tube at one end), then they will begin to catch up to those in front resulting in a more densely populated region separating two regions of relatively less molecular density. Across this region, molecules will exchange momentum with one another as if "some kind of permeable membrane were present." The region occupied by this "membrane" is a shock. Of course it is far too complicated to track the behavior of the individual molecules as they move through the tube and undergo the collisions in the shock region. And, once again, since we are interested in recurring phenomena, the individual (molecular) details of the development of any one shock will be quite different from the details of the development of another. Those details will, in other words be largely irrelevant to the pattern being investigated. As a consequence (familiar story!) one approaches the problem asymptotically by taking a continuum limit. We model the collection of molecules in the tube as a continuous fluid. Such a limit will shrink the shock region onto a two dimensional boundary. Sufficiently far from either side of the boundary the behavior of the fluid will be governed by the relevant partial differential equations of fluid mechanics. However, the behavior across the boundary is not governed by any differential equation at all. The fluid mechanical equations blow up.

The shock boundary represents a situation in which the theory fails to describe what is going on. Nevertheless, the boundary is a dominant feature of the phenomenon studied that, far from being unimportant, is crucial for scientific understanding. As Wilson puts it, "the allegedly 'suppressed details' have become crushed into a singular (hence not law-governed) factor that still *dominates* the overall behavior through the way in which it constrains the manner in which the 'law governed regions' piece together."[9]

The mathematical problem here is analogous to the problem of investigating the rainbow from the point of view of the asymptotics of the wave theory. The blow up at zero wavelength represents a boundary upon which the fundamental theory itself fails. Yet that boundary dominates the phenomenon—the behavior of the rainbow. In saying that the fundamental theory is explanatorily inadequate, I mean to be saying that we must take this boundary upon which the fundamental theory fails into consideration in our explanation of what is going on.

I suspect that one intuition behind Belot's (and Redhead's) objection, is that it looks like I am now saying that for genuine explanation we need to appeal *essentially* to an idealization. In this case it is the idealization of zero wavelength or equivalently, the talk of rays. But, of course, such an idealization is, as are all idealizations, strictly speaking false. And, furthermore, in speaking of this idealization as essential for explanation, they take me (as noted above) to be reifying the rays. *It is this last claim only that I reject.* I believe that in many instances our explanatory physical practice demands that we appeal essentially, to (infinite) idealizations.[4] But I do not believe that this involves the reification of the idealized structures.

Let me consider briefly one such situation referred to by Redhead in the

[4]See [3] for one such discussion.

passage cited above. This concerns the mathematical apparatus of gauge theories. Take the case of the Aharanov-Bohm effect.[5] Here we see that an electron looping around a solenoid containing a magnetic field (inside) and zero magnetic field outside, picks up a phase proportional to the magnetic flux through the solenoid. (This is *prima facie* odd as there is apparently no "causal and local" interaction of the magnetic field upon the electron.) One way of understanding this is to *idealize* the solenoid to be infinitely thin and infinitely extended. This allows one to think of the space or spacetime in which the electron evolves as being topologically nonsimply connected—space or spacetime with a mathematical line removed from it.[6] This idealization allows us to *explain* certain aspects of the the Aharonov-Bohm effect by reference to *topological* properties of this idealized space or spacetime. For instance, we can explain why it is that an electron that loops twice around the solenoid will pick up twice the "geometric phase" as one that loops around only once. Now, one might think that we should not appeal to such an idealization in our explanation of the physical phenomenon observed in Aharonov-Bohm experiments. Perhaps, we should appeal to "real" properties of loops in space or spacetime, thereby restoring a causal (if not local) explanatory account.[7]

In arguing that an account that appeals to the mathematical idealization is explanatorily superior to a theory that does not invoke the idealization, I am not reifying the mathematics. (I therefore, agree Redhead, that the mathematics with "arguably no physical reference is used to explain or 'control' what is going on ...." [8]) Nevertheless, I am claiming that the "fundamental" theory that fails to take seriously the idealized "boundary" is less explanatorily adequate.

[5]See [2] for an extended discussion.

[6]A space is nonsimply connected if there exist loops or paths in the space that cannot be contracted to a point. Thus, any loop around the "solenoid"—that is, around the line removed from the space—cannot be contracted to a point.

[7]This is Healey's [6] position. See [2] for my response.

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