How to Avoid Inconsistent Idealizations

Christopher Pincock (pincock@purdue.edu)

October 30, 2009
6882 words
DRAFT

Abstract

Idealized scientific representations result from employing assumptions that we take to be false. It is not surprising, then, that idealizations are a prime example of allegedly inconsistent scientific representations. I argue that the claim that an idealization requires inconsistent beliefs is often incorrect and that it turns out that a more mathematical perspective allows us to understand how the idealization can be interpreted consistently. The main example discussed is the claim that models of ocean waves typically involve the false assumption that the ocean is infinitely deep. While it is true that the variable associated with depth is often taken to infinity in the representation of ocean waves, I explain how this mathematical transformation of the original equations does not require the belief that the ocean being modeled is infinitely deep. More generally, as a mathematical representation is manipulated, its components are decoupled from their original physical interpretation.

I. For some time Maddy has claimed that scientific textbooks support her claim that "we assume the ocean to be infinitely deep when we analyze the waves on its surface" (Maddy 1998, p. 143). Her immediate goal with this claim was to undermine the indispensability argument for platonism about mathematics. The idea is roughly that if such assumptions are part of our scientific representations, and we fail to accept their truth despite their indispensability, then the same position is also viable for the mathematical assumptions that are part of these representations. In the 1992 paper "Indispensability and Practice" she notes that there are "merely useful elements" in our scientific representations which scientists do not believe in even though they are indispensable. She continues,

> But perhaps a closer look at particular theories will reveal that the actual role of the mathematics we care about always falls within the true elements rather than the merely useful elements; perhaps the indispensability arguments can be revived in this way. Alas, a glance at any freshman physics text will disappoint this notion. Its pages are littered with applications of mathematics that are expressly understood not to be literally true: e.g., the analysis of water waves by assuming the water to be infinitely deep or the treatment of matter as continuous in fluid dynamics or the representation of energy as a continuously varying quantity. Notice that this merely useful mathematics is still indispensable; without these (false) assumptions, the theory becomes unworkable (Maddy 1992, p. 281).[1]

Other philosophers have used this example to motivate other philosophical conclusions. Azzouni, for example, rejects Maddy's view that such applications do not involve a commitment to the truth. Still, he argues that these sorts of examples highlight the need for a more nuanced conception of the posits of our best theories (Azzouni 2004, p. 45 fn. 25). For Azzouni additional tests need to be met for an entity that we quantify over to be something that we genuinely are committed to. More recently, Colyvan has pushed Maddy's point even further. Noting that we also deploy sonar to determine the finite depth of the ocean, Colyvan concludes that "what we are really dealing with here is a contradiction between two pieces of theory. Taking the conjunction of the two pieces of theory, we have it that oceans are both infinitely deep and not infinitely deep" (Colyvan 2008, p. 117). He goes on to note that

> there is nothing too troubling here. The sonar theory is surely correct and the assumption of infinitely deep oceans is a mere idealisation. It is clear what we ought to believe here (and that was Maddy's point). It is just that hard-nosed Quineans, she suggests, have trouble delivering the right answer (Colyvan 2008, p. 117 fn. 4).

[1]The wave case is again noted at Maddy 2007, p. 316.

Despite these assurances, Colyvan goes on to argue that cases of inconsistency in science are common and need to be addressed directly through the development of a novel conception of scientific inference. The resources for such an account are to be found in some form of non-classical logic. It is apparently only through the use of these tools that we can arrive at the "right answer" concerning what to believe in.[2]

I believe that Maddy's original assumption is wrong. That is, we do not ever assume that the ocean is infinitely deep. Getting clear on why this is can help us deflect the philosophical conclusions of Maddy, Azzouni and Colyvan. But, more importantly, it can help us to understand how such idealizations are consistent with a limited form of scientific realism. I will focus on the idealizations necessary to understand water wave dispersion. This is the often observed phenomena in which an irregular pattern of waves gradually spreads out from the center of some disturbance of the water like a rock being dropped in an otherwise calm ocean. What we observe in this case is that waves of longer wavelength move more quickly away from the center. As a result, the wave pattern becomes more regular. As we will see, an accurate representation of this phenomenon depends on two different idealizations. First, we must deploy the "small amplitude" idealization which allows us to move from the nonlinear Navier-Stokes equations for incompressible, inviscid fluids to a set of linear equations. Second, we use the "deep-water" idealization which allows us to represent the relationship between the phase velocity of a wave and its wavelength. In a prominent fluid mechanics textbook, Kundu and Cohen's *Fluid Mechanics* (Kundu & Cohen 2008), we find that both steps are treated as *approximations*, i.e. assumptions that will not introduce significant errors into the representation. However a more mathematically sophisticated perspective, offered by Segel's *Mathematics Applied to Continuum Mechanics* (Segel 2007), lets us see that these assumptions actually correspond to applications of a mathematical theory known as perturbation theory. As he writes elsewhere, with Lin, "perturbation theory is often used implicitly when we formulate simplified physical models" (Lin & Segel 1988, p. 48). The difference between the two presentations corresponds to the priorities of the two textbooks. While Kundu and Cohen need only to assure their readers that these assumptions are appropriate in this particular case, Segel aims to explain in general when assumptions of this type are appropriate. It is the latter goal that also contributes to an understanding

[2] See also Colyvan 2009.

of these idealizations. For they show how apparently false assumptions can be used to reveal important features of real systems. On the picture of idealization that I will develop, an idealization transforms a representation which only obscurely represents a feature of interest into one that represents that same feature with more prominence and clarity. This involves, among other things, decoupling parts of the representation from their original interpretation. In my conclusion I will explore the issue of how widely this approach to idealization can be extended so as to avoid inconsistency in our best science.

II. Our best representation of fluids like the ocean treat it as a continuous medium subject to various internal stresses and external forces. The key magnitudes that we try to capture are the velocity vectors at a point at a time and the pressure magnitude at a point at a time. In our simplified case, we treat the density of the fluid as a constant and assume that the only force at work is gravity, with a constant acceleration downwards on a unit mass given by g. Therefore here we ignore the viscosity or friction due to the interaction of the fluid elements as well as the surface tension of the ocean at the surface boundary with the air. In these circumstances there is a scalar field, or potential, ϕ defined on the fluid elements such that the velocity v at a point is given by $\nabla\phi$. We treat the two-dimensional case, so this gives the x and z components of velocity as u and w, respectively:

$$u = \frac{\partial}{\partial x}\phi \, , \; w = \frac{\partial}{\partial z}\phi \tag{1}$$

We set $z = 0$ to be the surface of an undisturbed ocean system, H as the depth of the ocean, a as the maximum amplitude of the ocean wave and λ as the wavelength of the wave, i.e. the length between one crest and the next crest. The shape of the wave is given by the displacement $\eta(x,t)$ from the undisturbed case. See figure 1.

The first step in developing our representation is to move from the Navier-Stokes equations for such fluids (incompressible, inviscid) to the results of deploying the small amplitude wave idealization. This is how Kundu and Cohen motivate the idealization:

> We shall assume that the amplitude a of oscillation of the free surface is small, in the sense that both a/λ and a/H are much smaller than one. The condition $a/\lambda \ll 1$ implies that the slope of the sea surface is small, and the condition $a/H \ll 1$ implies that the instantaneous depth does not differ significantly from

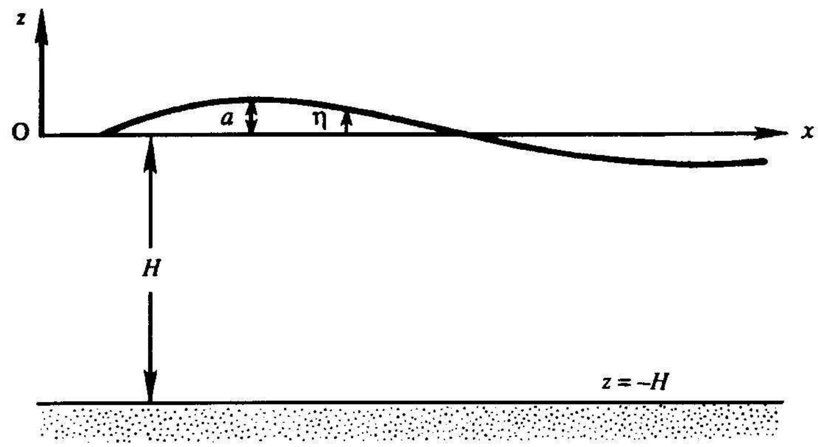

Figure 1: Surface Waves (Kundu & Cohen 2008, p. 219)

the undisturbed depth. These conditions allow us to linearize the problem (Kundu & Cohen 2008, p. 219).

The Navier-Stokes equations are nonlinear in the sense that they have terms like $u\frac{\partial u}{\partial x}$ that complicate their solution. In particular, we cannot use a principle of superposition to the effect that any two solutions can be combined by linear combinations to yield new solutions. Considering only waves of small amplitude allows us transform the problem into the solution of the Laplace equation for ϕ:

$$\frac{\partial^2 \phi}{\partial x^2} + \frac{\partial^2 \phi}{\partial z^2} = 0 \tag{2}$$

subject to the three conditions

$$\frac{\partial \phi}{\partial z} = 0 \text{ at } z = -H \tag{3}$$

$$\frac{\partial \phi}{\partial z} = \frac{\partial \eta}{\partial t} \text{ at } z = 0 \tag{4}$$

$$\frac{\partial \phi}{\partial t} = -g\eta \text{ at } z = 0 \tag{5}$$

The first two conditions are kinematic because they rely on the features of the boundaries, here the bottom of the ocean and the ocean surface. The third condition is dynamic because it turns on the interaction between the pressure and velocity of a fluid element at the surface. Crucially, the latter two equations are evaluated at constant $z = 0$ and not at the varying surface of the water. Again, this simplification is only possible because of the small amplitude idealization.[3]

We aim for a solution to η of the form

$$\eta = a\cos(kx - \omega t) \tag{6}$$

The key features of a wave are its amplitude a, the displacement at a point, the wavelength λ and the *period* T, the time it takes for a given phase of the wave to repeat itself at a given point or, equivalently, the time it takes for the wave to travel one wavelength. However, for convenience we also use k, the *wavenumber* equal to $2\pi/\lambda$ as well as ω, the *circular frequency* equal to $2\pi/T$. We can determine c, the *phase speed*, or speed of propagation of a part of the wave with a constant phase, like a wave crest, by $\frac{\omega}{k}$. Deploying our kinematic boundary conditions yields the general solution

$$\phi = \frac{a\omega}{k}\frac{\cosh k(z+H)}{\sinh kH}\sin(kx - \omega t) \tag{7}$$

which determines the velocity distribution throughout the fluid. We can also use it, along with (6) and (5), to find c in terms of λ and H:

$$c = \sqrt{\frac{g\lambda}{2\pi}\tanh\frac{2\pi H}{\lambda}} \tag{8}$$

This reveals a complicated relationship between the phase velocity of a water wave, the wavelength and the depth of the medium. Some care is needed to see exactly what its significance is for our case of a rock dropped in an otherwise calm ocean.

We first consider the case where $H/\lambda \gg 1$, that is, the depth of the ocean is much greater than the wavelength. Consider, for example, the case where $H > .28\lambda$. Then $2\pi H/\lambda > 1.75$ and $\tanh(2\pi H/\lambda) > .94138$. More generally, as $x \to \infty$, $\tanh x \to 1$. As the square root of .94138 is .97, replacing $\tanh\frac{2\pi H}{\lambda}$ by 1 will produce a maximum error of 3% when $H > .28\lambda$. Using this calculation, Kundu and Cohen note that "Waves are therefore classified

[3]In my reconstruction I am ignoring the subtleties needed to find the pressure throughout the fluid.

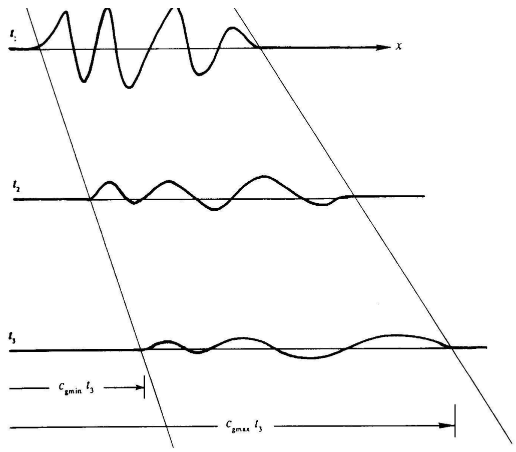

Figure 2: Wave Dispersion (Kundu & Cohen 2008, p. 243).

as *deep-water waves* if the depth is more than 28% of the wavelength" (Kundu & Cohen 2008, p. 230). Deep-water waves include ocean waves generated by wind, where the typical wavelength is around 150 m, while the ocean depth ranges from around 100 m to around 4 km. They can thus always be treated by

$$c = \sqrt{\frac{g\lambda}{2\pi}} \tag{9}$$

Among other things, this allows us to understand the phenomenon of water wave dispersion noted earlier. See figure 2. Based on (9) we can see that the crests of waves with longer wavelengths will travel faster than the crests of waves with shorter wavelengths. If we treat the initial disturbance at t_1 as a more or less random superposition of deep-water waves, then waves of varying wavelengths will become dispersed or spatially separated. In addition, the leading waves will gradually decrease in amplitude because of the conservation of energy for the whole disturbance.[4] This result does not follow for (8) generally as can be seen by taking the other extreme case of shallow-water waves where $H < .07\lambda$ and

$$c = \sqrt{gH} \tag{10}$$

This equation results from using the fact that $\tanh x \to x$ as $x \to 0$, so

[4]There are other features of water wave dispersion that I am ignoring here for simplicity of presentation.

$\tanh \frac{2\pi H}{\lambda}$ is replaced by $\frac{2\pi H}{\lambda}$. Here the phase velocity is independent of the wavelength, and so all wave crests travel at the same velocity. In such cases, an initial disturbance retains its irregular shape because the waves of differing wavelengths fail to disperse.[5]

Notice that (9) also results from (8) from thinking of $H \to \infty$. So, we can treat the deep-water wave representation as corresponding to the idealization that the ocean is infinitely deep. This sort of assumption could also be used to justify part of our small amplitude idealization that $a/H \ll 1$. And this is, in fact, how Kundu and Cohen sometimes speak, as when they treat "Waves at a Density Interface between Infinitely Deep Fluids" (Kundu & Cohen 2008, p. 255). It is tempting to say that it is only at the limit where $H = \infty$ where $\tanh \frac{2\pi H}{\lambda} = 1$ and the equality $c = \sqrt{\frac{g\lambda}{2\pi}}$ holds. If we take this perspective, then it looks like the false assumption that the ocean is infinitely deep is revealing something important about the behavior of real ocean waves. This can create various interpretative puzzles. One puzzle is that it looks like an agent that deploys the deep-water idealization has inconsistent beliefs. For, in the idealization, she makes the assumption that $H = \infty$. But when pressed for her belief about the ocean depth, she will of course concede that the ocean has some finite depth. Indeed, this belief seems required for any subject who thinks coherently about the ocean. An ocean of infinite depth is about as physically impossible as it gets. So, we appear to have a kind of deep or irreconcilable inconsistency at the heart of a successful scientific practice.

I think the most fruitful approach to such cases is to question this starting point. We need an account of idealization which allows that a false assumption relating to the depth of the ocean is not part of the content of the representation itself, but is only the means to obtain the representation. The basic idea of this approach to idealization is that false assumptions take us from an interpreted part of a scientific representation to an idealized representation where this particular part is no longer interpreted. In particular, when we make the false claim that the ocean is infinitely deep, what we are

[5] A somewhat surprising fact is that it is actually the shallow-water idealization that is used to understand the ocean in geophysical applications. See, for example, Kundu & Cohen 2008, ch. 14. This is because there is a natural boundary in the ocean known as the thermocline. It is the region of greatest temperature gradient as we travel downwards. The bottom of the thermocline can then be treated as the lower boundary of a relatively thin layer of fluid which affects the long wavelength waves relevant to geophysics.

really doing is shifting to a representation where H is decoupled from its former interpretation. This means that an agent can affirm the whole genuine content of the resulting representation, in this case the deep-water idealization. It also helps to explain why this representation is only appropriate for certain purposes. Our idealization has literally nothing to say about what goes on in the bottom of the ocean, so if we are interested in these aspects of the situation, then we need to shift to a different idealization.

This sort of proposal finds a natural place in the account of the content of a representation which emphasizes the structural relationship between a target system and the mathematical domain used to represent that system. When we deploy an idealization like $H = \infty$, what we are doing is transforming the representation so that it has merely a schematic content. The H no longer represents the depth of the ocean and so our representation can be accurate no matter what the depth actually is. The genuine content is obtained by filling out the schematic content so that the appropriate scope of application is clarified. That is, which target systems is the representation meant to accurately represent? This can be one of the trickiest aspects of scientific modeling. In this case, though, the informal argument that waves where $H > .28\lambda$ obey (9) gives us a good start on pinning down this genuine content. Using this argument to fix the scope of the representation produces a definite representation that can be experimentally tested with respect to the relevant kind of target system.

The characterization of deep-water waves is fairly ad hoc, and it would be better if there was some general theory which would allow us to clarify the scope of these highly idealized representations. This is where the perturbation theory mentioned by Lin and Segel becomes useful. While this is a large area of ongoing research in applied mathematics, we can start to understand the importance of the theory by considering how it works in our water wave case. I will discuss perturbation theory in two stages. In our first stage we will treat the theory in terms of the scale of the features of the system. In the next stage, discussed in the next section of this paper, we will see how this talk can be grounded more adequately in terms of series. For now, our goal is to use the parameters given in setting up the problem, such as the typical wave amplitude a and depth H, to form dimensionless variables. In our case, for example, $x*$ is a length variable, so a new dimensionless length variable x results from dividing all occurrences of $x*$ by a or by H. I will adopt the convention for this discussion that the original variables are labeled with a star while the dimensionless variables are without stars. If we carry this scaling through for each variable, then we can re-scale the equations by substituting the new variables in for the old variables while keeping track of the parameters. The goal is to do this in such a way that "each term is preceded by a combination of parameters that explicitly reveals the term's magnitudes" (Segel 2007, p. 329) for the domain in question. If this procedure is successful, then I will say that the set of scales is *adequate*. The existence of an adequate set of scales is not a purely mathematical claim, but is rather a claim about the features of the given physical system and their relative magnitudes. In those cases where we obtain such an adequate set of scales, then the terms preceded by combinations of parameters that are orders of magnitude smaller than other terms can be safely ignored. This corresponds to the claim that the features tracked by these parts of the equation are irrelevant for our given representational purpose. From this perspective it should not be surprising that a set of scales is adequate only with respect to a given context. For in another context where we are trying to accurately capture other aspects of the system a different array of features may be important and so a different set of scales is needed.

Our two idealizations can be understood, then, as the application of two sets of scales that turn out to be adequate for certain water wave systems. In this case, the second set of scales results from adjusting the first set, so the two idealizations can be combined. Consider, first, the small amplitude assumption and the elimination of problematic nonlinear terms like $u\frac{\partial u}{\partial x}$. On our scaling approach, what we are doing is not accurately described as assuming that the contribution of this term is negligible or unlikely to produce a significant experimental error. Rather, we are assuming that *relative to the linear terms* like $\frac{\partial u}{\partial t}$ the contribution of $u\frac{\partial u}{\partial x}$ is negligible. Consider the set of scales in terms of the amplitude a, period T and wavelength λ (Segel 2007, p. 325):

$$t = \frac{t*}{T}\ ,\ x = \frac{x*}{\lambda}\ ,\ u = \frac{u*}{a/T} \qquad (11)$$

Notice that there is a choice in scaling x. We employ the distance λ, but we could also have used any other distance given in the problem, e.g. a. In making the choice we do, we are claiming that the relevant processes operate in the x spatial direction only on the order of λ. Similarly, we are claiming that the velocity in the x-direction of a fluid particle, u, is similar in magnitude to the ratio between the amplitude and the period of the waves in question. Accepting these scales, ordinary algebra entails that

$$t* = Tt \, , \; x* = \lambda x \, , \; u* = \frac{a}{T} u \tag{12}$$

Making the substitution into the term $u * \frac{\partial u*}{\partial x*}$ yields

$$\frac{a}{T} u \frac{\partial (\frac{a}{T} u)}{\partial (\lambda x)} = \frac{a^2}{T^2 \lambda} u \frac{\partial u}{\partial x} \tag{13}$$

Similarly, making the substitution into $\frac{\partial u*}{\partial t*}$ gives

$$\frac{\partial (\frac{a}{T} u)}{\partial (Tt)} = \frac{a}{T^2} \frac{\partial u}{\partial t} \tag{14}$$

The ratio between the nonlinear term and the linear term is then given by

$$\frac{\frac{a^2}{T^2 \lambda}}{\frac{a}{T^2}} = \frac{a}{\lambda} \tag{15}$$

So, if this set of scales is adequate, then the magnitude of the nonlinear term is negligible when compared to the magnitude of the linear term on the assumption that $\frac{a}{\lambda}$ is much less than one.

To complete the set of scales sufficient to produce the linear equations we also need to specify what happens for the other variables z, w, p and η. Our focus is on the status of the depth H. The only place where it comes in is with the boundary condition (3): "At $z = -H$, $\frac{\partial \phi}{\partial z} = 0$". As with the scale $x = \frac{x*}{\lambda}$, we adopt $z = \frac{z*}{\lambda}$. Repeating the sort of algebraic substitution given above, (3) becomes "At $z = -h$, $\frac{\partial \phi}{\partial z} = 0$". Here $h = \frac{H}{\lambda}$. To shift to the deep-water case, Segel considers what happens as $h \to \infty$ (Segel 2007, p. 335). This transforms the above boundary condition into "As $z \to -\infty$, $\frac{\partial \phi}{\partial z} \to 0$". Notice that this is a claim involving the dimensionless variables. What we are saying is that terms preceded by h become orders of magnitude more important or, equivalently, that terms preceded by $1/h = \lambda/H$ become orders of magnitude less important. When h appears in the boundary conditions, this has the effect of moving the boundary as far from the system as possible. Thus for this set of scales to be adequate boundary effects must be irrelevant to what we aim to represent.

To the see the effects of this adjusted set of scales, consider what happens if we repeat the derivation of (7). We find that ϕ takes on the simplified form

$$\phi = \frac{a\omega}{k} \sin(kx - \omega t) \tag{16}$$

Using (16) instead of (7) when we deploy (6) and (5) gives us (9) immediately. So, if this adjusted set of scales is adequate, then the waves obey this simple relation between phase velocity and wavelength.[6]

The upshot of this analysis of idealization is that we can replace the vague notion of an approximately true claim with the claim that for phenomenon p some set of scales s is adequate. In our case the phenomenon that we are trying to capture is wave dispersion. This can be experimentally observed, but there is no general requirement that p be observable. When it is, however, we can directly specify what we are trying to understand with our representation. Starting with equations that we take to represent the system fairly realistically, we might be frustrated in our attempts to capture p. This can prompt an investigation of one or more set of scales to see if transforming the equations using s reveals anything of relevance to p. In our case, the main feature of wave dispersion comes out directly upon a particular choice of scales. Additional tests are needed to see if the representation does indeed accurately capture wave dispersion. On the assumption that it does, we have evidence that we have found an adequate set of scales.

What we see, then, is a kind of tradeoff between the completeness of a representation and its ability to accurately represent some phenomenon of interest. If we remain with the nonlinear Navier-Stokes equations or even with (8), then it is impossible for us to represent the observed pattern of dependence between wavelength and phase velocity. This is not just a point about mathematical tractability, although our inability to solve a system of equations analytically or numerically is a symptom of the problem I am alluding to. The problem is basically that a complete representation of a complex system will include countless details and these details typically obscure what we have selected as the important features of the system. By giving up completeness, and opting for a set of scales, we shift to a partial representation of the system that aims to capture features that are manifest in that scale. This partiality gains us a perspicuous and accurate representation as well as an understanding of features that would otherwise elude us.

III. The claim that a set of scales is adequate can be made more precise using concepts drawn from perturbation theory. This theory develops a network of interrelated techniques for solving mathematical problems that prove difficult to solve by more traditional means. As a textbook in this area summarizes things, "a perturbation procedure consists in constructing the solution for

[6] The interpretative issues are discussed briefly at Segel 2007, pp. 340-341.

a problem involving a small parameter ϵ, either in the differential equation or the boundary conditions or both, when the solution for the limiting case $\epsilon = 0$ is known" (Kevorkian & Cole 1981, p. v). We can divide this procedure into three steps: (i) isolating the small parameter ϵ and recasting the problem in these terms, (ii) considering a sequence called an asymptotic expansion our of unknown function that results from the first step and (iii) calculating the solution to the original problem using the sequence from the second step. In the simplest case, we are trying to find an $f(x)$ that satisfies some differential equations and boundary conditions. The approach to this case can be generalized to several functions with more than one independent variable. Given some ϵ that we take to be small for the domain in question, we first recast f as a function of both x and ϵ. The f we are after is then thought of as $f(x, \epsilon)$ where ϵ has some particular value, e.g. close to 0.

The next step is to find what is called an asymptotic expansion of $f(x, \epsilon)$ to N terms. To clarify this we need to introduce the notion of the order of a function. Consider two functions $\phi(x, \epsilon)$ and $\psi(x, \epsilon)$, where $\psi \neq 0$ for the domain in question. Suppose x is fixed and that as $\epsilon \to 0$, $(\phi/\psi) \to 0$. Then we say that $\phi = o(\psi)$ or "ϕ is of order ψ" or $\phi \ll \psi$. Using this terminology, we can posit a sequence of functions where as we go along in the sequence, the next function is of the order of the previous function. That is, we have $\phi_n(\epsilon)$ such that

$$\phi_{n+1}(\epsilon) = o(\phi_n(\epsilon)) \text{ as } \epsilon \to 0 \tag{17}$$

A standard example when $\epsilon < 1$ is ϵ^n: $1, \epsilon, \epsilon^2, \epsilon^3, \ldots$ (Kevorkian & Cole 1981, p. 3). Using these sequences, we then aim to isolate an asymptotic expansion of f. This sequence must satisfy

$$f(x, \epsilon) - \Sigma_{n=1}^{M} a_n(x)\phi_n(\epsilon) = o(\phi_M) \text{ as } \epsilon \to 0 \tag{18}$$

for each $M = 1, \ldots, N$ (Kevorkian & Cole 1981, p. 3). The crucial feature of such an expansion is that the difference between our unknown f and the sum goes down quickly as we consider larger N.

To use the asymptotic expansion to solve the original problem we first find f when $\epsilon = 0$. Then we use the features of the asymptotic expansion, the differential equations and boundary conditions given in the original problem to fix the a_1 term. Based on this, we can calculate a first-order correction to f for $\epsilon = 0$. Additional corrections can be made by calculating a_2 or later terms. However, often no additional calculations are needed beyond the first and second terms. This is because we know that whatever error remains will be of order ϵ^3 and the assumption that ϵ is small tells us that this error will be very small.

In our wave dispersion case, we solved the problem after dropping the nonlinear term preceded by a/λ. Let $\epsilon = a/\lambda$. A perturbation theory approach allows us to consider an asymptotic expansion of our unknown function $u(x,t)$. Now, though, we consider it as $u(x,t,\epsilon)$. In the case where $\epsilon = 0$ we have the linear case that we can solve directly. Then this solution can be used as the input to the perturbation procedure just outlined. We can then calculate the first or even second order corrections. This will involve some adjustments to the original (9) obtained above. However, we can be confident that if there is an asymptotic expansion of u that is valid for our entire domain, then these corrections will be tracked by the orders of magnitude of ϵ and ϵ^2. As we consider systems where ϵ gets smaller and smaller, we can see that these corrections will quickly pass below the level of experimental detection.

The point of this brief discussion of asymptotic expansions was to clarify what it means to say that a set of scales is adequate. The main advance that has been made is to go beyond the claim that a set of scales is adequate just in case it results in a set of equations where "each term is preceded by a combination of parameters that explicitly reveals the term's magnitudes" (Segel 2007, p. 329) for the domain in question. The cash-value of the notion of magnitude here is that carrying out the above perturbation procedure in terms of an asymptotic expansion for our unknown functions will deliver an answer to any desired degree of accuracy.

IV. In emphasizing the importance of asymptotic reasoning and perturbation theory for understanding our best scientific representations I am of course following very closely the work of Batterman in such places as *The Devil in the Details* (Batterman 2002) and the more recent paper "On the Explanatory Role of Mathematics in Empirical Science" (Batterman 2009). But Batterman has emphasized a distinction between two different kinds of perturbation problems. These are regular cases and singular cases. A *regular* perturbation problem can be roughly characterized as a case where the solution of the original equations can be recovered by starting with the solution to the $\epsilon = 0$ case and adding corrections using an asymptotic expansion. The water wave dispersion case is of this type for reasons outlined in section II. Unfortunately, the conditions of application for the regular perturbation theory are quite stringent. Problems arise when the $\epsilon = 0$ case is "different in qualitative character" (Lin & Segel 1988, p. 279) than the case when $\epsilon \neq 0$.

An equation that can be used to illustrate this kind of qualitative shift is

$$\epsilon m^2 + 2m + 1 = 0 \tag{19}$$

When $\epsilon = 0$, $2m + 1 = 0$, so $m = -\frac{1}{2}$. But if $\epsilon \neq 0$, then the equation has two roots and we cannot recover them by starting with the $\epsilon = 0$ case.[7] A symptom of this sort of failure of the regular perturbation approach is that the asymptotic expansion fails to remain valid for the entire domain under consideration. This can happen when the $\epsilon = 0$ case is not a limit of the small ϵ cases for some part of the domain. When this happens, we must shift to singular perturbation theory and try to develop an associated multi-scale representation. For example, in the boundary layer treatment of fluid flow around an object like an airplane wing we use one set of scales for the region near the wing and a different set of scales for the region beyond the boundary region.[8] These sorts of techniques have a much wider domain of application, although a cost here is that the interpretative significance of a successful treatment is often much less clear.

Batterman seems most concerned with the explanatory consequences of dealing with a singular perturbation case: "If the limits are not regular, then they yield various types of divergences and singularities for which there are *no* physical analogs. Nevertheless . . . these singularities are essential for genuine explanation" (Batterman 2009, p. 19). One case which Batterman emphasizes is the explanation for "why rainbows always appear with the same bow structure – the same pattern and spacings of the colored and dark bands" (Batterman 2009, p. 20). When we investigate the representations which are used to derive this pattern, we find that scientists employ the ray representation of light as opposed to the more realistic representation of light as an electromagnetic wave.[9] The ray representation results from the wave representation when we consider the case where the wavelength goes to 0. Singularities obtain, though, in features of the derived ray representation which include the intensity of the light which is perceived as a rainbow. Still, Batterman insists that

[7] Indeed, determining the roots directly, say using the quadratic formula, yields values for m in terms of ϵ that are undefined when $\epsilon = 0$.

[8] Singular perturbation theory deploys many different methods. For a discussion of boundary layer theory and the methods associated with multiple scales see Pincock 2009. For more discussion see Kevorkian & Cole 1981.

[9] See Batterman 2002, ch. 6 and Nahin 2004, §5.8.

> The asymptotic investigation of the wave equation leads to an understanding of the *stability* of those phenomena under perturbation of the shape of raindrops and other features. Stability under perturbation of details is exactly what is required for a phenomenon to be repeatable or reproducible (Batterman 2009, p. 21).

As with the water wave case, we might imagine someone who thought that the representation of the rainbow involved a kind of theoretical inconsistency. This is because we represent the rainbow using the assumption that the wavelength is 0 just as we seem to represent the ocean as infinitely deep. But of course we believe that the wavelength is not 0 just as we believe that the ocean has some finite depth. How, then, are we to avoid the charge of inconsistency?

My proposal is that even though we use the claim that the wavelength is 0 to arrive at the representation of the rainbow this claim is not part of the content of the representation of the rainbow. Instead, the claim merely permits us to arrive at a representation of an aspect of the rainbow phenomenon which interests us, namely the recurrence of the bow pattern across changes in the configuration of the rain drops and their shapes. This conception of idealization and the role of perturbation theory in understanding it seems to be shared by Batterman as he emphasizes that "what is often explanatorily essential is the mediating limiting relationship between the representative models" (Batterman 2009, p. 10).

Still, the distinction between regular and singular perturbation cases might suggest that there is some crucial difference between the wave dispersion case and the rainbow case. Maybe an account of idealization which is developed for the regular case will fail when it is extended to the singular case. On a first pass, it is hard to see why this mathematical difference would correspond to any difference in the interpretative significance of the representations which result. In both the regular and the singular cases a mathematical transformation decouples part of the representation from its original representation. In the wave dispersion case, this was the H which originally represented the depth of the ocean, while in the rainbow case it was the λ which originally represented the wavelength of light. The resulting representations then literally say nothing about depth or wavelength, respectively, and our recipe for avoiding inconsistent idealization is successful. The parallel is of course not complete as the mathematical techniques needed to solve the regular perturbation problem are considerably simpler than what is deployed in the singular perturbation cases. The basic issue is how different the $\epsilon = 0$ case is from the $\epsilon \neq 0$ cases which are our genuine focus. But this merely mathematical difference seems unrelated to how we should interpret the resulting representations when we can get the perturbation techniques to work.

A clear statement of Batterman's disagreement with this proposal comes in his reply to Belot's "Whose Devil? Which Details?" (Belot 2005). Belot responds to cases like the use of the ray representation of the rainbow by refusing to assign the relevant mathematics any novel physical interpretation:

> The mathematics of the less fundamental theory is definable in terms of that of the more fundamental theory; so the requisite mathematical results can be proved by someone whose repertoire of interpreted physical theories includes only the latter; and it is far from obvious that the physical interpretation of such results requires that the mathematics of the less fundamental theory be given a physical interpretation (Belot 2005, p. 151).

Batterman replies by arguing that Belot's "pure mathematician" needs the less fundamental theory to motivate the steps which are taken to develop the ray representation. These developments go beyond simply taking a parameter like the wavelength to 0. In particular, the initial and boundary conditions of the ray representation make sense only if we interpret them using the ray theory of light:

> those initial and boundary conditions are not devoid of physical content. They are 'theory laden'. And, the theory required to characterize them as appropriate for the rainbow problem in the first place is the theory of geometrical optics. The so-called 'pure' mathematical theory of partial differential equations is not only motivated by physical interpretation, but even more, one cannot begin to suggest the appropriate boundary conditions in a given problem without appeal to a physical interpretation. In this case, and in others, such suggestions come from an idealized limiting older (or emeritus) theory (Batterman 2005, p. 159).[10]

Notice that this response is only plausible when we have a singular case. For in a regular case there is no need to deploy a different interpretation of the

[10] See also Batterman 2002, p. 96.

parts of the representation which remain interpreted. In the wave dispersion case, for example, the increase in the depth H does not affect the physical interpretation of a, the wave amplitude. Similarly, the initial and boundary conditions besides (3) can be treated in the same way. But when the case is a singular one, then this attempt to preserve the interpretation of the remaining parts of the mathematics is frustrated. The change in "qualitative character" of the mathematics associated with the ray representation forces a severe readjustment in the interpretation of the representation. Batterman's claim is that in this singular case and in many others the readjustment only makes sense using the resources of a new scientific theory.

Notice, though, that this point about the reduction relationships between theories has no immediate connection to Batterman's other point that limiting relationships reveal what is stable "under perturbation of details". In the regular perturbation case of wave dispersion we also come to represent the system independently of any claims about the depth of the ocean. So the "detail" of the particular depth of the ocean is seen to be irrelevant. If explanatory power depends on factoring out these sorts of irrelevant details and clearly representing what is stable across these changes, then this explanatory power is available in both singular and regular cases. Batterman may be assuming that the sort of reduction that is available in the regular case deprives the reduced representation of its explanatory power. That is, the wave dispersion representation is not explanatory because there is an interpretation of it in terms of the original fluid mechanics representation. Presumably, then, it is the original fluid mechanics representation which is doing the explaining. But if this is the case, then we need a clearer statement of what the source of the explanatory power is as setting aside details which are irrelevant to the phenomenon being explained is not sufficient for explanation.

Finally, I would like to suggest that if Batterman is right concerning his claims about reductionism, then inconsistency threatens. This is because it is no longer clear that the beliefs associated with different representations of a system like a rainbow can be reconciled. The wave-theoretic representation that we start with interprets light and its features in terms of electromagnetic waves. The ray representation that we end up with interprets light and its features in terms of rays. Batterman insists that this does not require that we believe that light is both a ray and an electromagnetic wave, but exactly what it does require us to believe about the light or rainbows remains unclear. Until this situation is clarified, the advocate of inconsistent idealizations has not been totally defeated.

V. In the preceding sections I have outlined a recipe for avoiding inconsistent idealizations. This approach was developed to handle cases where regular perturbation theory is explicitly applied. But I would agree with Lin and Segel's claim that we can see perturbation theory at work on an implicit level even when it is not explicit. This attitude attempts to extend the scope of this picture of idealization quite widely. In the last section we saw the potential limits of my recipe when it is applied to cases of singular perturbation. These worries also apply to idealizations where we lack any explicit representation of what is going on in the non-idealized case. To revert to another case noted by Maddy, we often treat collections of discrete particles as if they formed a continuous substance (Maddy 1998, p. 143, Maddy 2007, p. 316). For example, fluids are often defined as something "which deforms continuously under the action of a shear force, however small" (Kundu & Cohen 2008, p. 4).[11] This might form the starting point of a fluid mechanics textbook and so it is not motivated based on any prior representation of a fluid like water. It is not clear how to apply my recipe to such cases because even though we know that the continuum representation is idealized we remain unclear about how it is idealized or what parts should be decoupled from their usual interpretation. Beyond the singular cases, then, there remain many idealizations which at least initially present the appearance of inconsistency. I would hope that future investigation would show, though, that these appearances of inconsistency can be removed through further clarification of what these representations tell us about the world and where they remain silent.

References

Azzouni, J. 2004, *Deflating Existential Consequence: A Case for Nominalism*, Oxford University Press.

Batterman, R. 2002, *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence*, Oxford University Press.

Batterman, R. 2005, 'Response to Belot's "Whose devil? Which details?"', *Philosophy of Science* **72**, 154–163.

[11]See Kundu & Cohen 2008, pp. 4-5 for an informal justification of this "continuum hypothesis".

Batterman, R. 2009, On the explanatory role of mathematics in empirical science. *British Journal for the Philosophy of Science*, Advance Access.

Belot, G. 2005, 'Whose devil? Which details?', *Philosophy of Science* **72**, 128–153.

Colyvan, M. 2008, 'The ontological commitments of inconsistent theories', *Philosophical Studies* **141**, 115–123.

Colyvan, M. 2009, Applying inconsistent mathematics. O. Bueno and Ø. Linnebo (eds.), *New Waves in the Philosophy of Mathematics*, forthcoming.

Kevorkian, J. & Cole, J. D. 1981, *Perturbation Methods in Applied Mathematics*, Springer-Verlag.

Kundu, P. K. & Cohen, I. M. 2008, *Fluid Mechanics*, Fourth edn, Academic Press.

Lin, C. C. & Segel, L. A. 1988, *Mathematics Applied to Deterministic Problems in the Natural Sciences*, SIAM.

Maddy, P. 1992, 'Indispensability and practice', *Journal of Philosophy* **89**, 275–289.

Maddy, P. 1998, *Naturalism in Mathematics*, Clarendon Press.

Maddy, P. 2007, *Second Philosophy: A Naturalistic Method*, Oxford University Press.

Nahin, P. J. 2004, *When Least is Best: How Mathematicians Discovered Many Clever Ways to Make Things as Small (or as Large) as Possible*, Princeton University Press.

Pincock, C. 2009, Towards a philosophy of applied mathematics. O. Bueno and Ø. Linnebo, *New Waves in the Philosophy of Mathematics*, forthcoming.

Segel, L. A. 2007, *Mathematics Applied to Continuum Mechanics*, SIAM.