

Asymptotics and the Role of Minimal Models

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1 Introduction

A traditional view about modeling in the physical and applied mathematical sciences holds that one should try to find the most accurate and detailed mathematical representation of the problem at hand. Once this is somehow accomplished (there is no recipe here—the process often requires deep physical intuitions and much experience), one uses the model to make predictions or to try to explain the behavior of interest. If the model fails to capture adequately those features of the phenomenon one is trying to understand, and if one believes that at base the model is sound, then according to this view there are a couple of things one can do. One can try to add more detail to the mathematical representation or one can try to adjust or tweak the parameters appearing in the model to better reflect what’s going on. The aim here is to effect a kind of convergence between model and reality. One tries, that is, to arrive at a completely accurate (or “true”) description of the phenomenon of interest. On this view, a model is better, the more details of the real phenomenon it is actually able to represent mathematically.

Opposed to this conception of modeling is an approach which holds that a good model is one which doesn’t let a lot of these details get in the way. In many cases, the fine details will not be needed to characterize the phenomenon of interest, and may, in fact, actually detract from an understanding of that phenomenon. On this view, what one would like is a good *minimal model*—a model “which most economically caricatures the essential physics.” [7, p. 33] The adding of details with the goal of “improving” the minimal model is self-defeating—such improvement is illusory.

These minimal models typically are highly idealized. But it is because of the extreme idealizations involved that they are candidates for exact solutions. Other, less idealized models, will often require approximation techniques and methods for their “solutions.” The question of interest to us here is which conception of modeling is better and why. Is there a relationship of methodological priority between the different approaches to modeling physical phenomena? In what follows I will present some reasons to think that the “details are better” approach is often misguided. Much depends, as you will already have seen, upon what exactly one means by the “phenomenon” being modeled, as well as on what counts as “the essential physics.”

2 Exactly Solvable Models

There is something slightly ironic about the distinctions we have just been discussing. On the one hand, the first view of scientific models—let’s call it the “traditional approach”—tells us that more details will make for a better model. As we’ve seen, one way of understanding this is that the goal is to have a more precise or *exact* fit between mathematical representation and physical phenomenon. Thus, adding details makes for a more exact model.

On the other hand, the more details that are built into the model, the more intractable the mathematical equations representing the behavior of interest are likely to be. That is, the more one builds into one’s model—the more “exactly” it represents the phenomenon—the more likely it will be that solutions to the equations will require methods of approximation. In this case, of course, the solutions will be, in some sense, less exact.

In his book *Exactly Solved Models in Statistical Mechanics* [4], R. J. Baxter notes a common reaction to exactly solvable—highly idealized—models that is consonant with the traditional conception of modeling. In particular, the models to which he is referring are models of statistical systems as being composed of spins on a lattice. He says

There are “down-to-earth” physicists and chemists who reject lattice models as being unrealistic. In its most extreme form, their argument is that if a model can be solved exactly, then it must be pathological. I think this is defeatist nonsense [4, p. v]

The traditional reaction, to which he objects, is that the world is a very complicated place and any model that can be solved exactly is surely such an obscene caricature that it won’t be able to tell us anything useful about the world at all.

Baxter’s claim that this is nonsense, and his defense of the usefulness of exactly solved models, depends on an appeal to *universality*—the fact that distinct systems can in certain cases display the same type of behavior. I need to say something about what universality is supposed to be. This requires that I also describe, briefly, the sort of phenomena with which Baxter is concerned.

It is a remarkable experimental fact that many systems as diverse as fluids and magnets display identical behavior at and near their respective

individual critical points. This universal behavior is reflected in the fact that the same dimensionless number called a “critical exponent” characterizes the behavior of the system at criticality. Let me explain some of this terminology. See figure 1. The critical point C for a fluid in a container, is a point below which it is impossible to pass, by increasing pressure at fixed temperature, from the gaseous phase to the liquid phase without going through a state in which both gas and liquid are simultaneously present in the container; and above which such a transition from gas to liquid is possible without passing through a regime in which both gas and liquid are simultaneously present. That is, for fixed $T > T_c$ (the critical temperature) one no longer observes a vapor phase followed by a liquid phase as the pressure is increased. Instead there is a unique “fluid” phase. In the figure the lines represent values of P and T for which two phases are simultaneously present. The point A is a “triple point” at which all three phases can coexist.

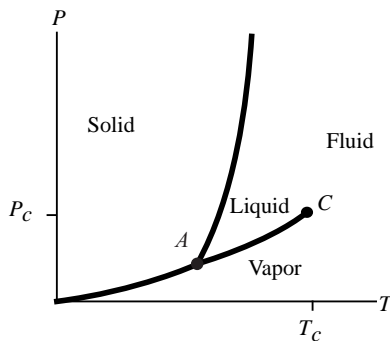


Figure 1: Temperature-Pressure Phase Diagram for a “Fluid”

The critical exponent describes the behavior of the density of the fluid as a function of temperature near the critical point. It describes a universal property in that the same behavior is observed almost without regard for the chemical constitution of the fluid. In the fluid case this universality is expressed by the fact that the order parameter Ψ —the difference between liquid density and vapor density—vanishes as a power of the reduced temperature t :

$$\Psi_{\text{fluid}} = \rho_{\text{liq}} - \rho_{\text{vap}} \propto |t|^\beta. \quad (1)$$

More remarkable, still, is the fact that the very *same* critical exponent, β apparently describes the behavior of magnets as they undergo a transition from

the ferromagnetic state with positive net magnetization below the critical point, to the paramagnetic phase with zero magnetization above the critical point. See figure 2. Here the order parameter is the net magnetization, M :

$$\Psi_{\text{magnet}} = M \propto |t|^\beta. \quad (2)$$

Universality is represented by the fact that the exponent β is the same in both cases.

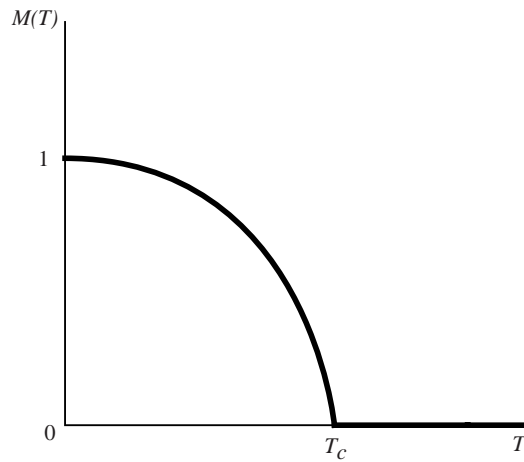


Figure 2: Temperature-Magnetization Phase Diagram for a Magnet

We can identify two features of universal behavior in this context:

1. The *details* of the microstructure of a given “fluid” are largely irrelevant for describing the behavior of the particular system of interest.
2. Many different systems (other fluids and even magnets) with distinct microstructures exhibit identical behavior characterized by the same critical exponent.

It turns out that the only things upon which the universality of critical phenomena depends are the spatial dimension of the system, certain symmetry properties of the Hamiltonian, and the fact that the forces between components are short ranged.

Now we can return to Baxter’s defense of the usefulness of exactly solved models of critical phenomena: One should try to find models for which the Hamiltonian is

sufficiently simple that the partition function can be calculated exactly. [Were one to have the partition function for a real system, one could calculate all of the thermodynamic quantities possessed by the system.] This may not give useful information about the values of the thermodynamic functions, but it will tell us qualitatively how systems behave, in particular near T_c . In fact if we could solve a model with the same dimensionality and symmetry as a real system, *universality asserts that we should obtain the exact critical exponents of the real system.* [4, p. 11, My emphasis.]

The idea is that such a model is a minimal model for critical behavior. Although it is surely a caricature of the actual system, it captures the essential physics for the observed phenomenon of interest.

The assumption of universality is clearly crucial. Without it, there seems to be no warrant for formulating and finding such highly idealized exactly solvable models. Baxter rejects all methods that seek approximate solutions to the partition function. [4, pp. 9–11] Among these he includes the so-called renormalization group. It is interesting and somewhat ironic that the renormalization group method is placed in the category of not-so-valuable approximation schemes, because it is *the* method that allows for an *explanation* of the very property of universality that Baxter requires to justify the usefulness of his exactly solved models. For Baxter, the assumption of universality is based on the surprising experimental results that the critical behavior of distinct systems can be characterized by the same critical exponent. But if one of the aims of mathematical modeling is to provide explanations of phenomena we observe, then it seems the explanation of this observed universality is also something that we should try to capture in our modeling. We would like, that is, an account which justifies, *from the point of view of fundamental theory*, the claim that exactly solvable (or computationally tractable) *minimal* models can feature in genuine explanatory accounts of phenomena we care about. How can these caricatures of real systems play explanatory roles? Asymptotic methods such as the renormalization group can answer this question.

3 Asymptotic Explanation

Let me now say something about the nature of asymptotic methods, how they allow both for an explanation of universality and for a justification of the use of minimal models.

Suppose we observe the following phenomenon. A stiff ribbon of steel—a strut—is securely mounted on the floor in front of us. Someone begins to load this strut symmetrically. At some point, after a sufficient amount of weight has been added, the strut buckles to the left. How can we account for the phenomenon we have witnessed? To satisfactorily answer this we need recognize a contextual ambiguity in the term “phenomenon.”

It’s true that we have witnessed the buckling of a particular strut, and we might only care about this particular event. More likely, though we might care about the general behavior of loaded struts. On the one hand, the *phenomenon* may be the particular event. On the other, the *phenomenon* of interest may be the general fact that struts, upon reaching some critical load, will buckle. (Imagine that I wonder if this was a freak event. Then I’d set about trying to see if it is repeatable. I would mount a similar ribbon of steel and see if it too buckled under similar circumstances.) If I care about building bridges, for example, then it is the general behavior—a pattern—that I will take to be the phenomenon of interest. I may further wonder about the behavior of struts made of different materials. Likely these will buckle under very different loads. Nevertheless, they all buckle and this is the pattern of behavior I’d like to understand.

I do not want to claim that we are never interested in particular events. There exist why-questions concerning the causal and mechanical details leading up to a particular event. However, it does seem that the mathematical modeler is interested primarily in accounting for repeatable patterns of behavior. Particular instances that fail to conform to observed patterns may require different sorts of investigations and different techniques, but they really only become interesting as failures of a generally observed pattern. I think, therefore, that it is fair to say that most mathematical models attempt to represent patterns of behavior.

The thermodynamics of critical phenomena is a case in point. We care about an observed universal macroscopic generalization and we find that this macroscopic phenomenology is largely insensitive to the underlying microscopic details. Similarly, our interest in the buckling behavior of struts is also a concern about universal phenomena. We find, in fact, that there

is a phenomenological formula, called Euler’s formula that characterizes the buckling behavior:

$$P_c = \pi^2 \frac{EI}{L^2}. \quad (3)$$

Here P_c is the critical buckling load, E is Young’s modulus which is a characteristic of the particular material out of which the strut is made, I is the second moment of the strut’s cross-sectional area, and L is the length of the strut. The functional form—that the buckling load P_c is proportional to I/L^2 —is universal. System specific details—that is, details dependent upon the microstructural make-up of the individual systems—are absorbed into the phenomenological parameter, E , which will be different for struts composed of different materials.

It is generally the case that for universal behavior, the microscopic details are in some sense absorbed into a finite number of phenomenological or measurable parameters. The mathematical modeler, as I’ve said, is often concerned to find relations that reflect observable and *repeatable* behavior. An essential part of her method requires the manipulation of a proposed set of model equations so as to extract this phenomenology. This manipulation often involves what I call asymptotic reasoning. Dimensional analysis is one form of this type of reasoning. Let me consider this in the abstract first and then consider a specific illustrative example.

The study of a phenomenon of physical interest often takes the form of finding relationships between the quantity of interest and the other parameters that govern the behavior of the system being studied. Following Barenblatt’s [1] discussion of so-called intermediate asymptotics, let us call the quantity of interest a and the governing parameters $a_1, \dots, a_k, b_1, \dots, b_m$.¹ The distinction between the a_i ’s and the b_j ’s is that the a_i ’s have dimensions that are independent of the other parameters, whereas the dimensions of the b_j ’s are expressible as products of powers of the dimensions of the a_i ’s.² The investigator looks for relationships of the form:

$$a = f(a_1, \dots, a_k, b_1, \dots, b_m). \quad (4)$$

One way of (partially) defining such a function would be provide a table of values containing the values for the a_i ’s and the b_j ’s and the corresponding

¹Sometimes our model will inform us as to the relevant parameters, at other times the best we might be able to do is guess about these from empirical study of the phenomenon.

²He makes this distinction because the study of intermediate asymptotics as Barenblatt understands it, both includes and is a generalization of, dimensional analysis.

value for a in each case.³ In the age of modern computers with the ability to run impressive numerical calculations, it is often claimed that this is all that is required to answer our questions about a —the quantity of interest. Barenblatt puts the point as follows:

Please, it is often said, formulate the problem whose solution you need and you will obtain without special effort a set of tables of all the numerical values you need with the desired (and, of course, paid for) accuracy. To understand this point properly [*that is, to see what's wrong with this point of view*] we have to recall that the crucial step in any field of research is to establish what is the minimum amount of information that we actually require about the phenomenon being studied. All else should be put aside in the study. This is precisely the path taken by the natural sciences since Newton. Features that are not repeated from case to case and cannot be experimentally recorded are always deliberately thrown out. Thus, the researcher is actually not so much interested in a table of values of the function $f \dots$ as in the principal physical laws that determine the major features of this relationship. These principal laws, the basic features of the phenomena can often be seen and understood when analyzing the intermediate asymptotics. [1, pp. 91–92]

The “principal laws” or “basic feature” are the phenomenological generalizations we are after. The sort of understanding we want is achieved once we are able to elucidate these principle laws. The asymptotic methods required to elucidate these laws play an essential role in this understanding. Barenblatt notes that these principal law are often determined by an “intermediate asymptotic” analysis of the proposed fundamental governing equation—the mathematical model, which typically is a partial differential equation.

We can begin to see what he means by “intermediate asymptotics” if we think of an impressionist painting such as Seurat’s pointillist “A Sunday Afternoon on the Island of La Grande Jatte.” A characteristic of this painting and of the impressionists, in general, is the fact that if you are too close, you get lost in the details—all you see are the various colored points. But also, as with any painting, if you are too far away, you cannot discern the image

³There are obviously cardinality problems with this suggestion of which Barenblatt, somehow, seems unaware. But the conceptual point is clear enough.

either. The “best” view—the view where one can best appreciate the image (or “understand” it)—is to be had at some intermediate distance from the canvas.⁴

Something analogous is involved in “any scientific study.”

Thus, the primary thing in which the investigator is interested is the development of the phenomenon for intermediate times and distances away from the boundaries such that the effects of random initial features or fine details in the spatial structure of the boundaries have disappeared but the system is still far from its final equilibrium state. This is precisely where the underlying laws governing the phenomenon (which are what we are in fact primarily interested in) appear most clearly. Therefore intermediate asymptotics are of primary interest in every scientific study.
[1, p. 94]

This last claim is probably a bit too strong. However, I think that broadly construed it is true of much of what gets studied in physics and other sciences to which mathematicians have applied their skills.

Barenblatt formulates a recipe for analyzing the intermediate asymptotics of a problem—for discovering the principal laws that govern it. One begins with a suggested dependence relation of the form given in (4). One might formulate this relation given only basic background empirical knowledge, or one might actually have a governing equation—a model—that tells you that the quantity of interest will be a function of the certain a_i ’s and b_j ’s. Next one searches for so-called self-similar or scaling solutions to this dependence equation. (In a moment I will discuss why this is important. Right now let me just outline the recipe for asymptotic analysis.)

As a first attempt, one employs *dimensional analysis* to determine the functional relationship between a and the other parameters. One expresses the dimensions of the dependent parameters b_j as products of powers of the dimensions of the independent parameters a_i . Thus, for each b_j we write:

$$[b_j] = [a_1]^{r_{j1}} \dots [a_i]^{r_{ji}} \dots [a_k]^{r_{jm}}. \quad (5)$$

⁴I am virtually ignorant of theories of art interpretation. So nothing here is meant to deny that part of “understanding” the painting is appreciating the details of *how* it was painted. Nevertheless, there does seem to be a range of distances at which the picture is best discerned.

Given the assumption that the set of governing parameters is complete, the dimension of the quantity a must also be expressible as the product of powers of the dimensions of the independent parameters a_i [1, p. 41]:

$$[a] = [a_1]^{r_1} \dots [a_i]^{r_i} \dots [a_k]^{r_k}. \quad (6)$$

Next, one nondimensionalizes the problem by forming the dimensionless parameters:

$$\Pi = \frac{a}{a_1^{r_1} \dots a_i^{r_i} \dots a_k^{r_k}} \quad (7)$$

$$\Pi_j = \frac{b_j}{a_1^{r_{j_1}} \dots a_i^{r_{j_i}} \dots a_k^{r_{j_m}}}, \quad (j \in \{1, \dots, m\}). \quad (8)$$

One can now rewrite the dependence relation (4) as follows:

$$f(a_1, \dots, a_k, b_1, \dots, b_m) = a_1^{r_1} \dots a_k^{r_k} \Phi(\Pi_1, \dots, \Pi_m), \quad (9)$$

which is the same as the nondimensionalized:

$$\Pi = \Phi(\Pi_1, \dots, \Pi_m). \quad (10)$$

The next step in the analysis of the intermediate asymptotics of a problem is to examine the relative “size” of the dimensionless parameters Π_j . Those that are not considered too small or too large are deemed to be essential parameters. On the other hand, if a parameter say, Π_m can be considered to be small (or large) one *assumes* that its influence on the problem can be neglected. If this is a legitimate assumption, and if Φ converges sufficiently rapidly as $\Pi_m \rightarrow 0$, then Φ can be replaced by a function Φ_1 of one less argument:

$$\Pi = \Phi_1(\Pi_1, \dots, \Pi_{m-1}). \quad (11)$$

These are, of course, very big *ifs*. But if they are satisfied, then we will be able to find a self-similar solution to our problem. Barenblatt calls this intermediate asymptotics of the first kind.

Self-similar solutions are important. They typically are solutions to a degenerate idealized problem. For example, suppose our problem was to determine the temperature distribution in a long bar at a time $t \gg t_0$ after which an initial amount of heat had been released into the bar say by holding

a lighter under it a some point. Suppose that the actual initial distribution was Gaussian of “width” l :

$$\theta(x, 0) = \frac{M_0}{\sqrt{2\pi}l} e^{-\frac{x^2}{2l^2}}. \quad (12)$$

M_0 is the “mass” of the initial distribution. That is, M_0 is the initial amount of heat released into the bar and satisfies the following equation:

$$M_0 = \int_{-\infty}^{\infty} \theta(x, t) dx \quad t \geq 0. \quad (13)$$

We are interested in determining the temperature, θ at some point x (a distance x from the source assumed to be at $x = 0$). We make the educated guess that θ at time t should be a function of the time t , the distance x from the source, the thermal diffusivity κ , the width l of the initial distribution, and the “mass” or quantity of heat M_0 ⁵:

$$\theta = f(x, t, \kappa, l, M_0). \quad (14)$$

If this were known, then using the recipe just described (even without knowing that the phenomenon was governed by the heat equation), we can arrive at a solution having the *form*:

$$\theta(x, t) \sim \frac{M_0}{\sqrt{2\pi(\kappa t)}} e^{-\frac{x^2}{2(\kappa t)}} \quad (t \rightarrow \infty). \quad (15)$$

Note that this solution lacks a parameter that appears as an argument in the dependence relation (14)—namely, the width l of the initial distribution. In finding (15) we take the limit $\Pi_l \rightarrow 0$. In other words, the result (15) is the solution to the idealized problem in which we keep t fixed and let $l \rightarrow 0$. The long-time asymptotic behavior of the temperature distribution for a wide variety of different initial conditions (initial distributions with different widths l) is given by the special self-similar degenerate solution in which the initial distribution is a delta function:

$$\theta(x, 0) = M_0 \delta(x). \quad (16)$$

⁵ $[\theta] = \Theta, [t] = T, [\kappa] = L^2 T^{-1}, [M_0] = \Theta L, [x] = L.$

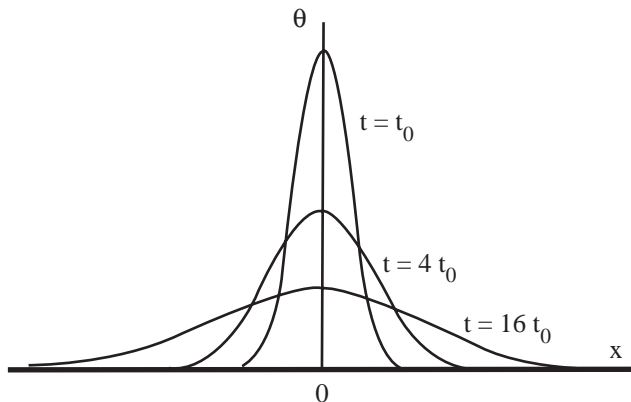


Figure 3: θ vs. x

The various actual distributions at different times are shown in figure 3. We can see the self-similarity here by plotting each of these curves in dimensionless “self-similar coordinates” $(x/\sqrt{kt}, \theta\sqrt{kt}/M_0)$. Each solution in figure 3 gets is represented by the same curve in these coordinates. See figure 4.

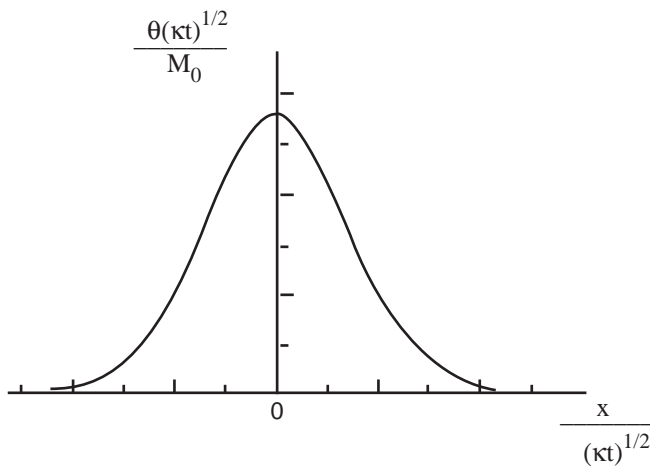


Figure 4: Each curve of Figure 3 in “self-similar” coordinates.

Self-similarity solutions are important as solutions to the special idealized degenerate case in which the initial distribution is a delta function. But they

clearly have much greater significance than this. They are also solutions to non-self-similar problems—problems, e.g, where $l \neq 0$. The existence of intermediate asymptotics of the first kind demonstrates that the process under investigation has solutions that stabilize in some asymptotic domain. And, so the idealized (far from actual) self-similar solutions have genuine physical significance.

Goldenfeld [6] offers a way of understanding this notion of stability that has an epistemic ring to it. Let me stress, however, that this epistemological characterization is really a consequence of a more fundamental, ontological conception of stability. The idea is that within the asymptotic long-time regime it is not possible to retrodict the actual history of the temperature evolution. That is, one cannot decide whether

- (i) $[\theta]$ was a delta function at $t = 0$, or (ii) $[\theta]$ was a Gaussian distribution with width l at $t = 0$, or (iii) $[\theta]$ was a Gaussian distribution with width l at $t = 10$. These and many other histories all give rise to the same distribution $[\theta(x)]$ [namely, that given by equation (15)] at sufficiently long times, i.e, in the intermediate asymptotic regime. [6, p. 304]

The model in which θ is a delta function at $t = 0$ is a *minimal model*. The details of the actual initial distribution are irrelevant, given our interest in the long-time phenomenology of the temperature distribution in the bar. In other words, given our interest in the *universal* long-time asymptotic behavior of the temperature distribution, we may as well opt for the simplest or most convenient model of the short-time, initial behavior of the system. Had we other interests—for example, had we cared about the initial transients and the details of the approach to the scaling solution for a particular bar, then, naturally, such a minimal model will not be appropriate. But then the “phenomenon of interest” would be not be that of a repeatable pattern of behavior.

The analysis just presented is essentially dimensional analysis. It is the simplest form of asymptotic reasoning that can model the principal laws and basic features—the similarity solution—that we want to explain. It’s clear that traditional view of modeling—the “more details the better” view—will fail to capture the emergence of the stable long-time asymptotic behavior that we care about.

Unfortunately, this simplest form of asymptotic analysis often fails. The most critical assumption that we made was that the limit $\Pi_m \rightarrow 0$ is regular.

Without this, dimensional analysis will completely fail. This leaves us with two possibilities: Either there still exists a self-similar solution or there is no similarity solution at all. If the latter is the case, then there isn't much more to be said. The problem will require other methods for solution. On the other hand, if we can find asymptotic limiting self-similar solutions despite the singular nature of the limit $\Pi_m \rightarrow 0$, then we have what Barenblatt calls "intermediate asymptotics of the second kind." In this case we assume that as $\Pi_m \rightarrow 0$, the function Φ has a power-type asymptotic representation as follows:

$$\Phi = \Pi_m^{\alpha_m} \Phi_1 \left(\frac{\Pi_1}{\Pi_m^{\alpha_1}}, \dots, \frac{\Pi_{m-1}}{\Pi_m^{\alpha_{m-1}}} \right) + o(\Pi_m^\alpha).^6 \quad (17)$$

The exponents $\{\alpha\}$ are certain constants to be determined (some of which may very well be zero) and parameter b_m (appearing in the original dependence equation of the form (4)) is no longer eliminable when one takes the (singular) limit $\Pi_m \rightarrow 0$. In fact, the exponents $\{\alpha\}$ are so-called anomalous dimensions. They are solutions of a nonlinear eigenvalue problem.

This talk of anomalous dimensions should bring to mind renormalization group arguments used to determine the anomalous exponents that characterize, e.g., universality classes of critical phenomena in statistical physics. And, in fact, Goldenfeld *et al.* and Barenblatt have done much to show that the renormalization group approach is essentially equivalent to implementing Barenblatt's recipe for intermediate asymptotics of the second kind. The only real difference between the renormalization group approach and Barenblatt's intermediate asymptotics is that in the statistical case, the asymptotic domain emerges in the large-scale thermodynamic limit; whereas in the examples of intermediate asymptotics we have been discussing, the asymptotic domain emerges in the long-time limit.

The self-similar or scaling solution (formally represented by equation (17)) will be, in fact, a fixed point of an appropriate renormalization group transformation. Thus, it characterizes a class of "phenomenologically" equivalent systems—all those exhibiting the same, that is, *universal*, intermediate asymptotic behavior. One then seeks to find a minimal model within this class of phenomenologically equivalent systems. Such a model will often be an exactly solvable model of the sort upon which Baxter focuses.

⁶The second summand is arbitrarily small compared with the first for sufficiently small Π_m .

4 Conclusion: The Role of Minimal Models

I believe that the best way to think of the role of asymptotic methods in mathematical modeling is that they are a means for extracting *stable phenomenologies* from unknown and, perhaps, unknowable detailed theories. (See [6] for a discussion of this point of view.) In the context of statistical physics, these detailed theories would characterize the actual microstructures and the actual intermolecular forces at play in a given physical system composed of many interacting particles. In many instances we neither know about nor care about these details, but would nevertheless like an account of what we observe—the repeatable macrolevel phenomena. What happens below some level of detail or description is mostly irrelevant given our interests.

The most important feature of the phenomenon of interest is its repeatability. Mathematically this manifests itself in terms of a kind of stability under perturbation of the microscopic details. This type of stability is known as “structural stability.” Stability guarantees that many different microscopic models will be compatible with the observed phenomenology. In the jargon of the renormalization group, these different detailed models will be in the same universality class defined by the stability properties of the renormalization group transformation.

Lest I be accused of buying into the so-called “stability dogma”⁷—the idea that *all* observed phenomena must be modeled by structurally stable equations—I need to qualify these remarks somewhat. The sophisticated modeler will allow that there are observable behaviors that aren’t repeatable. (Transient behavior is a good example.) A good model will allow for this fact. Therefore, we should require only that repeatable behavior is structurally stable. Thus, I advocate the view that what one wants is a “restricted” sense of structural stability. In this way, the criticisms of the stability dogma can be avoided. Chen *et al.* put the point as follows:

[i]f there are unstable or irreproducible aspects in the actual system being modeled, then a good mathematical model of the system must have features unstable with respect to the perturbation corresponding to that causing instability in the actual system. Thus a good model should be structurally stable with respect to the reproducibly observable aspects, but must be unstable with

⁷See [8, p. 259] for a critique of this dogma.

respect to the hard-to-reproduce aspects of the actual system. [5, p.117]

Let us say that an *ideal* model is one which captures *all* of the behaviors—repeatable and unrepeatable—of the system of interest. Such a model is the aim of the traditional approach to modeling. (Note that “ideal” here does not mean “idealized”—just the reverse, in fact.) It is, for all but the simplest systems, an unattainable goal. On the other hand, we have seen that *highly idealized minimal models* of the universal, repeatable features of a system are often obtainable. Asymptotic analyses of the sorts we have been considering have their formulation as the primary goal.

We see also that there is *no unique microscopic (or short-time) model* corresponding to the universal behavior of interest. In our example of the spreading of heat through the bar, we saw the difficulties involved in retrodicting, from the observed long-time phenomenology, the details of the initial heat distribution. However, since the heat equation is derived from a conservation law, it is the case that the initial “mass” of the distribution can be determined from the long-time principal law that we discovered. On the other hand, in the case of singular intermediate asymptotics of the second kind—where the physics is genuinely singular—even this weak retrodiction cannot generally be determined. An example I have discussed elsewhere involves the superficially similar spreading of a groundwater mass through a porous medium such as sandstone.⁸ For such cases, which are by far more common in mathematical physics than those involving intermediate asymptotics of the first kind, the nonuniqueness of a microscopic (or short-time) model relative to the phenomenology of interest cannot even be given an epistemic gloss. The nonuniqueness is a fundamental consequence of the singularities involved in the physics.

Such situations are completely analogous to problems encountered in quantum field theory. In fact, the unobservability in principle of the initial (groundwater) “mass” given the long-time scaling solution of the form (17) is completely analogous to the unobservability of the “bare” electric charge at long distances or low energies in quantum electrodynamics. The philosophy behind effective field theory endorses this unobservability as a virtue. Relative to a given energy range or to a given distance, there is no unique physics—no unique theory—beyond the relevant cut-off. As a result,

⁸See [3] for a detailed discussion of this example.

with respect to the universal behavior such theories attempt to characterize, one ought to search for a minimal model—a model for which, if we are lucky, exact solutions will be possible.

Minimal models play crucial computational and *explanatory* roles. Their use is justified by the asymptotic emergence of stable structures characterized by universality classes. In effect, one might as well calculate universal properties using minimal models.⁹ They are guaranteed by the asymptotic methods to yield conclusions that are accurate for any system in that universality class.

Let's return to the question raised at the beginning: Is there a kind of methodological priority among the different approaches to modeling in science? The answer is “yes.” Given our primary interest is to understand repeatable phenomenological behavior, it seems we ought first to search for minimal exactly solvable models of that behavior. The asymptotic methods involved in justifying the use of such models to explain universality themselves provide the understanding of this type of repeatable phenomena. Once we have these minimal models and we have our understanding of the universality of the phenomena, we can begin to look to try to understand nonrepeatable behavior displayed in different systems. We can, that is, begin to add more and more details to our mathematical models with the ultimate aim of finding an “exact” or “ideal” model. However, the irony that we noted earlier remains. The more details one adds, the more likely approximative techniques will be required for solving the equations; and, in a sense, the less exact will be the solutions we can actually produce.

⁹See [2] for a discussion of this strategy in a completely different context—that of justifying Gibbs' method in equilibrium statistical mechanics.

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