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SYMPOSIA PAPER

The Problem of the Missing Model

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Abstract

Missing-model problems are a type of problem that arises when scientists are trying to gain access to initially inaccessible phenomena. This article describes what missing-model problems are and examines examples of them from seismology, optics, and the theory of gases. It is argued that the examination of missing-model problems will give us a better understanding of the challenges scientists face when attempting to gain access. Two themes for the further study of missing-model problems are laid out: justification and representation.

1. Introduction

In the history of the physical sciences, there have been numerous significant cases where scientists face a challenge of the following form: They have access to a certain set of phenomena, and they are trying to infer facts about a set of phenomena to which they do not have direct access. They are trying to gain access to them.

An important example from the history of the physical sciences comes from microphysics. There is a set of phenomena that are accessible to us macrophysically—such as Brownian motion or spectroscopic data. From these accessible phenomena, scientists have attempted to infer microphysical phenomena that are not directly accessible to us, such as the sizes and masses of atoms. This particular example is of great significance in philosophy of science because of its central role in the realism debate.

Another example comes from seismology. We cannot directly access the Earth's interior beyond a very thin layer at the Earth's surface. Almost all the knowledge we have about the Earth's interior comes from seismology. Whenever an earthquake occurs, seismic waves emanate outward, some of them traveling deep within the Earth. These waves, containing information about the Earth's deep interior, are then detected by seismometers located at the Earth's surface. The state of the Earth's interior is inferred from these waves.

This article will describe a type of problem that sometimes arises in such cases in which scientists are trying to gain access to initially inaccessible phenomena. I will

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refer to such problems as *missing-model problems*. In the rest of this article, I will first explain what a missing-model problem is, using seismology as an illustrative example. I will then examine other instances of missing-model problems from the nineteenth century, focusing on optics and the theory of gases. Through these examples I hope to show that by examining missing-model problems, we gain a better understanding of the challenges scientists face when they are trying to gain access, and the ways in which these challenges must be addressed. I will end by briefly laying out two themes for the study of missing-model problems: justification and representation.

2. Missing-Model Problems

Almost all our knowledge about the Earth's interior comes from observations of seismic waves made at its surface. Knowledge about the Earth's interior is obtained through model-based measurement (Tal 2011). Seismologists construct models that mediate between a representation of phenomena in the Earth's interior and a representation of phenomena at the Earth's surface. Phenomena are measured at the Earth's surface, and then the model allows seismologists to make inferences about the Earth's interior. I will take this general picture of measurement for granted in this article.

Here is what a missing-model problem is, in a nutshell. The only way to obtain knowledge about the target phenomena in some sciences like seismology is through model-based measurement. Model-based measurement requires a model. But what does one do when it is not antecedently known what the appropriate model to use is?

Here is an example. Earthquakes happen when an event occurs at a fault that suddenly releases elastic energy, and this energy propagates outward from the fault in the form of seismic waves. This event is called the seismic focus. The focus is usually located at some point deep underground, inaccessible to us. A question that seismologists have been interested in since the beginnings of seismology in the mid-nineteenth century has been: Exactly what happens at the focus when an earthquake occurs? Because the focus is inaccessible to us, the only way to get at it is to infer it from seismic waves that travel to the Earth's surface. By the early twentieth century, a global network of seismographs had been put in place, and seismologists had huge amounts of data that could potentially be used to make such an inference.

But how do you make the inference? Huge amounts of data can be recorded, but the data are recordings of ground movements far away from the focus. They do not, by themselves, tell us anything about the focus. How can this data be turned into evidence about the focus? More generally, suppose there is a target phenomenon that you want to obtain knowledge about, but you do not have direct access to it. What you do have access to are observable effects of this target phenomenon. What do you need in place to measure physical properties of the target phenomenon? What are needed are the following:

(1) A parameterization of the target phenomenon: A set of parameters that are taken to represent physical properties of the target phenomenon.
(2) A parameterization of the observable phenomena: A set of parameters that represent measurable properties of observable phenomena.

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(3) A measurement model: A set of equations that relate the observable parameters to the target parameters, which may be used to determine values for the target parameters, given values for the observable parameters.

A number of conditions must be satisfied for this to work properly but let us set those aside for the purpose of this article. Let us assume that such conditions may be met, at least under some circumstances.

Let me return to the seismic focus example to illustrate the problem. In this case, the seismic focus is the target phenomenon, and seismic waves observed at the Earth's surface are the observable phenomena. The problem in seismology in the early twentieth century was that nobody knew what to do about two things. First, what is the proper set of target parameters, the parameters for representing the seismic focus? Seismologists knew so little about what seismic foci are that they did not know what physical parameters would be needed to characterize them in an appropriate way. Second, what is the measurement model, the relation between the target parameters and the observable parameters? The measurement model should be a set of equations that bridge between the seismic source and the seismic waves at the surface. Such equations would allow you to specify: If the seismic focus is like this, then the seismic waves we observe at the surface ought to be like that. These equations could then be used to make inferences from seismic wave observations back to properties of the seismic focus.

By the 1930s, theoretical seismology had gone a good bit of the way toward solving the second problem (Miyake 2017). Seismic waves are elastic waves. Suppose there is a block made out of an isotropic, homogeneous, elastic medium. Take a point in that block and imagine applying a unidirectional impulse force on it. Elastic waves would then emanate from that point. What would the elastic waves look like? This problem was solved in 1904 by A. E. H. Love. If the seismic focus could appropriately be modeled as a unidirectional impulse force, this would go a good way toward solving the problem of the missing model. The proper set of physical parameters for the target phenomenon, the seismic focus, would then be the set of parameters one needs to characterize a unidirectional impulse force: spatial coordinates, time, orientation, and magnitude of the impulse. Elastic wave theory would then provide the measurement model. It would be the set of equations that connect the parameters of the impulse force with the parameters that capture properties of seismic waves as they are detected at various locations on the Earth's surface. You could then, in principle, obtain the parameters of the seismic focus using model-based measurement. In seismology, this type of measurement is usually referred to as *inversion*.

Unfortunately, the seismic focus cannot be appropriately represented as a unidirectional impulse force. At least two forces are at work in a fault, that work in opposite directions on opposite sides of the fault. Something more complex is required. Two alternative possible representations of the seismic focus were proposed by seismologists in the early twentieth century. They were called the *single couple* and the *double couple*. The parameters needed to characterize a couple are spatial coordinates, time, orientation, and moment of the couple. The problem of determining the equations for the elastic waves that would emanate from a single couple and a double couple, respectively, was solved in the 1920s by Hiroshi Nakano (Miyake 2017, 115–16). These equations would then make possible the model-based

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measurement of the seismic focus. If the seismic focus is represented as a couple, the target parameters would be the spatial coordinates, orientation, and moment of the couple. The observable parameters would be the amplitudes of seismic waves over time as detected at various points on the Earth's surface. You could, in principle, measure the moment of the seismic focus by measuring the amplitudes of seismic waves over time at the Earth's surface.

The only problem was that seismologists couldn't come to an agreement about whether the proper representation of the seismic focus is a single couple or a double couple. This was called the seismic mechanism controversy, and it lasted from the 1930s to the 1960s (Miyake 2017). The seismic mechanism controversy was ultimately resolved by a theoretical breakthrough, which showed that the double couple is the proper representation of the seismic focus if the seismic mechanism is elastic rebound. This then allowed seismologists to measure a physical parameter of the seismic focus, namely, the moment of the double couple that represents the seismic focus, now called the *seismic moment*. This theoretical breakthrough then led to the further development of a representation of the seismic focus in terms of a tensor, now called the *seismic moment tensor*, and methods were developed for determining the seismic moment tensor of any given earthquake from seismic wave observations made at the Earth's surface.

3. Missing-model problems and the history of the physical sciences

This is a good example of something that has happened quite often in the history of the physical sciences. There is a target phenomenon that you do not have direct access to, but you have good reason to believe that it is connected to a certain set of phenomena to which you *do* have access. To gain access to the target phenomenon, you need to do model-based measurement. To do that, you need a model. But you do not know in advance what the right model is, and sometimes you do not even know the right set of parameters with which to characterize the target phenomenon. The problem of finding the right set of parameters and the right measurement model to enable model-based measurement is what I call a *missing-model problem*.

Missing-model problems are important in the history of the physical sciences, particularly in the nineteenth and twentieth centuries. There is a well-known tradition in nineteenth century physics, including figures such as George Stokes, Lord Kelvin, James Clerk Maxwell, Lord Rayleigh, and others, mostly educated at Cambridge. This is a tradition that is often associated with modeling. What was the point of the modeling that they were doing? There are, to be sure, numerous reasons why one might want to build models, but one important reason is this: They were trying out ways to solve missing-model problems. Just as seismologists were modeling things like the seismic focus to gain access to the Earth's interior, these scientists were modeling to gain access to microphysics and the ether.

To understand the origins of this tradition, we need to look at the generation of scientists before Stokes and Kelvin. One example of the kind of work this previous generation was doing is in optics. In the 1820s, there were two people in Britain who were doing the most advanced work in optics at the time: John Herschel and David Brewster. When we look back at the optics of the time, we tend to see it in terms of the competition between two major theories: the corpuscular theory and the wave

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theory. I think one part of the motivation for doing optics at the time, though, is underappreciated. They were trying to gain access to the microstructure of matter, and optics would provide the means for doing so.

This is clear if we look carefully at the writings of Herschel and Brewster at the time. In 1830, a 245-page article on "Light," by John Herschel, was published in the *Encyclopaedia Metropolitana*. It was a review article that laid out the state of the art at that time in optics, including both the corpuscular and wave theories of light, and it was the bible for a generation of researchers in the mid-1800s working on optics (see Good 1987). On the first page of the article, Herschel writes:

> Polarized light is, in the hands of the natural philosopher, not merely a medium of vision; it is an instrument by which he may be almost said to feel the ultimate molecules of natural bodies, to detect the existences and investigate the nature of powers and properties ascertainable only by this test, and connected with the more important and intricate inquiries in the study of nature. (Herschel 1830, 341)

Herschel refers to polarized light here as an instrument that is to be used to probe the "ultimate molecules" of bodies. Herschel was not the only one who wrote of optics as an instrument for gaining access to microphysical phenomena. Brewster, in his 210-page article on "Optics" for the *Edinburgh Encyclopedia*, writes:

> Had the subject of double refraction and polarisation formed only a branch of physical optics, the splendour of its phenomena, and the beauty of its laws, must have assigned it a high rank among the objects of scientific inquiry; but its importance arises above all limits, when we consider it as an instrument of research by which we can examine the structure and constitution of organised matter, and thus penetrate into regions which seemed to have been placed beyond the reach of human sagacity. (Brewster 1832, 617)

Brewster, too, refers to polarized light as an "instrument of research" for examining the structure and constitution of matter. Both Herschel and Brewster have a particular line of research in mind here. They were at the forefront of work on an optical phenomenon that initially seemed promising but eventually ended up being a dead end. As Good (1987) recounts, polarized light was discovered in 1808, and soon thereafter a theory of polarization was developed by Jean-Baptiste Biot and published in 1816. The work of Herschel and Brewster in the 1820s focused on elaborate and sometimes quite spectacular patterns of colored rings that can be observed when a biaxial crystal is placed between two crossed polarizers. Herschel in particular was working on ways to separate out and quantify features of these patterns and relating them to mathematical statements from Biot's theory.

On my view, the motivation for Herschel was to be able to end up with equations that would have, on one side, parameters representing microphysical properties of crystals, such as the magnitudes of forces acting between the crystal molecules and light corpuscles, and on the other side, parameters representing observable details of the optical patterns. That is, he was trying to solve the missing-model problem, so that observations of these optical patterns could be used to make inferences about the

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microphysics of crystals. In an 1822 paper that discusses these patterns, he writes: "It has opened new views on the structure of crystals, and will in all probability be the means of leading us to a more intimate knowledge of the nature and laws of those forces by which the ultimate particles of matter act on light and on each other" (Herschel 1822, 21).

This line of research did not ultimately lead to discoveries about the microstructure of matter. For one thing, the theory of optics on which this research was predicated, Biot's theory of mobile polarization, turned out to be wrong. The theory of optics that Herschel eventually adopted by the 1830s was the Fresnel wave theory. It turns out, further, that the optical patterns are not dependent on microstructural details of crystals, so they could not provide a means of gaining access to microphysics. The program failed because no model could be constructed that would connect the microstructure to measurable details of the ring patterns. They failed to solve the missing-model problem.

I want to emphasize here that this is no cautionary tale. Neither Herschel nor Brewster, nor anyone doing optical research in the 1820s, could have known in advance that the program would fail. The only way to make progress in such cases is by trying things out. Herschel took Biot's theory, constructed models connecting microphysical details of crystals with observable details of the ring patterns, and attempted to make inferences to those microphysical details. It did not work. But how else ought one to proceed in such cases?

It turns out that the path toward gaining access to microphysics didn't go through the investigation of crystals, but the investigation of gases, in the work of Clausius, Maxwell, and Boltzmann in the mid-nineteenth century. Let me focus here on the work of Maxwell. On my view, the investigation of gases in the mid-nineteenth century was in a situation analogous to seismology in the early twentieth, and optics in the early nineteenth centuries. They had a missing-model problem. There were numerous macroscopic phenomena involving gases, of which quantitative measurements could be made. These include measurements of pressure, volume, and temperature, and the relations between these parameters. They also include phenomena like diffusion, viscosity, and specific heats. Could models be built that would bridge between such macroscopic phenomena, and parameters of the microscopic systems that make up the gas?[1]

Let us take as an example Maxwell's famous 1860 paper, "Illustrations of the Dynamical Theory of Gases." On the first page of this paper, Maxwell refers to work by Clausius according to which the mean free path of a particle in a gas may be determined in terms of the average distance between particles and the distance between the centers of the particles when a collision takes place. He then states:

> We have at present no means of ascertaining either of these distances; but certain phenomena, such as the internal friction of gases, the conduction of heat through a gas, and the diffusion of one gas through another, seem to indicate the possibility of determining accurately the mean length of path which a particle describes between two successive collisions. (Maxwell 1860, 378)

[1] The view that kinetic theory provides a means of gaining access to microphysics originates from the work of George E. Smith. See in particular chapter 2 of Smith and Seth (2020).

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The phenomena described here—internal friction (or viscosity), heat conductance, and diffusion, are all macroscopically observable phenomena of gases. Maxwell is proposing to measure a microscopic parameter—the mean free path—from macroscopic observations.

What is needed here is a model that bridges the microscopic and the macroscopic phenomena. This motivates the famous derivation in the paper in which he proposes that the velocities of the molecules that make up a gas are distributed according to a normal distribution. He proceeds to derive an equation—a measurement model—that relates the internal friction of air to the mean free path, and from measurements of the internal friction, deduces a value for the mean free path of 1/447,000 of an inch (Maxwell 1860, 391). In section 2 of the paper, he derives[2] another equation that relates the mean free path to the diffusion of two gases passing in opposite directions through an opening, and from measurements of the diffusion of olefiant gas, deduces a value for the mean free path of 1/389,000 of an inch (ibid., 403). At the end of the paper, Maxwell makes a point of stating that the value for the mean free path determined through diffusion is "not very different from that deduced from experiments on friction." Maxwell emphasizes that he has come up with several ways of bridging between microphysical parameters of the system, such as the mean free path, and measurable macrophysical parameters like conduction, viscosity, diffusion, and specific heats. This depends, of course, upon a model that assumes that the molecules are hard elastic particles, which he knows is likely to be false. But it is an attempt to gain a foothold into the microphysical realm by coming up with a set of measurement models.

Naturally, one might ask: What evidence could there have been, in 1860, that these models capture the microphysical properties of gases with any accuracy? What justifies Maxwell in the use of these models? Let me give a very brief answer here. The use of these models must be understood as part of a historically extended process of gradually obtaining more and more evidence about initially inaccessible systems, slowly filling in more and more details. I will say more about this point in the next section.

4. Themes for the study of missing-model problems

The point I want to make in this article is a programmatic one. I am proposing a way of thinking about models in the physical sciences and their actual historical development that I think will be fruitful. Missing-model problems arise when there is a target phenomenon that is not directly accessible, and very little is known about it antecedently. The problem is: How do you construct a model that will allow you to gain access, and how do you use that model to determine further and further details of the target phenomenon? The study of missing-model problems, and how they are resolved, will give us a better understanding of the growth of knowledge in the physical sciences. I will end this article with two intertwined themes to guide further research on missing-model problems.

[2] This derivation was subsequently called into question by Clausius, and Maxwell later admitted that it was erroneous, but my point here is about what the motivation is for the derivation.

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The first theme is justification. Because, in these cases, very little is known antecedently about the target phenomena, measurement models may be constructed even when there is very little evidence for their validity. So how can the use of these models be justified? Here is an initial attempt at an answer. Suppose there is some target phenomenon that you want to gain access to, and there is a set of observable phenomena through which you believe you might be able to gain access to the target phenomenon. You start by constructing measurement models that are plausible, but which might lack much justification. You use these models to make measurements of parameters of the target phenomenon. You then look for discrepancies in these measurements. This may, for example, take the form of disagreements between two different measurements of what is taken to be the same quantity, such as the two different measurements by Maxwell of the mean free path. The discrepancies are then examined to try to infer what is going wrong. It could be that the measured parameter, such as mean free path, does not correspond to a stable quantity of the target phenomenon. It could be that the measurement model leaves something out. Refinements are made, either to the representation of the target phenomenon, or to the measurement models. Further measurements are made using the refined models. And the process is repeated, so that further and further refinements are made, either to the representation of the target phenomenon, or the measurement models. In some cases, it turns out that this process does not work, as was the case with attempts to gain access to the microphysics of crystals through optics. A case in which this method worked spectacularly well is in the 200 years of celestial mechanics after Newton, as described in detail by Smith (2014). The account of the method given here is based on Smith's account of the methodology of celestial mechanics. Significantly, fundamental features of this method were recognized by John Herschel through his work on physical astronomy in the 1820s, and it was described in his *Preliminary Discourse on the Study of Natural Philosophy* (Miyake 2023).

The second theme is representation. Parameterizations of target phenomena, and measurement models, always depart in some ways from reality. Consider the double couple as a representation of the focal mechanism of an earthquake. The actual focal mechanism is an incredibly complex process that unfolds on a section of a fault that can be dozens, sometime hundreds of kilometers long. So you can think of the moment of the double couple that represents the focal mechanism as an averaging out of all the forces involved in this incredibly complex event. But in what sense is this average the "appropriate" representation? When you try to use a model to represent complex systems, you often have to take averages in this way. But there is more than one way to take an average. What makes one averaged representation better than another? An initial answer is this: It has to do with in the process that I just described, which involves moving from an averaged-out or idealized picture to one with more and more details in it. An appropriate representation is one that can be used successfully in this process.

What this means is that if we want to understand the justification for the use of these models, or how these counterfactual representations can successfully result in knowledge, we need to examine carefully their role in historically extended and complex processes through which we gain access to more and more details of initially inaccessible phenomena.

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