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ESSAY REVIEW

Models all the way down

Paul N. Edwards: A vast machine: Computer models, climate data, and the politics of global warming. Boston MA: The MIT Press, 2010, 528pp, $32.95/£24.95 HB

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When I entered graduate school in 1984, there was scant serious work in history or philosophy of the modern earth sciences. Excellent scholars had written on Darwin, Lyell and nineteenth century geology, and some of that work heeded earth science *qua* earth science, but much of it attended to geology only as something that helped to lay the groundwork for an advance in biology: Darwin's theory of the origin of species through natural selection. Few had written anything of note on twentieth century earth science, and few had taken earth science seriously on its own terms.

In (1985), the ground shifted with the publication of Martin J. S. Rudwick's *Great Devonian Controversy*. Here was a work that addressed the epistemic question of how geologists established a fact about the natural world, and how they did so without the aid of laboratories and nearly without the aid of instrumentation, save a rock hammer that scarcely differed from a workman's tool, a rudimentary hand lens that any amateur could afford, and a notebook. Rudwick's work charted a new academic landscape, and since that time scholars have increasingly recognized that the earth sciences—geology, geophysics, oceanography, meteorology—are substantively different from mathematics, physics, and chemistry. Their work is done out of doors and not in a laboratory, their goal has been primarily to describe and explain the past rather than to test theories by predicting the future, and they increasingly rely on computers to simulate the natural world, but not replicate or reproduce it. And these differences have social and epistemic consequences.

Rudwick's work was crucial to my own career, because as geologist in the process of metamorphosis into historian, it was painfully obvious to me that the "model" of science about which my colleagues in philosophy argued passionately was at best incomplete–based as it was mostly on physics with some nods in the direction of biology. It missed completely many interesting and important questions

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raised by the earth sciences about replicability, incompleteness, scaling, deduction and induction, prediction and more.

It was just at that time—in the mid-1980s—that Paul Edwards became interested in the earth sciences as well, in his case meteorology and climate modeling. And perhaps this was no coincidence. Because, as Edwards amply demonstrates in this book, the earth sciences are profoundly important, not only because they challenge conventional philosophical portraits of how scientific knowledge is produced, tested, and stabilized, but also because they matter for the future of the *world*. We now know, largely thanks to climate scientists, that the world of the future will not be the same as the world of the present. Humans have altered the chemistry of our atmosphere in a consequential manner, something that earth scientists anticipated, and, it now appears, accurately predicted. Francis Bacon famously claimed that knowledge was power, but in the case of climate change, scientists have achieved remarkable knowledge about a complex, natural phenomenon—anthropogenic climate change—yet appear to be nearly powerless to convince the rest of us to do anything about it. Understanding that science is crucial for all of us who care about understanding the power of science and its limits.

Edwards's book is a game-changer. After this, it simply will no longer be possible to ignore or dismiss the earth sciences as 'special sciences' and therefore not so urgent to study as the ordinary sciences. Anyone who reads this book and takes its arguments seriously will immediately see that that the past century of work on computer models, climate data, and global warming—to use Edwards's subtitle—cannot be understood as an application of "more basic" sciences. This is so for two reasons—one social and one epistemic—and their explication constitutes the major part of the substance of Edwards's book.

A central argument of *A Vast Machine* is that climate science is, well, a vast machine. By that Edwards means that, like any machine, it had to be *built*. And, like any machine, it had to be adjusted, improved, and fixed based on the experiences of attempting to make it functional. By this Edwards does *not* mean socially constructed, in the sociological sense that has distressed so many philosophers and scientists in seeming to require an anti-realist epistemic stance toward the knowledge produced. He means constructed in the rather more literal sense of built, physically, from parts. These parts included data, models, and the infrastructure needed to produce the data and build the models.

Edwards is particularly interested in data. A good portion of the book is dedicated to unraveling the process by which meteorologists were able to get the data they needed, both to make weather forecasts and to build simulations of the global climate system. Getting these data was anything but straightforward. First, Edwards notes that scientists had to *make global data.* They had to compile an unfathomable number of individual measurements—of temperature, humidity, barometric pressure, and other variables—into a coherent collection of commensurable and therefore usable numbers. Weather data are always collected locally, but they are not much use in that form. In meteorology, data become useful when they are made global.

To make data that could be used to predict weather and understand climate, local data collectors and national weather services had to improve and standardize their observing systems. They also had to find ways to communicate information across national and international networks. The history of meteorology is thus closely linked to the history of telecommunications.

Moreover, in many areas of the world—over the oceans, in remote areas of less industrialized nations, in polar regions—very few observational data existed. Yet, to build an accurate weather or climate model, one needs data over the whole globe, not just part of it. So scientists developed techniques for creating data where it did not exist through interpolation, or modeling. A system emerged where a substantial portion of the "data" in models was not actually data at all—in the conventional understanding of the word—but was generated through modeling. Edwards (188) explains: "As time went on, these techniques [of observation and modeling] became so tightly intertwined that they transformed the very meaning of the term 'data'…Virtually everything we now call 'global data' is not simply collected; it is checked, filtered, interpreted, and integrated by computer models".

Edwards describes in convincing historical detail the enormous amount of thought and labour that went into this process. Among other things, it required the creation of a "climate knowledge infrastructure"—systems for observing the weather, recording the variables measured, linking the data to each other within any one system, and then between systems, linking data across relevant scales, and sharing these data across time and space to enable the production of synoptic forecasts and global circulation models. None of this was easy or trivial; all of it required the development of new social systems, new organizations, and new ways of recording and sharing information. In some cases, these systems were somewhat loose, such as the International Meteorological Organization, which worked in the 1930s to promote data standardization and exchange but without power to enforce those standards or require that exchange. Later, the systems became more formalized, as in the 1948 World Meteorological Convention, whose 31 signatories agreed to link national weather data reporting systems into a global data collection and processing system. In the 1960s, the World Weather Watch linked the ground-based weather systems to the data from newly built weather satellites and ocean-observing systems. These systems required work to build them, and work to create and sustain them.

Work involves the transfer of energy, and the transfer of energy invariably leads to friction. Edwards introduces the notion of "data friction" to characterize the energy losses incurred. This is a crucial point, because it leads to one of Edwards's most important epistemic claims: that none of this can be characterized in any way that is remotely adequate by the phrase "data collection". To be sure, data *were* collected. A man, woman, or child reading a thermometer at a weather station is collecting data in a traditional philosophical sense. But that is only just the tiniest fraction of the story that Edwards tells.

Once data were made commensurable, so that they could be compiled into global data sets, then the entire process was inverted. From the 1930s to the 1960s, scientists worked to make *global data*—that is to say, to have sufficient data from sufficient locales to meaningfully measure and represent the world weather and climate systems, and in sufficiently compatible forms as to be commensurable, and therefore usable. This, it was understood, was a prerequisite for accurate weather forecasting, because weather systems *moved*, and without data from the places that weather was coming from, one would never be able to make consistently accurate predictions. This, Edwards argues, was essentially a bottom-up process, starting from each and every location on Earth where a parameter was measured. But from the 1960s onwards, scientists also became concerned about making *data global:* that is to say, with making complete, consistent, and coherent data sets that represented the climate system as a whole, rather than being focused on weather in one particular place.

The imperative to make data global arose initially from the same demand that led to making global data: numerical weather prediction. As Edwards explains, the models used for numerical weather prediction require values at every grid point, to avoid crashing the simulation. But, as we have already noted, many areas of the globe lacked observational data. Initially, missing grid-points were interpolated by hand or entered using averages or norms for that place on earth at that time of year. But modelers soon realized that they could use the models themselves to generate "data" for the missing points. They did this by using data generated by the last model as input into the next one. For data-sparse regions, modelers soon found that this technique often worked better: the modeled data were more accurate than the interpolated, averaged, or best-guessed data. So the boundary between data and model began to blur, as the output from one model became input to the next. Evidently without irony, this form of data input came to be called "objective analysis."

"[T]he data images they created often proved more accurate than many of the observations on which they were based, at least in data-sparse regions," Edwards notes. "By the 1980s, computerized data assimilation systems routinely generated consistent, complete, uniformly gridded data for the entire Earth. These data [sic] became the most accurate available images of the planetary circulation over short periods of time, so climate scientists began to use them for general circulation studies". That is to say, climate scientists began to use the modeled data—or data models—or simulated data—as the basis for their studies of the general circulation.

In some sense, then, the *global model* became the object of study—rather than the globe itself. Edwards (253) concludes, "Ultimately, these techniques transformed the very meaning of the word 'data' in the atmospheric sciences." We might also add that it changes the meaning of 'model' too, as models now both embed and create data. The boundary between "data" and "model" becomes blurred and no longer maps onto the traditional distinction between "observation" and "theory" (if it ever did). The model is theoretical, but it is not pure theory, and the data embed observational information, but they are not just observations.

This discussion leads Edwards to consideration of what he calls "data analysis models" or "data models" for short: a family of mathematical techniques used to process historical temperature and climate records into a coherent picture of the global climate system, and how it has changed over time, including in response to anthropogenic forcings. These models are crucial to our understanding of the climate system, and to the scientific conclusion that human activities have now altered it. A crucial part of this work is data re-analysis: the re-examination of old records and the work of putting those records into commensurable formats. Once one appreciates the stupendous work involved, it makes it clear that any demand to see the 'raw data' that goes into General Circulation Models must be either malicious or ignorant.

It is this argument—about the blurred boundary between "data" and "models" —that leads to the most important, and perhaps provocative claim of the book: that it is models all the way down. That is to say, there is a sense in which data, as traditionally understood, no longer exist.

Edwards expresses this idea in a few different ways. "Without models, there are no data". "Everything we know about the world's climate—past, present, and future—we know through models". "If we cannot trust models without evidence, neither can we trust evidence without models". He does not conclude from this that climate science is unreliable, but rather that it is, quite simply, "the best knowledge we are going to get" (439). He might also have added that, given the colossal work that has gone into creating it, it is the *only* knowledge we are going to get.

For some time, scholars of science—and indeed, most scientists who have given the matter even a modicum of thought—have understood that "raw" data—unmediated sense impressions—play very little, if any role, in contemporary science. If any one was not yet convinced, Edwards's work should surely settle the matter. He has marshaled a huge amount of evidence—and done an enormous amount of work—to display before us the work that goes into building the data sets that make modern climate science possible. I doubt anyone reading this book with an open mind could be left unpersuaded by this central claim.

But in the process of convincing us that the "data" of climate science are themselves modeled, and that models are themselves an important source of data, Edwards leads us to a crucial question that remains unanswered, indeed, unposed. It is this: if all climate data are modeled, then how can we evaluate their *relative* reliability?

Edwards explains in good detail the various approaches and techniques used to build global data sets and make data global, but surely these techniques are not all equally good? He concludes that we must accept climate science because we frankly do not have any alternative, and I have argued along similar lines (cf. 2004, 2007). But to argue that all data sets are modeled in some way, and that models are also used to produce "data sets" of a certain type, is *not* to show that all data are equally valid, equally reliable. It is technically true that all climate data are modeled—in the sense that *all* data in science are modeled, insofar as theoretical assumptions are built into the instruments used to collect those data, to reduce the raw data, etc. But we might also argue that some data are more thoroughly modeled than others.

Consider for example, the Vostok ice core data, which provide our longest time series data on past temperatures, now stretching back nearly one million years. These data are our most important set of information on natural variability of the Earth's climate in the recent past and therefore a central basis of the claim that the observed recent increase in global mean temperature exceeds the envelope of natural variability associated with non-human climate forcings, including those that drove the ice ages.

These data are modeled, in the sense that scientists do not insert thermometers into a layer of old ice to determine the temperature 457,000 years ago. Rather, they measure the oxygen isotope composition of that ice, and from that, and knowing the temperature dependence of oxygen isotope fractionation, deduce the prevailing temperature at the time the ice formed. In an important sense, the temperature "measurement" is a model, based on an actual measurement of oxygen isotopes. But this is a weak sense of "model", insofar as it arguably applies to all data in modern science, and perhaps even all data in science, *tout court*. And yet this weakness is important, because many scientists would argue—and this historian would agree with them—that the Vostok ice core information is more reliable—less uncertain—than the projections of future mean global temperatures produced by General Circulation Models.

This distinction matters because, as Edwards notes, many of those who reject the scientific evidence of anthropogenic climate change do so on the basis that it is based "only on models". Edwards's response is to say that models are all you get, so you'd best get used to it. While I understand and sympathize with that response, it strikes me, in the end, as inadequate. For the track record of numerical simulation models in other domains is not so good—as the recent financial crisis made only too clear (cf. Oreskes and Belitz 2001). To argue that models are all we have is to argue that our knowledge is, in fact, rather insecure. And I think this conclusion is incorrect.

As I have argued elsewhere (2007), there is now an enormous diversity of evidence in support of the hypothesis—articulated more than a century ago—of anthropogenic climate change, and much of this evidence can be evaluated according to traditional philosophical norms. Edwards may have not meant to suggest that because all of the data are modeled, they are all equivalent. Indeed I suspect he did not intend to suggest that. Yet we are left with that impression, and given no handle on how we might find a path away from it, and therefore no firm sense of why, in the end, we might decide to accept the conclusions of our scientific colleagues, whether or not it is models all the way down. And the question of the reliability of climate science—and indeed, of all the sciences formerly known as 'special'—remains important, both epistemically and socially.

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