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Cartwright's THE DAPPLED WORLD: A STUDY OF THE BOUNDARIES OF SCIENCE

SUBLUNARY CLOCKMAKERS

Kevin D. Hoover

A review essay on Nancy Cartwright's, *The Dappled World: A Study of the Boundaries of Science.* Cambridge University Press, 2001. Pp. ix, 247. Cloth $54.95; paper, $19.95.

Since the publication of *How the Laws of Physics Lie* in 1983, Nancy Cartwright has been in the forefront of a movement in the philosophy of science aimed at reforming the vision of what it is that science does and what it achieves when it does it. Cartwright is a realist; but in that early book she argued that the laws of physics are, at best, instruments. They systematically distort reality (they "lie") in order to do their work. Contrary to the understanding – popular even among scientists themselves – that they describe the deepest reality, the laws are best thought of as tools. The scientist is a kind of skilled craftsman who uses them, just as he uses other tools and materials, to manipulate and probe reality and, sometimes to learn something about it.

It was, at first, easy to think that Cartwright was just another instrumentalist. But to deny the reality of laws was not to deny realism. In *Nature's Capacities and Their Measurement* (1989) she offered a constructive account of reality. The key notion was the *capacity*. Aspirin has the capacity to cure headaches because sometimes, in the right circumstances, it does in fact cure headaches. There is no law that says aspirin cures headaches. It does so in some cases, and not in others. Those cases may, in fact, be very special. A capacity is a

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real potentiality of something in the world that it carries with it from circumstance to circumstance. Capacities compose and interact. The regularities that we see in the world are generally not laws, not universal truths, but relatively transitory expressions of capacities interacting in special circumstances.

A striking – and rather unexpected – feature of *Nature's Capacities* is the use that Cartwright makes of econometrics. She sees the goal of the Cowles Commission program as the measurement of the strength of capacities through statistical models that articulate causal structures – that is, through models that trace the interactions of capacities that produce observable regularities. While it must warm the hearts of economists – so used to viewing their discipline as a poor stepchild to genuine science – to see its methods applied to quantum physics, it is only the statistical methods and not the substance of economics that caught her attention in *Nature's Capacities*. In the course of the next ten years, however, Cartwright has been engaged in a research program with several collaborators in which economics, as well as physics, is the object of study.

To those familiar with the work of the research group on Modelling and Measurement in Physics and Economics at the London School of Economics and with the string of papers produced by Cartwright and her collaborators, the current volume, *The Dappled World*, will seem familiar. It does not break new ground, but develops and elaborates old themes. And it is a dappled production itself – a patchwork of papers written for other occasions, stitched together to be sure, but with the seams still easily seen.

I

In *The Dappled World*, Cartwright takes two strong stands – one negative, one positive. She is against universalism. Whatever science has learned about the way the world works, it has learned in particular contexts. While science may know a lot that is true, it remains, she argues, a long, and rarely justified leap, from true-in-one-domain to true-in-every-domain. She rejects the picture of science as a hierarchy with applied sciences at the base (and social sciences perhaps in the sub-basement) and physics at the peak of a pyramid. Unlike in her previous books in which law-talk was consciously downgraded, Cartwright now seems rather at ease with laws. She takes it as read that their status is far less exalted than when she first accused them of lying. She now talks of a "patchwork of laws." Her title refers to a poem by Gerald Manley Hopkins: "Glory be to God for dappled things – /For skies of couple-colour as a brindled cow;/ . . . All things counter, original, spare, strange;/Whatever is fickle, freckled (who knows how?)" True to Hopkins's vision, Cartwright sees beauty in variety, not in uniformity. And she sees science as a variegated enterprise that knows

lots about many things, rather than a reductionist enterprise with a feasible goal of one theory of everything.

Cartwright also supports a positive thesis: the world is best thought of as constructed from things with specific capacities or – to use Aristotle's terminology, as she does in the current volume – specific *natures*. Capacities or natures are properties abstracted from the objects that carry them. They are what the objects are always trying to do. It is the nature of my sister-in-law's goats to escape their enclosure. How that nature plays out in practice depends on the expression of other natures (such as their nature to regard poison ivy, which happens to grow in the enclosure, as a delicacy or the nature of the electric fence to shock).

It is tempting to gloss the notion of a capacity as nothing more than an underspecified ceteris paribus condition: that is, what the thing would do if there were not some interfering factors. But Cartwright is after something else, a central element of capacities is that they compose. They are not expressed only when there are no interfering factors, but are expressed all the time. The outcomes are complex interactions of capacities that, on the one hand, mean that capacities are rarely observed directly, and, on the other hand, explain the production of particular local regularities. The various capacities of the parts explain the actions of the machine.

The machine metaphor is important for Cartwright. She argues that regularities are few and far between and are produced only by what she terms *nomological machines* – i.e. machines that make laws. Here "law" refers not to something universal, but merely to a reliable, robust regularity. It is only in special nomological machines – for example, in the apparatus of a laboratory experiment – that capacities can be made to reveal themselves in fairly pure forms.

The laws that appear in highly developed scientific theories – e.g. Newton's second law of motion – are, on Cartwright's view, best seen as abstract descriptions of capacities. *Force* = *mass* × *acceleration* really says, if anything is a force applied to another thing that is a mass, then it has the capacity to accelerate. Only in the very special circumstances of a laboratory experiment or of astronauts standing on the moon, in which the mass is shielded from, or compensated for, the expressions of other capacities will $F = ma$ be observed directly in a pure form. Nevertheless, it describes accurately the way different forces and masses act and may, for example, aid us in designing a machine. The key thing is the abstraction must be made concrete. $F = ma$, tells us little about the world. But, if we can say of gravity that GMm measures its force, or how to calculate the force acting between two charged masses or the force of wind resistance and so forth, then the capacity expressed by $F = ma$ tells us one of the concrete tendencies of the mass in question.

Cartwright fleshes out the relationship between the abstract and the concrete in a manner that has considerable resonance for economists. At the height of the two-Cambridges controversy in capital theory in the 1960s, Robert Solow suggested that the aggregate production function should be viewed as a *parable*: it did not literally describe production, but it acted as an analogy that carried an important message about what production was like. For Cartwright a scientific law, like Newton's second, is not an analogy or parable, it is a *fable*. Cartwright draws on the account of fables due to Gottfried Lessing, the German Romantic poet and playwright. For Lessing, the fables of Aesop and others, do not say that the story is *like* something in our lives. Rather the moral of the fable is an abstract, but precise, description of an aspect of the story. The fable shows us how to move from the abstraction to one concrete expression of the moral. Its utility lies in our ability learn from the tale about animals how to concretize the moral. It might then be concretized in ways that apply to ourselves or other people. For Cartwright, $F = ma$ is the moral, Newton's theory of gravity or Columb's law of electrical interaction or the theory of wind resistance are concrete instances of the moral. Scientific textbooks are essentially books of fables that provide edifying stories of how the moral is exemplified. The student learns how to apply the moral to situations not contemplated directly in the book.

Scientific theory, according to Cartwright, is a system of abstractions – a book of morals. Such abstractions are not automatically tied to the world. The substance of science is found in the varieties of ways in which the abstractions are made concrete. In large part, it is found in the construction of nomological machines. Such machines exemplify various scientific morals. Much of scientific analysis, on this view, takes place not at the level of highly abstract theory, but at the level of models. For Cartwright, a model is the blueprint for a nomological machine. It is a plan for making abstractions concrete. Nomological machines produce particular regularities – a different machine for each regularity. Cartwright's work speaks to economics; for economics is a discipline devoted to modeling.

Since in the remainder of this review I shall be trying to highlight points on which I disagree with Cartwright, I would like to conclude this section by stating my general agreement with her perspective in *The Dappled World*. Cartwright's earlier work up to, and including, the earlier versions of many of the chapters in *The Dappled World* have greatly influenced my own thinking about the philosophy of science and particularly how it is applied to economics. On most of the larger points, I think that she is fundamentally correct. Still, there are important points on which I think that she draws the wrong lessons – especially for economics.

II

Although trained in physics, Cartwright has the soul of engineer. For her, regularities are rare, they are the products of carefully designed machines. In the famous Argument from Design, God is shown to exist on the premise that the world is a machine of exquisite complexity (and beauty). A wonderful clockwork must have a Divine Clockmaker. Cartwright adopts a secularized version of the argument. Instead of a single machine, there is a variety of machines that produce infrequent regularities in a vast sea of irregularity. Each regularity, she concludes, has its clockmaker. Only exceedingly rarely does there occur a natural clock, a nomological machine that occurs without human intervention. The solar system is, perhaps, one such natural machine. But generally, regularities occur only where they are consciously designed to occur.

But is this so? Are regularities really that rare? Clearly not. We rely on regularities constantly – physical and social. I know that there is a regular connection between the amount I depress the accelerator pedal of my car and the speed that it obtains. I know that other drivers generally keep to the right side of the road (at least in the United States). I know that the majority of enrolled students will attend my classes at the announced times.

Although much of her work is aimed at reducing the pretensions of scientific laws, Cartwright still carries the ghost of the physicist's ideal of precision. In order not to deny (absurdly) the reality of the coarse regularities on which our lives literally depend, Cartwright draws what I believe to be an untenable distinction between such quotidian regularities and the precise regularities of science. Far from being exceedingly rare, regularities are exceedingly common. They are – to be sure – highly circumstantial and local. They do not come close to being laws of nature – yet we rely on them constantly. And so must every scientist – not only in ordinary life, but also in the life of the laboratory.

For Cartwright, the laboratory scientist is a builder of nomological machines. But how are they built? There is an unbroken chain from the stone hammers and knives of the Neolithic age to the emerging nanotechnologies. The exceedingly precise has been built out of the imprecise; the refined regularities of the laboratory depend on the robust regularities embodied in glass, lenses, machined metal, drawn wire, silicon, and other materials. Nomological machines are constructed out of other machines, or at least out of components, some quite ordinary, others quite special, whose capacities are known and instantiated in the ubiquitous regularities known to skilled and unskilled craftsman. Such workaday regularities and their role in knowledge stand in as much need of philosophical and methodological attention as the refined regularities produced in tightly controlled experiments.

Cartwright's insistence that the regularities of interest must be precise and, therefore, correspondingly rare seems unnecessary to her general world view. It seems partly vestigial – the result of her background in physics – and partly the result of a shift away from the epistemological focus of *Nature's Capacities* towards a concern with the ontology of natures (or capacities). Whatever the reason, the insistence that regularities must be precise and rare colors Cartwright's understanding of econometrics, its scope and limits.

III

Cartwright's method of analysis is often to draw an expansive lesson from a small number (sometimes just one) example. It is extremely useful to work with concrete cases, but there is a risk of overgeneralization. In the case of econometrics, she chooses Anand and Kanbur's (1995) cross-country regression study. Why this study is taken to be prototypical is unclear. It investigates the effect of public investment on social outcomes in developing countries.

Her objection to the study is of a piece with her stress on design as essential to producing (rare, precise) regularities. Modern econometrics is grounded in the theory of probability. But for Cartwright, the world cannot be *generally* characterized probabilistically. Probabilities are not there for the taking, but are characteristics of quite particular set-ups (e.g. of roulette tables or particular configurations of unstable atoms). Only in such designed set-ups do objects display well behaved probabilities. Cartwright maintains that the political and economic differences among the countries in Anand and Kanbur's study render them clearly insufficiently homogeneous to be regarded as the nomological machines generating economic data belonging to the same probability distribution. One cannot in principle learn anything from such a study. No argument or evidence is offered for the lack of homogeneity on relevant dimensions. It is an ~~*obiter dictum*~~. The particular study, however, is not the issue. The lesson that Cartwright draws condemns a large swath of applied econometrics as completely useless.

I have no interest in defending or condemning Anand and Kanbur's particular study, but it seems to me that Cartwright's general attack is misdirected.[1] First, she seems to forget a point emphasized in Mary Morgan's *The History of Econometric Ideas* (1990) and cited in *Nature's Capacities*, that econometrics aims in part to provide a substitute for controlled experiments. Its techniques aim to account for salient differences between cases (in Anand and Kanbur's case, between countries) in order to render the residuals sufficiently homogeneous for informative comparison. Statistical controls take the place of shielding and compensation mechanisms to permit an underlying shared capacity to be

clearly observed. Second, she adopts a vision of econometrics grounded in Haavelmo's and the Cowles Commission's program of structural econometrics. She sees the goal of every regression as the articulation of the detailed structure of the nomological machine. Few econometricians would agree. Even those who retain an interest in structural estimation are well aware that much of econometrics consists of tools for systematically identifying high level regularities, the structural account of which requires further study. Finally, her point seems to be fatally damaged by her concession that a valid cross-sectional study might be possible if health care plans were distributed across countries by lottery. Much recent cross-sectional econometrics has addressed the question of how to exploit undesigned variations as if they had the sort of design that Cartwright requires. It may not always be possible to use observed data in place of experimental data, but that is a matter of the particular case and not of general principle.

The attack on econometrics is part of a surprising disregard for the epistemological problems of establishing what are true regularities – of any degree of precision or universality. On the one hand, the statistics that often form the best basis for uncovering the regularities are held to be suspect. On the other hand, Cartwright claims to know particular capacities almost by direct acquaintance. How do we know that aspirin has the capacity to cure headaches? She often writes as if it were obvious from experience. But experiences of the type I-took-it-and-I-got-better equally well support that laetrile cures cancer or that copper bracelets cure arthritis. When Cartwright states "I know . . . that feeding the hungry and the homeless will make for less misery; and that giving more smear tests will lessen the incidence of cervical cancer" (p. 23) or that the Chicago economists carry rational expectations too far or that the IMF uses inappropriate models to argue for the reduction in direct welfare expenditure in the third world (p. 18), we are entitled to ask on what basis she is so sure.

All these claims involve either the establishment of important regularities or the demonstration that a model cannot generate observed regularities. Econometrics and other statistical techniques aim in part to sort out the reliable from the unreliable regularities. It is not just that Cartwright does not tell us the evidence that convinces her, so that someone unsympathetic to her beliefs might conclude that they were wishful thinking or political predisposition, it is that she launches a pretty indiscriminate broadside against methods that aim to resolve just such questions. The methods deserve scrutiny. But rather than scrutinizing them for their efficacy in concrete cases or even on more general epistemological grounds, she aims to dismiss them as out of keeping with sound ontology. Anand and Kanbur, in fact, conclude that the cross-sectional evidence does not allow them to identify a key causal mechanism, but that is something

that they learned from using the tools of econometrics to process data, and not something that they could have legitimately concluded from prior metaphysical analysis.

What, in the end, I find most puzzling in Cartwright's attack on applied econometrics is that it is in no way implied by her general framework of the dappled world. The systematic investigation of such a world needs tools such as those the econometricians attempt to develop and employ. A more sympathetic reading of the practices of applied econometrics would, I believe, reveal a good deal of common ground between Cartwright and applied econometrics.

NOTE

1. Hoover (forthcoming) provides detailed criticism of Cartwright's views of econometrics, including the particular case of Anand and Kanbur's regression study.

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Sciabarra's TOTAL FREEDOM: TOWARD A DIALECTICAL LIBERTARIANISM

Paul R. Diesing

A review essay on Chris Sciabarra's, *Total Freedom: Toward a Dialectica Libertarianism*. University Park: Pennsylvania State University Press, 200(467 pp. ISBN 0-271-02048-2, $65.00; 0-271-02049-0, $24.50.

This is a historical work that describes the development of libertarian though moving from the slightly dialectical thought of 19th century Spencer an Menger, through the Austrians such as Hayek and Mises, to the recent mov to dialectical thinking. Sciabarra wishes to promote this move by describin dialectic and showing how it is changing and can change libertarian theory.

The first half of the book describes dialectic. First, dialectical thinking deal with how things change over time (p. 141). Consequently Sciabarra's elucidatio of dialectic is also historical; it shows how dialectical thinking developed fror Plato and Aristotle to the present, and how it produced manifold variations. I began as dialogue, discussion, during which thoughts developed, and late included interpretations of earlier dialogues and texts. The texts thus becam more complex and varied in their implications. Later it became apparent tha societies also change over time, through their own dialectical processes Consequently dialectical thinking could also be a research process to uncove the underlying dialectic of social change. And so on.

Several themes recur through this history of dialectic, and these themes ten to become central characteristics of dialectic in Sciabarra's interpretation. Th

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