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Discussion

Structure and Domain-Independence in the Formal Sciences

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There is much less disagreement than at first appears between Kevin de Laplante's 'Certainty and Domain-Independence in the Sciences of Complexity' (de Laplante, 1999) and my article of which it is a critique (Franklin, 1994). De Laplante does not deny that *some* cases of reasoning in the formal sciences, such as the Königsberg bridges example, may lead to practical certainty about the real world, and he labours to show that certainty of that kind is not available in many other cases. The original article conceded that there were indeed many cases where certainty was not possible, but insisted there were some cases where it *was* possible to attain provable certainty about actual parts of the world—this being thought to be the conclusion of most philosophical interest, and dubbed the 'discovery of the philosophers' stone'.

Where there is some residual disagreement is over the rarity or typicality of cases where certainty *is* attainable. While I concede that in cases like complex ecosystems certainty is not attainable, and for the reasons de Laplante explains, I would not agree that cases like the Königsberg bridges are vanishingly rare. For one thing, de Laplante has chosen to relegate to a footnote the other kind of example I suggested, surveyable computer programs, whose logical structure is open to inspection. Despite the best efforts of Microsoft to make all computer programs so large as to be incomprehensible, small surveyable programs are still common items.

Whether a given piece of reasoning in the formal sciences produces certainty must, then, be evaluated on a case by case basis. Nevertheless, de Laplante advances two general arguments which would, if correct, suggest that certainty is rare. These arguments, it will be maintained, are not correct.

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The first argument is a new version of the old claim that applied mathematics (always or usually) deals with a simplified or idealised model of a real situation, not with the thing itself. ‘But in the majority of realistic modelling situations,’ de Laplante says, ‘the models involved are simplified abstractions of the real system, and strict isomorphism between the model and the physical system is impossible to establish.’ One thinks typically of modelling coins by perfect Euclidean circles, and using that model to calculate their area: the answer will not be exactly true.

The flaw in this reasoning is that it does not appreciate that modern mathematical models are ‘structurally stable’ (Franklin, 1983), that is, their (qualitative or approximate quantitative) predictions are insensitive to small changes. A circle is not structurally stable, in that a slightly deformed circle is not a circle. But the system of Königsberg bridges retains exactly the same topological structure if its islands are eroded slightly, or its river narrows. Similarly with the predictions of typical chaotic dynamical models: the qualitative predictions of the model do not change at all if the inputs or parameters vary slightly. It follows that accuracy of measurement of the inputs or parameters is not needed for certainty of the predictions. In a particular case, one will need to know something about how robust the model actually is to changes—but that is a purely mathematical fact about the model, itself knowable with certainty.

De Laplante further argues that typical theories in the ‘formal sciences’ are really physical theories, and hence subject to the usual uncertainties of physical—as opposed to mathematical—theories. What ‘domain-independence’ they have, he concludes, is not due to their formal or mathematical properties. He instances the ‘dominance of indirect effects’ phenomenon of network ecology: that as a network of flows becomes more complex, the ratio of the magnitude of indirect to direct flows increases. This result, he says, is not a purely mathematical one, as it refers to physical entities like flows, cycling and interactions; it is about ‘a *physical* network of flows of material or energetic substance’.

That is not correct. It no more follows that the phenomenon is ‘really physical’ because it can be realised in a network of flows than it follows that Euler’s mathematical result on topology is ‘really physical’ because it was realised in the network of physical bridges in Königsberg. One might as well argue that since two angels and two angels make four angels, ‘2 + 2 = 4’ is essentially a theological proposition. The question is whether there is a purely mathematical theorem of ‘dominance of indirect effects’, which can be realised in a physical network which satisfies the conditions of the theorem. In the reference which de Laplante gives (Higashi and Patten, 1989), the reasoning proceeds entirely in the context of directed graphs with connections labelled by numbers, and the reasoning itself is purely mathematical. The interpretation of the numbers on the connections as flows and the summations of them as effects lies outside the reasoning. The situation is very similar to Gauss’ Divergence Theorem, which is most naturally stated in terms of the flux of a fluid across a surface, and is therefore often claimed by physicists working in either fluid flow or electricity, but is really a purely mathematical theorem about vector fields (Bourne and Kendall, 1992, p. 195).

The lesson is that one must take care when dividing a theory into its physical and mathematical (or formal, or structural) parts, and that one will typically find that more lies on the side of the mathematics than is usually believed. This is becoming increasingly recognised, both in recent philosophies of mathematics that emphasise structure (Devlin, 1994; Maddy, 1997; Resnik, 1997; Shapiro, 1997) and in work that shows the preservation of the structural content of physical theories through apparent revolutions (Ladyman, 1998). What remains to be decided in each case is whether the non-mathematical, purely physical, residue of each theory is something that can be known with certainty. The residue is frequently so small that it can.

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