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Response

‘Limited but Useful’: Datafied Brains and Digital Twins

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I very much appreciated the thoughtful response to my *Datafied brains and digital twins: lessons from industry, caution for psychiatry* provided by Douglas W. Heinrichs. I am encouraged that we differ merely in emphasis among the issues upon which we share a wider understanding.

In his response, *Assessing the Dangers of the Next Reductionist Fantasy*, Heinrichs elaborates upon an underemphasized dimension in my *Digital Twin* article. Heinrichs approaches this dimension through “a semantic understanding of scientific theorizing.” According to this understanding, all science is fundamentally concerned with model making, and testing of models whose efficacy is evaluated on an adaptable basis relating to factors including accuracy and utility. Heinrichs’s emphasis upon this semantic understanding is most welcome, and expands on my own piece very helpfully.

For my part, I am in accord with this approach to understanding scientific endeavors. I would cite the examination of causality provided in Ismael’s (2016), that delivers a similar account to that put forward by Heinrichs. Ismael too is interested in the nature of the models put forward in scientific theorizing. She says of the logical structure of causal relationships,

> They are not simple relations between pairs of events, but are relativized to networks, [and] that the asymmetry of causal relations (what we think of as the direction of influence) was imposed by the choice of exogenous and endogenous variables, and that it can be reversed by making different choices. Which networks we are interested in, and which variables we treat as endogenous and exogenous, are determined by context and purpose. …From the point of view of physics, it is the rules that govern components that are basic, and causal pathways are emergent regularities that can be used as strategic routes to action. (Ismael, 2016 p. 136)

The choices, contexts, and purposes that emerge as routes to action include the ways in which we can carve out of reality the objects and phenomena with which we are interested at some point in time. From these points in time we can draw conclusions about those objects and phenomena, as if holding fixed all other variables and concentrating only upon those that interest us. The practical but relatively abstract unity of events constrained by such a model allows us to represent a manageable portion of reality that we can better understand it. This coheres with my talk of crypto-induction masquerading as deduction. The former is the standard acceptable and expected within model making, the latter too grand a mantle for it to bear.

*The author reports no conflict of interest.

Digital twins and bridges or epileptic seizure devices, use such models in order to ground strategies to aid in achieving goals. An inductive approach ranging across limited but relevant variables will suffice here because the variables chosen are adequate for the well-defined purposes. Where greater complexity is at stake than bridge maintenance, or where wider goals than instrumental interventions on an electrically overstimulated brain are on the table, the model making ought to respond in kind to meet the need. What 'will suffice' becomes less obvious, and maybe less tangible.

Especially where human beings and choices are at stake, the task of model making becomes enormously arduous. This is not least owing to the role of decision-making in human behavior. Again, Ismael is useful here as she discusses control:

> There is no simple, general relationship between environmental stimulus and behavior, no fixed structure that can be exploited like the levers on a toaster to bring them under our control. You can make frog tongues flick and change the direction of a school of fish by producing the right stimulus. You can make cells secrete and dogs salivate and flowers bloom by producing the right stimulus. But it is very hard to control the voluntary behavior of another human being, because choice effectively randomizes the connection between stimulus and response. (2016 p. 96)

Holding variables as fixed is at best made bafflingly complex where choice is present, and with it modeling too. Where narrow inductive inferences are licensed by choices among variable held fixed, we can come up with strategies to achieve goals. Where choices are possible, this is less clearly the case. Any induction will need to be made form an array of variables vastly outnumbering those relevant to bridges and schedules. Nothing will approach a model of the whole, I suggest. Where choice itself may be part of what is at stake, as it might be in clinical evaluation or treatment for psychiatric conditions, issues are compounded.

Where psychiatric disorders are at stake, there is a variety of depths and levels of complexity. Personal value judgments, social norms, politically garnered norms of public health, for instance, ought to be drawn upon. If the strategy is to alleviate psychiatric suffering, or to promote nonpathological behaviors, or to allow user control of the brain, modeling ought to be vastly complex. This will require accounting for healthy variation, pathological, and other behaviors, as well as the neural basis for them. This is an undertaking not available to even the most complicated neural recording paradigm, as I see it.

Part of my use of the schematic outline of brain data generation can be seen as pointing out a too parsimonious variable-fixing. The apparent recording of brain signals as representing neural activity does not present in data the complex mapping of neuroelectrical activity it appears to – it includes elements of circularity which are limiting to any model that introduces distance from the brain it would model. Using this data model to try to substantiate a 'normal' to which any given brain ought to be calibrated misses out everything to do with the complexity of choice.

Heinrichs adds to this the idea that, anyway, a neuroelectrical model wouldn't suffice. The holism of the brain would make nonlinear reactions to interventions the norm, rather than an exception. Faith in the chances for a whole-brain modeling strategy might be an instance of what Heinrichs calls "the latest reductionist fantasy." In my way of putting it, the induction licensed by any model comes from too small a set of variables, and consequently the garb of deductive inference is too ostentatious. My concerns emerge from efforts to justify the quasi-deduction rather than explaining the usefulness that there is in the induction.

For my part, Heinrich's use of the phrase 'useful but limited' in the midst of his response, resonates. I wonder if that might be a good epithet for emerging reductionist models in general, especially where complexity is likely. Rather than hype and hope that the next big thing will be the silver bullet, assuming instead that the next model might be useful but limited could be a methodological improvement worth getting behind.

Reference

Ismael, J. T. (2016). *How physics makes us free*. Oxford: Oxford University Press.