43. Wallis, G. & Rolls, E. A model of invariant object recognition in the visual system. *Prog. Neurobiol.* **51**, 167–194 (1997).
44. Riesenhuber, M. & Poggio, T. Are cortical models really bound by the "binding problem"? *Neuron* **24**, 87–93 (1999).
45. Amit, Y. & Geman, D. A computational model for visual selection. *Neural Comput.* **11**, 1691–1715 (1999).
46. Bülthoff, H. & Edelman, S. Psychophysical support for a two-dimensional view interpolation theory of object recognition. *Proc. Natl. Acad. Sci. USA* **89**, 60–64 (1992).
47. Riesenhuber, M. & Poggio, T. The individual is nothing, the class everything: Psychophysics and modeling of recognition in object classes. AI Memo 1682, CBCL Paper 185 (MIT AI Lab and CBCL, Cambridge, Massachusetts, 2000).
48. Edelman, S. Class similarity and viewpoint invariance in the recognition of 3D objects. *Biol. Cybern.* **72**, 207–220 (1995).
49. Moses, Y., Ullman, S. & Edelman, S. Generalization to novel images in upright and inverted faces. *Perception* **25**, 443–462 (1996).
50. Riesenhuber, M. & Poggio, T. A note on object class representation and categorical perception. AI Memo 1679, CBCL Paper 183 (MIT AI Lab and CBCL, Cambridge, Massachusetts, 1999).
51. Hinton, G., Dayan, P., Frey, B. & Neal, R. The wake-sleep algorithm for unsupervised neural networks. *Science* **268**, 1158–1160 (1995).
52. Chelazzi, L., Duncan, J., Miller, E. & Desimone, R. Responses of neurons in inferior temporal cortex during memory-guided visual search. *J. Neurophysiol.* **80**, 2918–2940 (1998).
53. Haenny, P., Maunsell, J. & Schiller, P. State dependent activity in monkey visual cortex. II. Retinal and extraretinal factors in V4. *Exp. Brain Res.* **69**, 245–259 (1988).
54. Miller, E., Erickson, C. & Desimone, R. Neural mechanism of visual working memory in prefrontal cortex of the macaque. *J. Neurosci.* **16**, 5154–5167 (1996).
55. Motter, B. Neural correlates of feature selective memory and pop-out in extrastriate area V4. *J. Neurosci.* **14**, 2190–2199 (1994).
56. Olshausen, B. & Field, D. Emergence of simple-cell receptive field properties by learning a sparse code for natural images. *Nature* **381**, 607–609 (1996).
57. Hyvärinen, A. & Hoyer, P. Emergence of phase and shift invariant features by decomposition of natural images into independent feature subspaces. *Neural Comput.* **12**, 1705–1720 (2000).
58. Földiák, P. Learning invariance from transformation sequences. *Neural Comput.* **3**, 194–200 (1991).
59. Weber, M., Welling, W. & Perona, P. Towards automatic discovery of object categories. in *IEEE Conference on Computer Vision and Pattern Recognition* (in press).

Viewpoint • On theorists and data in computational neuroscience

A diversity of activities in neuroscience are labeled 'theory'. Developing Bayesian spike sorting algorithms, making a theory of consciousness, attractor neural network dynamics, constructing multi-compartment simulations of neurons, these and many other activities have a theoretical component. So of course there is a role for theory in neuroscience.

A question about the future of computational neuroscience can be bluntly put. Is understanding how the brain works going to be an enterprise in which pure theorists, scientists without experimental laboratories and not mere subsidiary parts of an experimentalist's laboratory, make essential contributions? Are independent theorists important to neuroscience? Important enough, say, to merit independent faculty positions in universities? Or will researchers doing experiments (or at least controlling experimental laboratories) make all the significant contributions, and be the only appropriate occupants of professorial positions in neuroscience?

The history of chemistry is the closest parallel. It is a subject in which both qualitative theory (the periodic table, the chemical bond) and quantitative theory (statistical mechanics, quantum mechanics) have been important. Modern quantitative theory and its impact on chemistry was brought forward by people who did not themselves do experiments, such as chemistry Nobelists Onsager and Kohn, whose ability in mathematics was key to understanding how to make new predictions and how to ground in understanding concepts that came qualitatively from experiments (in the areas of chemical bonding and irreversible thermodynamics).

Physics, geology, chemistry and astronomy have developed independent theorists when the breadth of these subjects exceeded the span of talents of a single individual. Within neuroscience I know no one who is both outstandingly able to perform inventive rat brain surgery and able to cogently describe modern artificial intelligence theories of learning and learnability. These are such different dimensions of expertise! Having both the talent and the time to span such a range is now impossible. Computational neuroscience is therefore in the process of bifurcating into theorists and experimentalists.

Sensible theory in science is rooted in facts, be they general or specific, so theory and experiment must interact. In physical science the development of a theoretical branch was at the time made easier because the relatively small number of essential experimental facts were all available in scientific journals. Now, in the more complex parts of these subjects, large data sets are only summarized in publications, and sharing of the extensive data sets themselves has become commonplace. Two forces have pushed this accessibility. One is the genuine wish to advance science rapidly. The other is pragmatic: doing experimental science is expensive. Science is chiefly paid for from the public purse, either directly by government or indirectly by the tax-free subsidization of charitable foundations. In appealing for publicly based support for a science, it is important that resources are seen to be used effectively.

Good experimentalists excel in the art of knowing which parts of their own unpublished data should be ignored, so not *all* data ought be shared. But certain sharing should become common practice. For example, neuroscientists understand that the (partial) publication of data only through summaries such as post-stimulus time histograms can conceal what is actually happening. In these days of web sites, it would be trivial to make available all spike rasters from which summaries are published.

Some of my friends lament "we will fail to get credit for our work." But most scientists know that it was the careful measurements of Tycho Brahe that led Kepler to his three laws of planetary motion. Reputations of experimentalists are only enhanced by having their data cited as significant by others in the motivation or testing of ideas.

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