

Churchland and the talking brainⁱ

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I

In the space of a single, though large, book Patricia Churchland has been able to cover *almost* everything that needs to be covered in the heady interdisciplinary area comprising the various branches of the neurosciences, issues in the history of epistemology with substantial focus on the philosophies of science and mind, various areas of cognitive science including aspects of theoretical and empirical psychology, several areas of computer science with an extensive discussion of connectionist models. Taking the area as a whole, the 32-page bibliography is one of the richest that I have seen.

It is surprising, therefore, that this extensive coverage has no discussion of linguistics and, in general, almost next-to-nothing on studies on language. In the index, there is no entry either on linguistics or on Chomsky; the bibliography contains just a single, and dated, item from Chomsky. There is no mention of the Chomsky-related works of Eric Lenneberg and George Miller. The entire area of formal approaches to learnability is not mentioned; hence, there is no mention of the pioneering works of Wexler and Culicover and Pinker. This large-scale omission bears significantly on the validity of Churchland's argument. More of that later.

With respect to what Churchland *does* include, it is no mean feat to be able to tell a very long story from Plato to the connectionists with engaging style and scholarship. People from the

neurosciences will find the philosophical chapters to be lucid, if a little rushed at some places; similarly, people from the humanities will appreciate the initial chapters on neuroscience since much of the material presented in those pages is not easily available outside the concerned sciences. No specialist will probably find the material on *her* area to be particularly novel; but every specialist will probably find some novelty in the interactions of specialized knowledge that Churchland presents so skillfully.

Yet the book is not designed to be an encyclopedia. It is not meant to be merely a neutral text-book for the advanced study of the subject (though it can very well be used for the purpose). She has some specific axes to grind and the usefulness of the book must primarily be judged from that point of view.

II

On the concluding page (p. 482), Churchland says that her primary objective has been to show that neuroscience matters to philosophy. This *very* general claim has been subdivided into the following: (a) mental processes are brain processes, (b) the theoretical framework resulting from a co-evolution of neuroscience and psychology is bound to be superior to folk psychology, and (c) it is most unlikely that we can devise an adequate theory of the mind-brain without knowing in great detail about the structure and organization of nervous system.

How significant are these claims? The general claim is quite trivial since, for all that we know, *everything* matters to philosophy. The sub-claim (c) is tautological: how can we know about the mind brain unless we know a “great detail” about the brain? The sub-claim (b) is methodologically trivial since *any* developed science, (for example, cognitive science) is bound to be superior to common sense. The sub-claim (a) is substantive, but what substance is left to

the *literal* Cartesian idea that there is more in the mind than what there is in the brain? There must be more meat in her actual argument than what is too cautiously stated at the end.

Let us pre-emptively grant that Churchland's arguments and evidence against the Cartesian substance dualist, a rare species anyway, are successful. To that end, let us grant that most of the conceptual arguments of Descartes, Popper and the like are either highly contentious or circular or downright false, given counter-arguments and data. Let us also grant that the neurosciences are extremely active fields of research *some* of whose results can now be used to address *some* of the psychological issues with *some* degree of success. Yet that cannot be all that Churchland wants to agree.

I believe the work leaves a much larger impression, usually unstated and left as a background, to which a larger part of this notice will be directed. The work leaves the impression that any research programme in psychology which does not already directly take the neurosciences into account is not a scientific programme at all. To this, of course, a lot of people will not agree (even if we ignore the issue of what is it for something to be counted as a science) and that precisely is the interest of Churchland's work.

In the absence of precise statements from Churchland, I could not be more precise about her larger claim and, unfortunately, matters have to be discussed from this vague ground. I feel that anyone aiming for such large claims faces a predicament. Churchland couldn't have claimed that everyone interested in psychological issue should *only* do neuroscience. She doesn't claim that and it would have been preposterous to claim so. She couldn't even have claimed that people are free to develop any kind of psychological models with the restriction that only neuroscientific data count. Yet she wants (and needs) to claim something lot stronger than merely claiming that

the neurosciences are advancing rapidly and that people should take notice of that. Given the current intellectual climate, that sort of a claim goes without saying. Given this predicament, it is difficult to see how one could make a statement which is, at once, precise and large. Even then Churchland's underlying larger claim, vague and impressionistic though it is, is enormously interesting since it has an eager audience.

This audience will readily agree with much of what she has to say against the Cartesian substance dualist, will read her chapters on the neurosciences with interest *and* expectation, will agree with much of the success stories of neurosciences as described in her book, and will, in general, support her leading idea (i.e., there is nothing in the mind etc.) *without* agreeing with her larger claim. More specifically, this audience is likely to agree with each of the following steps of her argument-steps which cover over three-fourths of the work-without granting the final, implicit, step.

(1) The neurosciences have advanced far enough to be able to say something, in good detail, about actual behavior, say, the behavior of the sea hare (p. 71) and the leech (p.145).

(2) Some of this behavior may be described not only with the minutest empirical detail but also with abstract principles, e.g., geometrical models (Chapter 10).

(3) Applied to humans, there is a reasonable expectation that such explanations may be available for much of *motor* behavior e.g., reflexes.

(4) Many psychological disorders, e.g., some forms of schizophrenia, may have excellent and adequate neurological explanation (pp. 82-88).

(5) The effect of drugs, with the attendant neurochemical explanations, on various mental states are interesting and well-documented (pp. 77-81).

(Note: Not surprisingly, Churchland expresses skepticism about the possibilities for large-scale theorizing the moment discussion reaches items such as 3, 4 and 5. Hence, these discussions are much smaller and sketchier than the discussion of, say, action potentials and pathway organization in the nervous system.)

(6) The importance of split-brain studies (pp. 174-92) is undeniable. In particular, the lateralization effects found in various forms of brain conditions do tell us something about the location and distribution of various cognitive functions in the two hemispheres.

(7) Turning to matters philosophical, it is simply implausible that the identity thesis can be ruled out merely on conceptual grounds. Since, via Quine, Sellars and others, there is no privileged access to introspective states and since, via Kuhn and others, a scientific theory is never falsified by merely attending to ‘direct’ data, introspective or otherwise, the identity theory must be examined on the usual scientific grounds.

These seven steps consist of the meat of Churchland’s argument for the first 350 of the 482-page book. Our audience will readily agree with each of them since none of them are particularly surprising and each of these are largely irrelevant for the view shared by our audience, a view that is quite skeptical of Churchland’s larger claim. The grounds for this view are familiar, but I shall sketch them anyway since I suspect that Churchland has not placed the origins of this view on the correct perspective. Moreover, I need the sketch to prepare for the final step of *my* argument.

III

Up to about the fifties, psychology and philosophy of mind were quite hopelessly caught in an impasse regarding the classical mind/body problem and no solution was in sight. On the one hand, given Darwin and all that, there was no interest left for a full-fledged substance dualism; on the other, despite dramatic advances, the neurosciences and behaviouristic psychology led nowhere with respect to the ‘higher cognitive functions’ of the human mind. Since the very domain of psychology was so enmeshed with the dark mind/body problem, psychology could not generate an autonomous talk.

Beginning, roughly, with the sixties, many people began to think that the scene was getting at least partly clear for the possibilities for an autonomous theoretical psychology. Various called “Intentional Psychology”, “Folk Psychology”, “Cognitive science”, “Computational theory of mind”, “Representational theory of mind”, “Functionalism” and the like, a new attitude toward the study of human mind emerged. As the various titles partly show, there was no clear unified doctrine over and above a cluster of methodological stances. There was, and still is, much internal dispute concerning individual theoretical claims. What binds the ‘new’ psychology, however, is an attitude, the attitude of learning *to ignore the mind/body problem*. I am not suggesting that it was ever stated in a manifesto. The attitude was displayed rather in the practice: no clear solution was ever offered to the mind/body problem to the satisfaction of the dualist or the monist, yet psychological research flourished at an unprecedented scale. For the sake of a tag, let us call this emerging paradigm the “Representational Theory of Mind” (RTM) since this tag has been used in an important global critique of the paradigm (Stich 1983).

Two approaches emerged. One approach was to take the mental substrata for granted together with a certain abstract structure for this substrata. The abstract structure consisted of *rules* defined over *representations*. Then, it was shown that, given this structure, explanatorily successful theories can be formed for certain domains of cognitive behavior. Handy historical parallels, such as Newton's postulation of gravitational field, were recalled. Stringent criteria for explanatory success were invited. If the theory survived, it counted as an evidence that such a structure is indeed a property of the brain. What else could be responsible for the concerned behavior?

That is what the work of Noam Chomsky on language accomplished for RTM. One may challenge the theory on empirical grounds and, in so challenging, grant a degree of autonomy to the enterprise. While the empirical issues were addressed with due rigour, it was hoped that the conceptual postulates of the theory will become digestible in time. Kuhn and Feyerabend support everyone. It was further hoped that someone in future will tell how the conceptual postulates of the theory link up with the properties of the brain. Keeping the conceptual postulates dangling, RTM (language) ignored the mind/body problem just as most biologists tend to ignore the nature/nurture problem. In the mean time, people showed their sense of humour by using the unitary notion of mind/body, as Chomsky did, and left matters at that.

The other, more hazardous, approach was to grow impatient and to argue for a more detailed theory of the mind with its attendant concepts and assumptions. It is quite surprising that, given the infancy of the RTM paradigm, this enterprise has already produced a number of classics (Fodor 1975, Pylyshyn 1984) - a fact that tells us something about our intellectual climate. People argued for the difference in 'grain' between psychology and *current* neuroscience which, in turn, suggested that the mental substrata may be instantiated on the brain rather than being

identical with it. This idea was developed by drawing a parallel with the software and the hardware of the computer. Finally, it was argued that such a substrata is assumed in much of our common sense psychology anyway and that this ‘folk’ psychological notion of the mind may be used as a starter to describe the regularities of various cognitive behaviour in an explanatory interesting way.

Each of these hazardous claims can be, and has been, challenged. Yet, when the merits of the RTM are judged from both the approaches, challenges to the elements of the second will show, at most, that an RTM scientist does not have a clear theory of his own success. Given the degree of autonomy achieved via empirical success, in language theory and elsewhere, there is no good reason why an RTM theorist cannot cheerfully admit his lack of conceptual clarity *while* standing on his ground for now he, like Newton, is armed with a success theory.

IV

Immediately following the steps described in (II) above, Churchland devotes a long chapter titled “Functionalist Psychology” where she develops a fairly exhaustive critique of the second approach sketched above. She attacks Fodor, Pylyshyn and a number of other theorists, as did Stich in his (1983). Not all of the arguments are equally convincing, but I will leave the task of finding rejoinders to the RTM ideologues.

My worry is that Churchland did not address the *first* approach at all which is squarely situated within the RTM paradigm, warts and all. If she wants to argue for an alternative paradigm, this time directly involving the brain, she must find some way of explaining (or, explaining *away*) the success of the existing paradigm. To take a handy precedence, this is what Einstein did for the Newtonian paradigm. She must either deny the success or co-opt the success

somehow in her own terms. This she never even tried to face, as evidenced by my list of omissions mentioned at the outset. I believe that the implication of this point for Churchland's larger claim is far-reaching.

I have already granted a scenario in which an RTM theorist can live with his (alleged) conceptual weaknesses. What are the crucial features of this scenario? (a) The RTM theorist gets to work without waiting for the conceptual issues to be clarified in advance; that has never happened in the history of science. (b) The RTM theorist is not interested in the global and intuitive notion of psychology and psychological states. That is why he readily grants steps such as 3-5 listed in (II). (c) The theorist is interested in a specific cognitive area, the higher cognitive functions, and in clearly identifiable domains within that area. One such domain is the human language ability which is specie-specific and which enables the theorist to ask a number of answerable questions without begging the mind/body issue. (d) Certain detailed models of this ability can be formulated with due empirical and formal rigour. (e) Adjacent domains, which seem closely to interact with language, and which seem to have structural similarities with the language domain (though probably not analogically), may then be isolated for similar studies. These will include problem-solving abilities, the ability to form hypotheses, to do mathematics and the like. In sum, the identification of the area of higher cognitive functions is itself a part of the research programme, not some object given in advance. (f) With respect to this area with linguistic ability at its core, only top-down models are of any scientific interest for now, whatever be the underlying conceptual basis for such models (there must be some, otherwise how come they are so successful?).

How can *this* scenario be challenged? With a spade, of course, i.e., by doing interesting science; nothing short of that can count. What will such an alternative interesting science look like?

An alternative science must somehow enter the guarded castle of the RTM, viz., the domain of language, for the RTM enjoys whatever support it does, in ever-expanding circles, around this core. A neuroscientific theory of the human language ability is, then, what Churchland needs to clinch her larger claim.

About the only neuroscientific research on language with Churchland reviews (Item 6 above; see Gardner (1974) for a more extensive popular survey), however, will not do. Split-brain research, I have agreed, does say interesting, sometimes even quite dramatic, things on the localization of linguistic ability. But such research throws hardly any light on the central issue, viz., an *explanation* of such ability. Split-brain research says, at most, things of the following sort: a cognitive capacity C_i is located in the area X_i . Sometimes such descriptions can be finer and more interesting: a sub-capacity S_i of C_i is located in the sub area Y_i of X_i . Probably one can go finer still. Yet, however fine it goes, it can only keep on describing without explaining the nature of C_i , S_i and so on. Indeed, there is a severe limit even to its descriptive depth in the absence of a theory of the concerned capacity.

Split-brain research looks fascinating to the neuroscientist and the layman partly because of moral and Cartesian hang-ups which, though important, are not under discussion here. The other reason why split-brain research looks interesting is that people *do* have some rudimentary conception of what is to count as a cognitive domain, for example, language. In that sense that neuroscientist does work with a theory of some sort which is largely shared by the layman as

well – probably, a theory to the effect that language is a collection of sound with meanings attached, or some modification thereof for controlled research. Such a theory, even when modified, is very far from, say, Chomsky’s theory of language (*grammar*, to be more precise) which, in any of its versions, contains hardly any folk element. So much for Churchland’s tirades against folk psychology.

Further, the preceding point relates to the point that Chomsky, Fodor, Pylyshyn and others have pressed vigorously under the rubric ‘competence’. These two points are related since the limitation of split-brain research (for the explanatory issue under discussion) is essentially linked to the limitations of the neuroscientist’s answer to the question ‘what is C_i ?’ It is very well-known that interesting answers to such questions can be reached only if we are able to focus directly on the concerned cognitive behavior, say speech, to form some idealized, abstract notion of such behavior. These idealisations are essential if we are to eliminate the effects of other cognitive systems and various irrelevant factors (performance-factors) from the study. Particular proposals for the notion of competence may be suspect, but the preceding general idea must be available for any interesting research to take shape. Thus, the neuroscientist needs the top-down models of competence for, otherwise, there is nothing *further* for him to look at after his initial cartography. The neuroscientist needs top-down theories; the converse, however, can well be ignored (ignore the issue of ‘hardware-constraints’, an entirely different matter) since no notion of competence will emerge from simply looking at the brain.

Churchland does say things of the above sort-the co-evolution/complimentarity of cognitive science and neuroscience-in her introduction and elsewhere. But I find no evidence that the importance of these points for her larger claim has been seen.

Suppose we tone down Churchland's larger claim. Suppose she gives up the primacy of bottom-up research and settles for true complementarity tilted for towards top-down research. What will this stance amount to?

Suppose we take a sample of the kind of work that has been done on language in the RTM/top-down tradition. Among many, three sorts of work stand out, as I listed at the outset. The first sort concerns fairly abstract grammars written for large fragments of a wide variety of languages resulting in the isolation of some universal principles and constraints which are supposed to be a part of the genetic endowment and, hence, instantiated on the brain from the outset. The second sort, e.g., the work of Wexler and Culicover, concern the computational properties of grammars telling what sort of grammars are likely to be learned by what computational systems. The third sort, e.g., the work of Pinker, studies the learnability principles of some grammars with respect to actual child data. How will the neuroscientist get into this picture?

So far as I know, the preceding research has not yet converged, not the same grammars have been studied from all three different angles. Suppose research does coverage (and this is not empty speculation). Suppose then that we have an abstract principle P about which we know almost everything that can be known in the top-down way: we know that it is a universal principle, we know the nature of the system that learns it and the mechanisms of such learning (here we *are* speculating). So we hand this principle over to the neuroscientist. Can he handle it? Can he tell us something to the effect that principle P will be activated just in case such-and-such a cluster of neurons under so-and-so wiring is triggered off by such-and-such a neurochemical?

Nothing is more fantastic and nothing less will count given the current state of top-down research; and *the gulf is increasing*. Cognitive science awaits some dramatic conceptual revolution in the neurosciences. Such a revolution, when it happens, will be recognized at once.

Notes

i Patricia Smith Churchland, *Neurophilosophy: Toward a Unified Science of the Mind/Brain* (Bradford Book. The MIT Press, Cambridge, 1986), pp. xi + 482 + notes, bibliography, index.

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