The Architecture of Belief

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Let us assume that intentional realism is false, that intentional states won’t feature in The-One-True-Cognitive-Psychology, our best science of the mind. One advantage if it turned out that intentional realism were true would be that it would automatically bestow upon folk psychology an ontological and explanatory role for states such as beliefs and desires. Indeed, if one believed in the singularity of explanation thesis (from chapter 1) as do the intentional realists and eliminative materialists, then intentional realism’s being true would be the only way that intentional psychology could have such explanatory or ontological role to play: intentional realism’s being false would force us to dispense with folk or intentional psychology as an explanatory exercise. Thankfully, the singularity of explanation thesis is wrong—or so it has been argued above.

With our assumption still in place, what kind of role might intentional psychology play in order that we do not dispense with it in face of The-One-True-Cognitive-Psychology’s not requiring intentional states? I think there are at least two possibilities regarding roles other than those presented by The-One-True-Cognitive-Psychology. The first has to do with the possible nonpsychological roles intentional state attributions might play. As we shall see below, this strategy actually divides in two. As part of its nonpsychological usefulness, intentional psychology might be utilised by disciplines other than psychology viz. the social sciences. That utilisation might well be enough for us not to dispense with folk psychology. The second has to do with taking a broad conception of psychology so that even though intentional states might not coincide with states postulated by The-One-True-Cognitive-Psychology, there nevertheless remains a psychological role for folk states to play. We begin with some considerations regarding the nonpsychological uses of folk psychology.
1 Is Folk Psychology Too Fine Grained in its Explanations?

It is often suggested that one reason why intentional realism is false, i.e. that common sense belief-desire explanations will not feature in a future science of the mind—The-One-True-Cognitive-Psychology—is that the states and processes over which The-One-True-Cognitive-Psychology quantifies will be too rich and fine grained to count as folk intentional states and processes.1 My present thought is that perhaps this charge has things the wrong way around. Perhaps it's intentional psychology which will turn out to be too fine grained and rich in the kinds it postulates in order to be quantified over by The-One-True-Cognitive-Psychology. Let me explain.

Intentional psychology is normally taken to be modelled on sets of two attitude types: belief and desire. Since Hume (1748) there has been a prevailing requirement for at least these two attitude types; beliefs to represent the world and desires to motivate the agent. Fodor, for one, is very explicit about central role of these two attitudes, as evidenced by his calling intentional psychology "belief-desire psychology". But as we know, there are multifarious propositional attitude types: there are beliefs, desires, hopes, rememberings, thoughts, fears, etc. Clearly, when The-One-True-Cognitive-Psychology is complete there is going to be a determinate number of attitude-types quantified over if intentional realism is true. As to how many of the attitudes types are quantified over is, I take it, an empirical matter. However, some of the attitude types the users of intentional psychology attribute to agents seem to do more work than psychological work. Let's take a look at this in some detail. While I think that the considerations below show that the failure of the intentional realist programme will not mean the demise of intentional psychology, they can, in addition, be used as arguments for rejecting intentional realism—provided the criteria for vindication of the attitudes spelled out in chapter 1 (section 4.4) are in place.

1.1 What is a Psychological State?

Assuming that we often do frame explanations of an agent's behaviour in terms of more than just beliefs and desires, why should we be required to do so from a psychological point of view? Consider the following explanations of Reagan's ordering the destruction of the Iranian gun boats:

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1For example, Schiffer (1987 p. 41) and Loar (1981 pp. 17-18) use this putative feature of intentional psychology in their discussions.
EX1 (from chapter 1): Reagan believed that the Iranians were pirating Hollywood westerns and that the boats were smuggling copies across the Gulf. Reagan desired that the pirating of Hollywood westerns stop. He also believed that the ordering of their destruction would be an effective means towards ending pirating.

EX2: Reagan knew that the Iranians were pirating Hollywood westerns and that the boats were smuggling copies across the Gulf. Reagan desired that the pirating of Hollywood westerns stop. He also knew that the ordering of their destruction would be an effective means towards ending pirating.

Such explanations are commonplace. Our current question is: what does EX2 give us over EX1 in terms of the explanation of Reagan’s action? I think EX2 does give us something more in the way of an explanation, but not anything more of an explanation of why Reagan did that particular action.

EX2 tells us that the Iranians really were pirating Hollywood westerns and smuggling the copies across the Gulf. But what has this have to do with Reagan’s deciding to perform his action? Let’s suppose that Reagan claimed to know about the Iranian’s gambit. Even if it is true that Reagan did know at that time, he did not know that his claim was justified. Claims to know something when that something turns out to be false get downgraded to the status of beliefs. So, what over and above mere believing do we require in this explanation? Nothing I claim, since irrespective of whether Reagan merely believed or knew, he would have performed that action. Describing Reagan’s psychological state as that of knowing that p seems somewhat redundant in intentional psychological explanation.

There is an obvious way in which EX2 is not redundant. It is that EX2 establishes why Reagan’s action was efficacious in bringing about the satisfaction of Reagan’s desire. If the Iranians were not running the pirated movies across the Gulf, then destroying the gun boats would fail to stop the distribution of the westerns. The attribution of knowledge to Reagan explains the success of his actions. Whether or not Reagan knew that the bombing would be an effective means of ending the pirating will determine the likelihood of Reagan’s performing that action. Perhaps there are two candidate ways of stopping the pirating: bombing the gun boats or writing a letter of protest to the Ayatollah Khomenei. If he claimed to know that the former would work but only believed the latter would then we get a better idea of why he performed that action rather some other.
This, however, commits us to nothing more than differing degrees of belief on the part of an agent. An agent might claim that he knows that p. But that is only a claim. The question of the agent's actually knowing that p remains open.

Typically, explanations such as EX1 and EX2 are third person and post hoc. The reason why there is a redundancy in these cases is that while we possess information relevant to why an agent performed some action, that information does not seem relevant from the agent's point of view in whatever psychological processes are going on within him or her at the time s/he decides to perform or performs the action. From the agent's point of view, there can only be a claim to know that p before the event.

This question of the redundancy of certain propositional attitude ascriptions raises the questions of just what is a psychological state, and which of the attitudes correspond to psychological states. From the above discussion it would not seem that propositional attitudes ascriptions involving factive verbs such as 'know', 'remember', 'perceive' and 'regret' are going to count as psychological states on the intentional realist story. According to intentional realism, to have a psychological state of the type posulated by intentional psychology is to bear some relation to an internal mental representation. Since an agent's knowing or remembering that p requires that some state of affairs to be the case, then the requirement of intentional realism that an organism knows that p iff it stands in some relation to a mental representation cannot be met.

Fodor recognises that there are these problems for intentional realism to sort out. As early as The Language of Thought (1975 p.75fn) he suggests that some propositional attitude ascriptions might be problematic from the point of view of psychological theory. In a footnote he says:

> Clearly, no organism knows that a is F unless it is the case that a is F. ...
>
> It follows that there can be no computational relation to a formula such that (an organism knows that a is F) iff (it stands in a relation to that formula). ...
> one could not expect more than a rough correspondence between the inventory of propositional attitudes that we pre-theoretically acknowledge and the ones which psychological theories prove eventually to be about. (1975 p.76fn)

Assuming that only those states quantified over by psychological theory will count as psychological states, then attitudes involving factive verbs would seem not to be purely psychological states—according to intentional realism.

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2 For a discussion of factive verbs see Kiparsky & Kiparsky (1971).
at least. We saw in chapter 1 that intentional realism might well have to employ a mixed taxonomy of intentional states in order to account for the various attributions of propositional attitudes which do not consist in a relation to a mental representation. Propositional attitude ascriptions involving factive verbs will, presumably, have to feature as another variant in this mixed taxonomy.

The upshot of these considerations seems to be that intentional state ascriptions, while undoubtedly having some psychological salience, also have epistemic significance. What EX2 presents to us is not merely Reagan qua agent but Reagan qua knower. Presenting Reagan as a knower adds a further justificatory component to Reagan's acting on his desires: he knew that the Iranians were infringing Copyright, and so, granting his desires, the action he undertook was most justified. If he had been wrong then we would demur from our judgment that he was justified, other things being equal.

It seems that in the case of propositional attitude psychology, not only does the philosophy of psychology meet the philosophy of language, but also epistemology. Presuming that epistemology is an important theoretical enterprise, then these considerations constitute a start at determining the extra-psychological work performed by intentional psychology. The next section builds upon this start.

1.2 Explanatory Work and Justification

We can further explore the issue about justification just raised by considering yet another description of the etiology of Reagan's action framed in terms of intentional states. Consider:

EX3: Reagan feared that the Iranians were pirating Hollywood westerns and that the boats were smuggling copies across the Gulf. Reagan preferred that the pirating of Hollywood westerns stop. He also hoped that their destruction would be an effective means towards ending pirating.

How does EX3 differ from EX1 and EX2? The first difference is one's possible surprise if it turns out that Reagan's action is efficacious. After all, from the description of Reagan's epistemic condition given in EX3 it is not even clear that the Iranians are in fact pirating Hollywood westerns. While it also does not follow from EX1 that the Iranians are in fact pirating the westerns, at least if one claims to believe something, one must have reasons for doing so. Fears on the other hand, are often not accompanied by justifying reasons, at
all. It’s also not clear that the destruction of the gun boats will put a stop to the pirating; the Iranians might have other channels by which to move their celluloid contraband. It seems that EX3, just like EX1 and EX2, has built into it an epistemic evaluation component. Irrespective of whether or not Reagan’s action is efficacious, if we explain his action by EX3 rather EX1 or EX2 then we imply that there is a potential epistemic objection to whether Reagan was \textit{justified} in taking his course of action. I want now to take a look at this idea of the epistemic justificatory component of intentional explanations.

There are a couple of ways in which intentional explanations contribute to the praise or blame of an agent’s actions. The first, but not very interesting way from the point of view of our current concerns, is that an attribution of a desire or preference of the agent is made. Now if we think that the agent’s desire is not worthwhile—as Reagan’s would seem not be worthwhile (well, to most of us anyway)—then any consequent action based upon that desire is going to be deemed not worthwhile or even irresponsible. The second and interesting way takes as a given that the desire of the agent is worth acting in accordance with, but questions the epistemic condition of the agent and how that condition affects the satisfaction of that desire. If it is not clear or perhaps guaranteed that destroying the gun boats will satisfy Reagan’s desire, then we will assess his action as irresponsible, or whatever. This epistemic evaluative role is what EX3 makes clear where the other candidate explanations do not.

To the extent that EX3 plays a part in the epistemic evaluation of an agent’s actions, it has some legitimate explanatory work to perform. However the question requiring answering here is: is that explanatory work performed by EX3 work required from a psychological (read: intentional realist) perspective?

Forgetting for the moment about epistemic evaluation and its role in the assessing of an agent’s action, from a psychological point of view, it would not seem that EX3 would be required as an alternative to EX1 or EX2 as an explanation of Reagan’s action. In so far as we want to explain the brute behavioural output of Reagan (which remains constant across the explanations, I should point out), EX1 is going to perform all the explanatory work we require. The reason why is much the same as in the case of why EX2 might not be required as an explanation by the intentional realist. In the case of EX3 there is no factive component of the intentional states cited in the explanation. But just as the question of the truth or falsity of the belief seemed irrelevant to the intentional realist’s explanatory enterprise, so too the epistemic justification would seem irrelevant, since the behaviour of Reagan would be the same across EX1 and EX3.
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One might object that there is an obvious way in which EX3 does more psychological work. It will explain Reagan’s surprise if his gambit pays off, since he was unsure as to whether the course of action he undertook would be efficacious. Perhaps when making his original decision, Reagan might have sniggered to himself and Shultz because he thought it would be fun to upset the Iranians even if the information upon which he was acting was not epistemically justified. In this case, however, Reagan’s behaviour is different from the case described above where EX1 or EX2 seem relevant. Reagan’s action, as initially described, was the simple ordering of the attack, and not ordering the attack with a snigger to Shultz. The behaviour to be explained needs to remain constant if this point is to hold.

If the claim that explanations such as EX3 are not going to be required by psychology seems counterintuitive, it shouldn’t. What I am not saying is that explanations such as EX3 do not have a role to play in our describing and assessing the actions of agents. Of course EX3 is not redundant in this respect. The point I am making is that it might be the case that The-One-True-Cognitive-Psychology will not require explanations such as EX3. What I am suggesting is that perhaps intentional psychology does more work than mere psychological work. Intentional psychology might be bound up in our assessments of an agent’s action—whether that action is justified, right or wrong etc.. Given that intentional psychology is basically a folk theory, and such folk theories developed in conjunction with our epistemic and moral conceptions, then this extra non-psychological work performed by intentional psychology should come as no surprise.

A problem for the intentional realist, therefore, is to decide how much of this extra work s/he thinks The-One-True-Cognitive-Psychology is going to buy into. What decision the intentional realist ultimately makes will depend upon views regarding issues such as whether our psychological conceptions of sanity and rationality are inherently evaluative in ways other than those we might think a purely scientific enterprise should be. Whether or not the intentional realist is going to easily decide upon such issues, or whether the issues are decidable at all, is not my purpose in raising these issues. Our concern has been to point out that any extra-psychological component is going to pressure one to accept that intentional psychology may well have uses even when intentional realism fails.

2 A Broader Conception of Psychology

We have thus far been considering non-psychological uses for intentional psychology that might ensure that folk psychology will not be dispensible.
There are, however, denials of dispensibility on purely psychological grounds. I am thinking of Kitcher (1984) and Jackson and Pettit (Forthcoming). These works, though, are silent on the question of the status of intentional psychology given that intentional realism fails. Kitcher merely attempts to play down the supposed shortcomings of folk psychology as argued by the eliminativists (see chapter 1). Jackson and Pettit attempt to give an account of intentional folk psychology which would make it consistent with whatever ways The-One-True-Cognitive-Psychology or the neurosciences turn out—whichever of these two comes up being the proper scientific leader in the explanation and prediction of behaviour. This belief in the consistency thesis from chapter 1, claim J and P, consists not in intentional states reducing to say neuroscientific states or states postulated by The-One-True-Cognitive-Psychology; they allow that intentional psychology might taxonomise and organise states of agents differently from that of its more scientific cousins. From Part I we know that in so doing these enterprises will constitute different level of explanation-description. I have preaching throughout this work that intentional psychology does so constitute a distinctive level of explanation description.

From this sensible position, Jackson and Pettit then go on to argue that “beliefs and desires will very likely be found within what completed neuroscience tells us” (Forthcoming p. 20), and again, “Completed neuroscience will indeed provide a complete story about when and why we do what we do, but will incorporate rather than eliminate beliefs and desires in this complete story” (p. 23). The idea seems to be that intentional psychology will “show which part of any likely complete neuroscience story is the part which says (though not in so many words) that there are beliefs and desires” (p. 21). They seem to be saying that because we are dealing with distinct levels of explanation then neuroscience tells us that the roles we identify through intentional psychology really are filled. That is, neuroscience might tell us that our brains are just receivers of signals from Mars, where Martians controlled us (Jackson & Pettit, Forthcoming p. 14), thereby contravening the principle of agency (from chapter 7). In such a case we would not possess beliefs and desires even though we acted as if we did have them. This keeps intentional psychology at a different level from The-One-True-Cognitive-Psychology or the neurosciences, but allows the possibility of there being evidential and/or supervenient relations between them, the former relation being allowed by the considerations of chapter 3.

Now, assuming that the intentional psychological level is a different level from that of the successful explainer and predictor of behaviour, how might we utilise such a level of explanation-description within psychological
theorising? The easiest way would be to conclude that the domain of psychology really encompasses various levels of explanation-description. Just as there are many levels to the neurosciences—neuroanatomy and neurophysiology—there may well be many levels to the psychological domain. There is neuropsychology, as we have seen above, cognitive psychology and social psychology, and even psychiatry, maybe. A clinical psychologist, for example, might find it useful to draw from elements of each of these fields.

Where does intentional psychology fit into the picture? If intentional realism is false, and intentional states do not feature in The-One-True-Cognitive-Psychology, then there is always the possibility that intentional psychology might find its home in one of the higher level areas falling within the purview of the psychological domain, social psychology or psychiatry, say. Indeed, these enterprises make use of intentional states now, and may well make use of them in the future, despite intentional realism's being false. This assumes, of course, that we will still be pursuing social psychology and psychiatry in the face of The-One-True-Cognitive-Psychology. The-One-True-Cognitive-Psychology might well turn out to be a powerful weapon in the explanation and prediction of human behaviour, but I have as yet to see any arguments that it will exhaust the enterprise of psychology. If it doesn't, then there still may be some psychological role for intentional psychology to play assuming intentional realism to be false.

3 The Social Sciences

There is a final means by which intentional psychology may be salvaged: we may appeal to even higher level disciplines than any so far mentioned. At an even higher level of explanation-description there exists economics, sociology, and anthropology. The social sciences have at times a need to attempt explanations and predictions of behaviour. Because these enterprises are explanatory-descriptive levels, they will quantify over kinds that suit their explanatory ends. To the extent that they do appeal to states of agents in forming explanations and generating predictions of their ensuing actions, then the social sciences will also appeal to the states of agents that will do the explanatory work required of the particular levels in question. It might be the case that the social sciences will only require to advert to Level One states of agents in order to frame the generalisation peculiar to their discipline. The Level Two states being of no consequence from the point of view of those high level disciplines.
The failure of intentional realism is regarded as a massive catastrophe for the social sciences according to some of the literature on folk psychology.\textsuperscript{3} I think this view is fuelled, in part, by the assumption that the singularity of explanation thesis holds true. If one assumes that there is some other calculus for generating explanations and predictions of human behaviour other than The-One-True-Cognitive-Psychology such as intentional psychology, then even though that alternative calculus is not as explanatorily and predictively accurate as The-One-True-Cognitive-Psychology in that its generalisations are less than perfect, nevertheless those generalisations may be adequate from the social scientific point of view. The mistake behind the pessimists regarding the status of the social sciences with respect to a beliefless and desireless One-True-Cognitive-Psychology is they assume that those social sciences must take their psychological ontology from The-One-True-Cognitive-Psychology. As with the rest of their ontology, high level disciplines such as the social sciences are free to taxonomise the world in the way they want, to choose ontologies that best suit their explanatory-descriptive ends.

\textsuperscript{3}See for instance Stich (1983 p. 228-9).
Appendix A

Historical Antecedents of VFT

In 1802 Franz Joseph Gall (1758-1828) was thrown out of Vienna by decree of Emperor Francis I, he was excommunicated by Pius VII in 1817 and generally denounced by the medical establishment throughout Europe (John Marshall 1980 p. 24). Gall was, after all, the founder of what was to become known as *phrenology*: the doctrine that important traits of character can be determined from a study of the bumps on the skull (Robert M. Young 1970 p. 9).

As a young school boy growing up in the middle part of the eighteenth century, Gall observed that classmates with prominent eyes tended to have good memories. Subsequent observations of fellow students at university led him to conjecture that he had a genuine hypothesis relating physical features to a cognitive capacity, a hypothesis which he believed was capable of being tested. How he went about attempting to prove this hypothesis provides both Gall's major contribution to psychology and his methodological downfall.

He says:

> I could not believe, that the union of two circumstances which had struck me on these different occasions, was solely the result of accident. Having still more assured myself of this, I began to suspect that there must exist a connection between this conformation of the eyes, and the facility of learning by heart. (Quoted in Young 1971 p. 13)

Gall then proceeds to generalise:

> Proceeding from reflection to reflection, and from observation to observation, it occurred to me that, if memory were made evident by external signs, it might be so likewise with other talents or intellectual faculties. From this time all the individuals who were distinguished by
any quality or faculty, became the object of my special attention, and of systematic study as to the form of the head. (Young 1971 p. 13)

However, Gall was later to qualify these remarks by the following:

I had in the interval commenced the study of medicine. We had much said to us about the functions of the muscles, the viscera, etc., but nothing respecting the functions of the brain and its various parts. I recalled my early observations, and immediately suspected, what I was not long in reducing to certainty, that the difference in the form of the heads is occasioned by the difference in the form of the brains. (Young 1971 p. 13)

So Gall transcends his purely physiognomical beginnings to become a propagator of functional neuropsychology.

Today, we (although not everyone, unfortunately) dismiss phrenology as utter nonsense and qua phrenologist Gall deserved any scorn heaped upon him. But Gall lived a double life; he was at once a craniologist interested in cranioscopy and at the same time a skilled anatomist using advanced dissection techniques. Under this latter guise Gall achieved early justified eminence as a neuroanatomist, setting the groundwork for much of modern neuroscience. For instance, it was Gall who distinguished cortical grey and white matter and differentiated projection, associative and commissural fibres (C.G. Gross 1985 p. 17). Gall also leaves us with a psychological legacy: the beginnings of VFT. Where Gall's credibility comes unstuck is the false physiognomical suppositions accompanying his neuropsychology, and poor empirical methodology. We can summarise the main tenets of Gall's programme thus:

(1) Mental capacities develop differentially across both inter- and intra-species individuals.

(2) Mental capacities depend upon fundamental innate faculties.

(3) Faculties are localised in specific organs of the cerebral cortex.

(4) The prominence of the faculties is a function of the size of the cortical organ.
The size of the cortical organs is represented by the shape of the skull or cranium.\footnote{This list is a summation of Gall's hypotheses given by Gross (1985 p. 17) and Young (1971 p. 12).}

From the point of view of the history of neuropsychology (1) through (3) are perfectly respectable theoretical hypotheses. That history also tells us that (4) and (5) are pure physiognomy; (4) has proved to be wrong and (5) is little better than patently untenable. In fact Gall actually says:

The moral and intellectual dispositions are innate; their manifestation depends upon organisation; the brain is exclusively the organ of the mind; the brain is composed of as many particular and independent organs, as there are fundamental powers of the mind—these four incontestable principles form the basis of the whole physiology of the brain. (Herrnstein and Boring 1965 p. 219)

It is, therefore, possible to interpret Gall as believing (1) through (3) to be the central hypotheses of his programme. Gall, in fact explicitly rejected the craniological label attached to him: “They call me a craniologist, and the science which I have discovered, craniology. I rather think that the wise men have baptised the child before it was born. The object of my researches is the brain” (Marshall 1980 p. 24). We can see the Gallean antecedents of VFT by taking a look at (1) through (3) from a contemporary perspective.

1 Individual Differences and Competence

Gall's interest in individual differences takes three forms. The first is the degree to which a particular individual's cognitive capacities differ according to the task at hand. Thus, Smith might be particularly good at remembering numbers, but hopeless at remembering faces. How the deployment of intra- personal faculties can vary across content domains in this way plays a central role in Gall's arguments for how faculties are localised. Much more on this later.

Secondly, Gall is interested in the failure of cognitive capacities to correlate across individuals. Jones is good at remembering things but cannot add or subtract whereas Smith can perform immense calculations in his head but cannot remember his telephone number. According to Fodor (1983 pp. 18-20), Gall has the annoying tendency to run intra-individual differences with these differences across individuals. To be sure, there are two distinct
motivations for attributing faculties here, and they ought to be kept distinct. As we shall see in the next section, I think both motivations can be retained within a VFT framework.

The third set of differences are inter-specific. Gall proposed that innate propensities and faculties are "unequally shared by different species of animals" (Marshall 1980 p. 24). Although Gall's reflections on inter-species differences are pre-evolutionary, he does recognise some cognitive capacities across species. He says: "The same organ, which in the nightingale produces singing, in the beaver the faculty to build, produces correspondingly in man music, architecture... the arts and sciences were not invented because of the necessities arising for them, but because of our innate disposition" (Marshall 1980 p. 24). Gall frequently analogises faculties to instincts in this way. Fodor claims this to be a problem for Gall since it does not sit very well with the argument from degrees of individual differences. Why? Because while both instincts and faculties are going to be genetically determined, the presence of an instinct is going to be inferred from competences undifferentiated across subject populations of species; but Gall's arguments from individual differences require differences in competences across subject populations of species. Or to put the problem another way, Gall wants individual differences to be inherited in the same way in which instincts are inherited; but that is to confuse issues regarding genetic determination and species specificity. What is instinctive is genetically determined, but not all genetically determined capacities need be instincts. Faculties attributable because of the evidence individual differences provide can be genetically determined without their being instinctual.

Although Gall's interest in intraspecific differences does not sit well with the instinct analogy, VFT has the option of restricting the instinct-faculty analogy to the discussion of inter-specific differences and not intra-specific differences. In fact, as evidenced in the last quotation, it is not, strictly speaking, inter-specific differences which are of interest to Gall at all, but inter-specific similarities. What VFT must do is factor two distinct arguments out of Gall here: inter-specific similarities and intra-specific differences.

At this point I should clarify just what it is that Gall is attempting to explain here. At various stages Gall refers to the explananda in question as "powers", "qualities", "instincts", "propensities", "aptitudes" and "talents". This is a decidedly mixed bag, the elements of which are not obviously coextensive. We can marshall the general idea of cognitive capacity or competence to aid Gall here. Particular faculties are postulated as a contribution to the explanation of a particular cognitive capacity such as
Appendix A

Historical Antecedents of VFT

language competence. The set of faculties ultimately postulated will contribute to accounting for overall cognitive competence. How coarse or fine grainedly Gall and VFT carve up competences is crucial; we shall examine this question below.

2 Explanation by Faculty

Gall's postulation of particular faculties responsible for mental capacities ought not, strictly speaking, be taken as an attempt to explain those capacities. If Gall was doing that then he would suffer the circularity inherent in the old faculty psychologies. To postulate a faculty of amativeness to explain amativeness is no better an explanation than that offered by Molière's physician who explained that opium produces sleep because it has a soporific tendency (Young 1971 p. 22). Instead, Gall's postulation of faculties ought to be seen as an assumption in a larger scale explanatory programme, in terms of an evolutionary account of the existence of Level Two faculties and the detailed functioning of the brain. Unfortunately, Gall does not go much beyond this assumption. He has a lot to say about what faculties there are—that is what the functions of the brain are; but little to say about how the brain functions (Young 1971 p. 22). For Gall, knowledge of the functions of the brain must precede knowledge of the structure of the brain (his empirical methodology was to suffer because of this belief).

Having put the question of circularity aside, two features of faculties should be noticed. The first is that Gallean faculties are innate. By innate we must take Gall to be claiming that faculties are genetically determined. His nativism, though, is far from naive. As Marshall summarises: "The claim that the 'elementary qualities of the mind are innate' would nonetheless require, of course, that these qualities must be 'drawn out and cultivated' by the environment" (Marshall 1980 p. 23). This allows for the possibility of triggering in the development of these "fundamental powers of the mind". Possession of a particular innate faculty implies a basic competence, but unlike instinctive competence Gall must have differentiation within a species if he is to account for individual differences.

How differentiation in competence is to be explained is going to be a problem for Gall's project. While he can account for individual differences by appeal to difference in the performance of the underlying faculties across those individuals, he is not going to be able to account for differences in

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2Although for how much one can explain in this way see chapter 2.
performance within the individual. In short, he required a performance-competence distinction.

This brings us to the second feature: the fundamentality of faculties. Gall requires some principled way of determining just how many faculties there are. As we shall see in the next section he goes about this determination on the basis of categorising individual differences in cognitive capacities from anecdote collection. The trouble is though that there are too many individual differences to go around; a one-to-one correspondence between striking behaviour and faculties is psychologically implausible. So Gall's faculties are fundamental in the sense that there are a few faculties out of which varied behaviour can arise depending upon the "mix" of the basic or fundamental faculties. 3

Fodor claims that Gall denied the existence of horizontal faculties such as memory, judgement and reason. But as we saw in the previous section, this is a misrepresentation. What Gall is denying is that these horizontal categories can constitute faculties. It is possible on Gall's programme for there to be memory, judgement, sensation, etc., but these categories would apply within each vertical faculty associated with a particular cognitive domain rather than cutting across those domains.

That the faculty theorist must be careful about individual differences can be seen in the subsequent identification of more faculties after Gall. Spurzheim, Gall's collaborator, extended Gall's original list of twenty-seven to thirty-five. By the end of the century there were over one-hundred phrenological faculties postulated (Gross 1985 p. 17).

Why might cognitive architecture be arranged vertically? Gall's reasoning was something like the following. If there is only one faculty cutting across different domains then the performance of that faculty should remain constant across task domains. But there is no such constancy. Individuals can be good at remembering faces but hopeless at numbers. Fodor claims this reasoning to be fallacious. All Gall has shown is that facial memory is distinct from numerical memory which is, he claims, compatible with facial and numerical memory being exercises of the self same faculty with respect to faces in the one instance and numbers in the other. As he says: "there is no obvious reason why the same faculty should not be strong in one employment and weak in another, so long as the employments are not themselves identical" (Fodor 1983 p. 17). But this won't do. Fodor needs to cash out his use of 'employment' here. One reading seems to be that different employments result from different mechanisms operating. This is exactly what Gall needs to show. He would be quite happy to show "merely" that

3The question of faculties mixing and interacting will become important below.
facial memory is distinct from numerical memory since each form of memory is restricted to a particular cognitive domain. Gall does not, despite Fodor's belief to the contrary, have to eschew the term 'memory' from each domain provided that there is no domain cutting psychological mechanism corresponding to that horizontal faculty.

But another reading is possible. Different employments might mean differential inputs: number representations on the one hand and facial representations on the other. If there is one memory with different inputs then we can expect different outputs. This, however, does not explain the differential performance of the mechanism involved. If it is a truly horizontal mechanism the representations (be they of faces or numbers) are going to be uniformly processable by that mechanism, no matter what they are representations of. That is, no matter what the representations are representations of, those representations will be processable by the mechanism due to some properties—syntactic properties, say—of the representations that allow them to be so processed. Whatever those properties requisite for processing are, both representations of numbers and faces are going to possess those properties if there is a horizontal faculty in operation here.

Two other options are available. It might be the case that it is the memory's accessing the different modalities from which information is to be stored that affects how easily different types of information are stored. But that is another problem, separate from the question of the employment of the memory mechanism. This option does, however, call into question Gall's argument from intra-individual, cross domain, differences. The differences in intra-individual memory performance might result not from there being different memory systems, but from the different performances of, say, different domain specific perceptual mechanisms along the lines of Fodor (1983). It follows then that the argument from individual differences is not sufficient for Gall's claim here, since the data provided by individual differences does support various cognitive architectures. Anyhow, this remains a different objection from that introduced by Fodor since it's not the employment of the mechanism per se that causes problems for Gall.

The final option is that the one mechanism processes various information differently. This will automatically give Gall his point since there will be distinctive processing involving different types of information (numbers and faces, say) in accordance with the domain specificity of vertical faculties. So much for individual differences.

Fodor also questions Gall's methodologically important arguments from inter-individual differences. As we have seen Gall cites cases in which Smith and Jones might differ in musical ability. Fodor claims that this should
not lead one to postulate a special vertical musical faculty. Musical ability might just result from a certain "mix" of various horizontal faculties—or, for that matter, interactions between other vertical faculties. There is going to be an optimal mix for each of the vertical categories stipulated by Gall and there will be differences in the extent to which individuals approximate possessing that optimal mix (Fodor 1983 p. 19).

At the level of individual differences across a single cognitive domain Fodor’s response seems appropriate enough. But in the case of explaining individual differences across a variety of tasks Fodor’s reply breaks down. If Smith and Jones have approximately the same musical ability due to having similar “mixes” of horizontal faculties, it would seem to follow that they should also have roughly equivalent competences in other cognitive domains as well. Why? Because whatever the relevant properties of horizontal faculties are that contribute to a particular mix those properties will presumably remain constant across whatever cognitive domain is involved. What I have in mind here is this. Say the mix for a particular cognitive ability is two parts perceptual acuity, one part sensitivity with a dash of judgement. Now the strength or weakness of these individual faculties is fixed for the individual; that’s why some of us are more or less better at some cognitive skills than others for our entire lives. So no matter in what other mixes these faculties take part in they will be weak or strong. Suppose that some other ability consists of one part perceptual acuity, three parts sensitivity and lots of judgement. If Smith and Jones are equal in musical ability then we should expect that they would also be equal in this other ability as well, just because those very same horizontal faculties contribute to that other ability.

There’s a reply to this. It claims that we should not assume that Smith and Jones’ equal musical ability arises from equally strong or weak horizontal faculties. Maybe Smith is very “perceptual” and not very “sensitive” whereas Jones is just the opposite: not very perceptual but very sensitive; their respective equality of musical ability is just the averaging out of these differences. If this is the right story as to why Smith and Jones have equal musical ability then what guarantee do we have that this same averaging out will give equality of ability with respect to the other cognitive domain I have claimed they should also have equal abilities in? Perhaps differences in perceptual acuity and sensitivity will combine to generate different abilities in this other cognitive domain.

The trouble with this response is that although it might be the case that different proportions of horizontal faculties might affect the overall mix given constancy of competence within those horizontal faculties one needs to
show that such constancy will produce a different competence in the resultant mixed domain. Equally, I suppose, the defender of Gall would need to show that competence would remain constant for this other domain. But too little is really known about the psychological reality of these mixed non-fundamental faculties to decide which result would be forthcoming. I think the burden of proof lies here with Fodor. My challenge is that his argument against Gall only works when certain results obtain from comparisons across cognitive domains. Since it is not clear whether those results favour his case or Gall’s, I think his original argument falls short of the mark.

Just in case the reader thinks that the defender of Gall should be burdened with some of the proof, some actual cases from neuropsychology seem to provide arguments which count against objections based upon mixes of horizontal faculties such as Fodor’s. Take, for example, a hyperlexic. Such a child is characterised by developmental lags in which the onset of speaking and walking are delayed. The distinguishing feature of the hyperlexic is that by the age of three or four, despite slowness in other cognitive areas and without appreciable help from environmental factors such as parental intervention, he or she has mastered oral reading. Gardner recounts:

A boy aged four years and ten months ... could read a third grade level passage fluently despite woefully inadequate speech and understanding and an IQ in the mentally defective range; so much he enjoy this activity that he sought to read all materials in sight. A three year old youngster seen by these investigators could read newspapers aloud and also recited everything in sight including dictionaries and telephone books. (Gardner 1977 p. 135)

Gardner also quotes from a case study in which a six year old could not desist from reading:

Reading had the features of a compulsive ritual: he could not be distracted or easily deterred. If he were given a book, he had to start at the beginning, reading the entire title page, including the publishing house, date of publication, Library of Congress number, etc. He showed great resistance to interruption and would return later to this point and carry on the exercise. Gardner 1977 p. 135)

There is no evidence to suggest that what the hyperlexic reads aloud is comprehended:

Reading for him consists strictly in the oral decoding of visual graphemes: his prosody and accentuation do not vary with the sense,
nonsense words are read as effortlessly and emphatically as meaningful ones, jokes are not laughed at, commands are not honoured, and even references to the present situation may be missed. (Gardner 1977 p. 136)

Another case I want to consider is that of L. Again the quote is from Gardner.

This boy, eleven years of age at the time when he was most intensively studied, had an IQ of 50, placing him in the severely retarded range. Deficiencies of information and reasoning capacities notwithstanding, this freakish youngster could perform all sorts of numerical feats. He was able to remember endless series, such as railroad timetables and newspaper financial columns. He could immediately state the day of the week for any date between 1880 and 1950. Given twelve two-place numbers to sum, he came up with the total the instant the presentation was completed. His speed and accuracy at other arithmetical challenges were equally impressive. (Gardner 1977 p. 231)

L.'s numerical abilities were detected early; at the age of five years he could count by 2's, 4's, 8's and 16's. Another famous calculator was Fleury who, while being severely retarded, provided the cube root of 465,484,375 (=775) in thirteen seconds (Patricia Churchland 1986 p. 232).

What is peculiar about both the hyperlexic and idiot savants such as L. is the other cognitive capacities they exhibit. Whatever horizontal cognitive mechanisms we might want to attribute in such cases, the hyperlexic and idiot savant display extreme cognitive deficiencies in all other cognitive domains. If one wanted to explain the hyperlexic's proficient reading in terms of the mix of some set of horizontal mechanisms, one needs to show how that mix can be produced in the apparent absence of competence in any horizontal domain. There is no problem in explaining the mix in the case of individuals with an adequate spread of cognitive capacities since there are capacities out of which such a mix can eventuate. This need is especially evident in the case of hyperlexia. Any of the horizontal capacities we might want to attribute are just lacking in these cases. To attribute the child's ability to its "remembering" all the phoneme-to-grapheme group correlations necessary for the production of this incessant reading (in the face of the general language deficiencies evident) is exactly what we do not want to do. Again, we seem to have exhibited a range of cognitive abilities that varies across individuals,
where those abilities are unique in the cognitive economy of the subject. Hence some underlying mechanisms specific to those abilities.

3 Cerebral Localisation

Crucial to Gall’s programme is the idea that faculties are localised in specific organs within the brain. First organs and then localisation.

3.1 Mental Organs

Central to Gall’s thinking here is the analogy between cognitive functions and anatomical organs. Chiefly, Gall wanted to, and needed to given his contemporaneous intellectual climate, argue that the brain was the organ of the mind. The brain in turn gets subdivided into smaller organs each being responsible for particular cognitive functions. Given that other anatomical functions have particular organs of their own—seeing, hearing, salivating etc.—Gall asks “But, if she [Nature] has constructed a particular apparatus for each function, why should she have made an exception of the brain? Why should she not have destined this part, so curiously contrived, for particular functions?” (Young 1971 p. 19).

3.2 Localisation

Young defines cerebral localisation as “the doctrine that various parts of the brain have relatively distinct mental, behavioural and/or physiological functions” (Young 1971 p. 10). Localisation of function is hardly new; it is as old as anatomy and physiology. Early forms of localisation were inadequate; they were ventricular, speculative and based on the old faculty psychology. They localised function not in the solid regions of the brain but in the ventricles. They were non-empirical. The faculties postulated were derived from the Platonic division of the soul and mind. So, sensation and imagination were localised in the anterior, reason or thought in the middle and memory in the anterior ventricles.4 Later the centre of localisation was to shift from the ventricles to the solid cerebral structures. Gall obviously rejected the ventricular nature of the old faculties; but his most important contribution was to reject their speculative nature and in so doing reshape

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4Young (1971 p. 19). Other faculties postulated often included the will, judgement and language.
the kinds of faculties needed to be postulated. Much more will be said about localisation in chapter 9.

4 Empirical Methodology

One reason for Gall's rejection of the old faculties was that psychology thus far had been non-empirical. The faculties had grown largely out of philosophical psychology. It is Gall's chief contribution to contemporary functional psychology that he made the shift to an empirical methodology. Gall intended to replace the speculatively derived faculties postulated by the philosophers with empirically derived faculties which reflected the mental capacities of individual organisms and were the determinant variables in individual behaviour (Young 1971 p. 16). As Young says of Gall:

His conception of the domain of psychology makes their [the philosopher's—JF] categories quite useless. Gall's faculties are designed to serve a purpose quite different from those of the philosophers. He sees the goal of psychology as a differential one with its domain as the behaviour, roles, talents and differences of men and animals. (Young 1971 p. 18)

Gall thus argues:

None of the faculties mentioned, describes either an instinct, a propensity, a talent, nor any other determinate faculty, moral or intellectual. How are we to explain by sensation in general, by attention, by comparison, by reasoning, by desire, by preference, and by freedom, the origin and exercise of the principle of propagation; that of the love of offspring, of the instinct of attachment? How explain, by all these generalities, the talents for music, for mechanics, for a sense of the relations of space, for painting, poetry, etc.? (Young 1971 p. 18)

The existence of the philosopher's categories is not denied by Gall. He claims they are merely abstractions and generalities:

they are not applicable to the detailed study of a species, or an individual. Every man, except an idiot, enjoys all these faculties. Yet all men have not the same intellectual or moral character. We need faculties, the different distribution of which shall determine the different species of animals, and their different proportions of which
explain the difference in individuals. All bodies have weight, all have extension, all are impenetrable in a philosophical sense; but all bodies are not gold or copper, such a plant or such an animal. Of what use a naturalist the abstract and general notions of weight, extent, impenetrability? By confining ourselves to these abstractions, we should always remain in ignorance of all branches of physics, and natural history. ...From most ancient to most modern, they have not made a step further, one than another, in the exact knowledge of the true nature of man...(Young 1971 p. 18. Emphasis added)

The argument in this passage is not convincing. An abstract notion such as weight qua atomic weight would seem to provide the naturalist with a good criterion for taxonomising natural kinds, for instance. The important point for our purposes here, though, is the italicised sentence. By ‘empirical’ Gall means not that functional psychology should be interested in studying and accounting for brain localisation to behaviour correlations central to modern neuropsychology. Rather, he means that one should be studying and accounting for the intra- and inter-species differences of organisms. It is because of his erroneous assumptions (4) and (5) that Gall was to look for these differences in cranial prominences and anecdotal evidence involving striking behaviour. By and large, his neuro-anatomical studies played no part in his data collection. Where neuroanatomical details were included they were restricted to natural mutilations where the localisation had been established on other grounds (ie. cranial and anecdotal grounds). Gall rejected experimental ablation to either humans or animals, not for ideological reasons involving, say, vivisection, but various technical and theoretical reasons. Examples being the imprecision of his contemporary surgical techniques and the fact that the structure of the brain requires that “a part being wounded or irritated, wounds or irritates all the rest” (Young 1971 p. 49). However, no matter how imprecise are surgical techniques or how pervasive the non-isolatability of brain lesions, accidental mutilations of brain tissue are going to be less isolatable than experimental lesions. Again, Gall’s reasoning seems to lead him methodological problems.

The upshot of this is that Gall is left with the largely physiognomical empirical methods of craniology and anecdote collection. And these forms of data compilation led him into serious error. For instance, because of a protuberance over the ear was found on a medical student so fond of testing animals that he became a surgeon, the organ of destructiveness was placed there. And as Gross says: “the organ of amativeness was placed in the cerebellum because Gall had noticed that a passionate widow’s neck was hot to his touch!” (Gross 1985 p. 17). This uncritical methodology also allowed the
easy explaining away of counter-examples. The most amusing being that revolving around Descartes’ skull. The skull was found to have remarkably small anterior and superior regions of the forehead where the rational faculties were located. Spurzheim replied that Descartes was not as great a thinker as we had previously thought! (Young 1971 p. 43).

So despite his claims to be interested in the brain and not cranioscopy Gall’s methodology was empirical but it was at the same time physiognomical. His theory failed to be consistent with his practice.

5 Early Objections to Faculties

The idea that mental capacities depend upon the brain was around a long time before Gall. The principle of localisation that Gall was arguing for went beyond this mere dependence; on his account fundamental faculties could be located in specific areas of the brain. Gall’s chief critic in the nineteenth century was Pierre Flourens. Flourens also believed that the brain was the organ of the mind, and gave credit to Gall for the establishment of the point. He says:

the proposition that the brain is the exclusive seat of the soul is not a new proposition, and hence does not originate with Gall. It belonged to science before it appeared in his Doctrine. The merit of Gall, and it is by no means a slender merit, consists in having understood better than any of his predecessors the whole of its importance, and in having devoted himself to its demonstration. It existed in science before Gall appeared—it may be said to reign there ever since his appearance. (Quoted in Young 1971 pp. 20-1)

Despite this agreement and Flourens’ recognition of Gall’s contribution—in fact, Flourens held Gall in utter contempt—there is little else upon which they agreed. They had methodological disagreements. Flourens, for instance, was committed to experimental ablation (surgical removal of various components of the nervous system) in his data collection; his precise use of ablation becoming the standard practice in cerebral research. However, the psychological aspects of his work is not strong. He was particularly weak when it came to the behavioural consequences of his physiological experiments. He had no serious criteria for loss of function (Young 1971 pp. 30-1). Indeed, lacking from Flourens work is a set of psychological categories that his ablation experiments were designed to test. Gall recognised this deficiency when he said:
It would have been requisite to know what could be found, and what ought to be sought for, in the brain. It would also have been necessary, that the mutilators should be divested of every metaphysical prejudice; that they should have a detailed knowledge of the fundamental powers. Where is the physiologist, where, the anatomist, who has been able to follow this direction, and who has not wished to find generalities and abstractions. (Young 1971 p. 70)

Young claims that Flourens granted this point *in principle*, although there is no evidence of it affecting his research. (Young 1971 p. 71)

From the point of view of methodology, Gall and Flourens are interesting contrasts. Flourens was especially sound when it came to the physiology of the brain although his methods regarding the psychological affects of that physiological work are very poor. On the other hand, Gall’s acceptance of the cranium-brain correspondence meant that the physiological underpinnings of his programme were unsound. But he, at least, rejected an introspective appeal to establishing psychological generalisations and opted for an investigation of *overt* psychological phenomena (Young 1971 p. 73).

Part of the reason for these disagreements was the different heritages each saw himself working within. Gall saw himself reacting against the old introspectionist philosophical psychology in favour of a modern empirical psychology. Flourens saw himself as attempting to justify the philosopher’s categories of the mind from an empirical perspective. He says:

I frequently quote Descartes: I even go further; for I dedicate my work to his memory. I am writing on opposition to a bad philosophy, while I am endeavouring to recall a sound one ... “I remark here, in the first place,” says Descartes, “that there is a great difference between the mind and the body, in that body is, by its nature, always divisible, and the mind wholly indivisible. For, in fact, when I contemplate it—that is, when I contemplate my own self—and consider myself as a thing that thinks, I cannot discover in myself any parts, but I clearly know and conceive that I am a thing absolutely one and complete”.

Now here is the sum of Gall’s psychology. For the understanding, essentially a unit faculty, he substitutes a multitude of little understandings or faculties, distinct and isolate.

Gall reverses the common philosophy ... According to common philosophy, there is one general understanding—a unit; and there are faculties which are but modes of this understanding. Gall asserts that
there as many kinds of peculiar intelligences as there are faculties, and that the understanding in general is nothing more than a mode or attribute of each faculty. (Young 1971 p. 72)

It is seeing Flourens in this light, viz. as a Cartesian, that one can see his greatest disagreement with Gall. Of the plurality of faculties he says:

There are as many faculties as there are truths to be known ... But I do not think that any useful application can be made of this way of thinking; and it seems to me rather more likely to be mischievous, by giving to the ignorant occasion for imagining an equal number of little entities in the soul. (Young 1971 p. 71)

Gall's philosophy consists wholly in the substitution of multiplicity for unity. In place of one general and single brain, he substitutes a number of small brains; instead of one general sole understanding, he substitutes several individual understandings. (Young 1971 p. 71 & pp. 71-3)

He is willing to grant that men and animals show very different mental capacities (and again the Molierian objection):

No doubt of it. But what sort of philosophy is that, that thinks to explain a fact by a word? You observe such or such a penchant in an animal, such or such a taste or talent in a man; presto, a particular faculty is produced for each one of these peculiarities, and you suppose the whole matter to be settled. You deceive yourself; your faculty is only a word—it is the name of the fact—and all the difficulty remains just where it was before. (Young 1971 p. 71)

Flourens also claims empirical support for the thesis that "the cerebral hemispheres concur, by their whole mass, in the full and entire exercise of the intelligence" (Young 1971 p. 73). Flourens sees any qualification to the unity of the soul or its organs (ie. the unity of the brain) to be a denial of the existence of the mind.  

We can see in these passages the beginnings of CSR inspired objections. Flourens believes that there is one "general sole understanding", one single process underlying the mental capacities of individuals. Such a system has to be horizontal in nature since a unitary mechanism is

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5Young (1971 p. 73). Unfortunately, I cannot assess Flourens' actual arguments here since my source does not list them. At the time of this draft Flourens' original work was unavailable.
necessarily undifferentiated with respect to cognitive domains. If, as he says, the whole mass of the brain is exercised in the deployment of intelligence, then, in principle, we cannot strictly *localise* the capacity in any specific region of the brain. The 'strictly' is important here since Flourens allowed for localisation of function—his *proper action*—combined with a form of equipotentiality—his *common action* (Herrnstein and Boring 1965 pp. 221-3). His final assumption is that only a unitary cognitive system can yield a unified consciousness, mind, soul or whatever.

This brief summary of Flourens work has set the stage for the contemporary VFT-CSR debate. It also shows us that what is generally regarded as the tension between philosophical psychology and empirical psychology is far from a recent development.
Appendix B

Neuropsychology

The paradigm within which we have been exploring, contains a background assumption to the effect that the mind is, at least, supervenient upon, or stronger, identical with the brain. Given this assumption, it is not surprising that psychologists and neuroscientists alike have taken an interest in those unfortunate persons who have suffered some sort of brain damage, in the hope of learning something about the mind. Enter the cognitive neuropsychologist who attempts to develop theories about normal cognition from those suffering impaired cognitive abilities due to acquired brain lesions. What I want to do in this appendix is to peruse some pages of the neuropsychologist’s clinical notebook to see what light can be cast in the direction of the current dispute between VFT and CSR. Although the evidence available is somewhat indirect, I think it supports the principles underlying VFT.

1 Isolation by Brain Damage

The most common causes of brain damage induced cognitive dysfunction are strokes and cerebral intrusions such as automobile accidents. It is a neuropsychological commonplace that brain lesions produce very fine grained cognitive disorders. Let’s look at some in some detail.

1.1 Aphasia

Following Gardner, we may define aphasia as a disturbance in language function following injury to the brain. Characteristic of aphasia is not an across the board reduction in a person’s verbal capacity. Instead, capacities such as talking, understanding, reading and writing can be spared or
destroyed in relative isolation from each other. Some combinations of these capacities can be destroyed or spared at once or never at all. It is not even the case that more complex linguistic functions such as comprehension of long sentences are impaired before more simple functions such as repetition or following commands (Gardner 1977 pp. 52-3). Take, for example, what has become known as Broca’s aphasia. In this form of aphasia verbal comprehension usually seems more or less spared whereas speech production is vastly impaired. The Broca’s aphasic is reduced to the use of content words; a huge disparity exists between his or her ability to name objects and the ability to use grammatical terms. ‘If’, ‘and’ and ‘but’ are almost totally inaccessible. Thanks to relatively intact comprehension the Broca’s aphasic can answer questions such as ‘Does a stone float on water?’ and ‘If I say “The lion has killed the tiger” which animal is dead?’.

Contrast this affliction with a second form of aphasia: Wernicke’s aphasia, named after Carl Wernicke who in 1874 hypothesised that the posterior portion (front and second temporal convolutions) of the left temporal lobe controlled language comprehension, as opposed to Broca’s area of the same lobe which controlled language production. A person suffering this affliction is likely to utter the following collection of sentences:

Oh sure, go ahead, any old think you want. If I could I would. Oh, I’m taking the word the wrong way to say, all of the barbers here whenever they stop you it’s going around and around, if you know what I mean, that is tying and tying to repucer, repuceration, well, we are trying the best that we could while another time it was with the beds over there the same thing ... (Gardner 1977 p. 68)

Such word salads are common with this type of aphasia. They articulate extremely well with excellent intonation and syntax. Accompanying this lack of coherent speech is an eerie nonawareness on the part of the sufferer that they have any language deficit. They also perform at a level no better than chance when answering the type of questions posed to Broca’s aphasics. As to following commands, some preservation of comprehension is evident. One of Gardner’s patients, for instance, could consistently carry out “whole body” commands such as “stand up”, “turn around three times” and “assume the position of a boxer” while being unable to follow commands regarding specific limbs. “Raise your hand”, “touch the chair” and “make a fist” all went unexecuted (Gardner 1977 p. 72). In the case of naming objects difficulty increased with diminishing familiarity. ‘Book’ and ear’ would be recalled
whereas only attempts could be made at less familiar objects such as ‘paper’ ‘fork’ and ‘ankle’.  

According to Gardner, in general, Broca’s aphasia and Wernicke’s aphasia represent complementary clinical patterns. The former’s strengths—comprehension and general intellectual capacity—are the weak points of the latter. While the latter’s preserved capacities—the ability to articulate easily, the inclusion of grammatical particles in speech—are lacking in the former (Gardner 1977 p. 75).

A third form of aphasia called anomia presents a very different clinical picture from those forms just mentioned. According to classical aphasiological theory anomia results from damage to the angular gyrus—that portion of the brain in which outputs from the various sensory systems are associated. The anomic displays neither set of deficits exhibited by the Broca’s or Wernicke’s aphasic. To the casual observer the anomic might appear to possess normal language capacities. On closer inspection though, a striking impairment is evident. If asked to name some common objects in a room the anomic is at a loss. When Gardner pointed to a clock, for example, his patient responded: “Of course, I know that. It’s the thing you use for counting, for telling the time, you know, one of those, it’s a ...” (Gardner 1977 p. 76). In cases of severe anomia the sufferer has difficulty in naming objects and if told the name the sufferer exhibits considerable uncertainty about whether it was in fact the correct name. As it turns out, anomics can often choose the correct name for an object from a group of words—although he or she would remain uncertain about the choice.

Perhaps the most fascinating aspect of anomia is that the inabilities just described are evident only when the name of an object is required to be produced in isolation from its usual context. So the anomic might well say “Would you mind passing me the chair?” when at the same time he or she is totally unable to produce the word ‘chair’ if requested. The classic case is reported by Gardner. Due to anomia a person is unable to produce ‘no’ on request. After many futile attempts the patient throws up his hands in disgust exclaiming “No. No! I told you I can’t say ‘no”. Even after such an outburst the patient remains unable to produce that word on request (Gardner 1977 p. 78).

In order for a word to be produced out of context, it is thought that a person must be able to abstract away from the present concrete situation. This flexibility is what is thought to be lacking in the anomic. In examination of anomic speech one can detect a certain concreteness, to get lost in minute

\[1\text{See Gardner 1977 p. 70 for a description of the attempts made. There seems to be some evidence here for the role of phonemic representation in verbal recall tasks.}\]
detail sometimes at the expense of the general point. In interpreting proverbs, for instance, there is a distinct tendency toward “concrete thinking”. In interpreting the saying “Too many cooks spoil the broth” one of Gardner’s patients was unable to rephrase it in order to bring out its meaning. His attempts were: “Too many cooks, you know, cooks standing around the broth, they are talking and cooking” and “If you want things to turn out the right way, you’ve got to be careful about that sort of thing” (Gardner 1977 p. 79).

1.2 Alexia

Alexia comes in at least two forms: pure alexia and alexia with agraphia. Pure alexia is the condition in which one loses the ability to read writing. A person with alexia with agraphia can speak and understand perfectly, but cannot read, write or spell. Gardner cites the case of pure alexia from the end of the last century, of a French businessman, call him C., who had a rare stroke which made it impossible to see objects in the right half of his visual field and was responsible for his alexia. Gardner recounts:

Surprisingly, however, C. was able to express himself without difficulty, to recognise and name instantaneously obscure technical and scientific instruments, to understand everything said to him, to recall the most minute detail of past events. Even more astonishing, he could still write without difficulty, both expressing his thoughts spontaneously and transcribing what was dictated to him; yet he was quite unable to decipher his own handwriting, unless he could independently remember what he had written or had been dictated to him. In fact, he preferred to write with his eyes shut, for he got “tangled” when he monitored his own writing. When letters were etched on his hand, or when his fingers were guided through the air in the form of a word, he could instantly identify these verbal materials. In short, all his language functions, including writing, were preserved with the exception of the decoding of words and letters presented to his eyes. (Gardner 1977 pp. 115-16)

... He continued to play cards, for he recognised the different numbers and suits; furthermore, he was able to read numbers, to perform elaborate calculations, to follow his business and stock market investments. Why the particular lesion in his brain should destroy recognition of verbal and musical symbols, while sparing numerical ones, remains nearly as mysterious to neurologists today as it was to
C., but at any rate his capacities in the numerical realm were not in the least impaired. (Gardner 1977 pp. 116-17)

Certain written materials were understood by C.. However, these materials were not understood as a result of reading as such. As Gardner says:

To be sure, when shown the French newspaper L'intransigeant he was unable to read its title in a fifteen minute trial; but when shown the newspaper Le Matin, he immediately called out its name. He could not identify the individual letters 'R' and 'F', but when a circle was drawn around 'RF', he instantly reported "Republique Francaise". C. himself explained that these recognitions were due to an appreciation of the "form" or "picture" which these symbols assumed. Identifying Le Matin was not a matter of deciphering seven letters or three syllables, but rather the recognition of a familiar shape; ... C. was reading these signs in the same way that a Westerner might learn to recognise a Russian signature despite ignorance of the Cyrillic alphabet, or a child the name of a restaurant by the appearance of its billboard or marquee. (Gardner 1977 p. 116)

Contrast this with the alexic with agraphia. He or she loses this ability to understand letters or words as "graphic entities".

An interesting variation to alexia occurs in the case of polyglots. In one case the patient was totally unable to read his first language, English. He could, however, read some Latin and read Greek perfectly.

Before the case of C. there had been no anatomical evidence for the causes of alexia. Luckily, a clear clinical profile of C. was available with which to compare post mortem evidence. It appears that the left half of C.'s visual field was intact as was the centre for language normally located in the left hemisphere of right handed individuals. Because of the stroke, C. was, supposedly, unable to transmit information from the preserved part of his visual system in the right hemisphere to the areas of the left hemisphere where lexical concepts are stored in the language centre. Thus, C. could name letters aloud, write, and recognise letters by touch since the pathways connecting the tactile as opposed to visual system to the language centre were intact.

But why could C. retain the ability to recognise and name people, objects and especially numbers? An answer is offered by Norman Geschwind. In studying a case of pure alexia without agraphia in the early 1960's, Geschwind and Fusillo discovered that pure alexia is accompanied by "an
Appendix B

inability to match seen colours to their spoken names” (Geschwind and Fusillo 1966 p. 146). They say:

The patient failed in all tasks in which he was required to match the seen colour with its spoken name. Thus, the patient failed to give the names of colours and failed to choose a color in response to its name. By contrast, he succeeded on all tasks where the matching was either purely verbal or purely nonverbal. Thus, he could give verbally the names of colours corresponding to named objects and vice versa. He could match seen colours to each other and to pictures of objects and could sort colours without error. (Geschwind and Fusillo 1966 p. 144)

Such deficiencies are not perceptually based, despite the patient’s insistence that objects looked grey when they were in fact some other colour. The patient passed pseudo-isochromatic tests of colour vision, outscoring one of the examiners who was moderately red-green colour blind. The patient’s confabulatory responses regarding colour names (i.e. insisting that a red object was in fact grey) results from the speech centre’s being disconnected from that area of the right hemisphere of the brain undergoing the visual experience because of damage to the corpus callosum. The colour name offered by the examiner which is heard and hence processed in the left hemisphere cannot be compared with the colour seen which is processed by the right hemisphere.

Nor can this inability result from loss of verbal memory for colour names since the patient correctly named objects corresponding to named colours; the patient admits that bananas are yellow and paper is white but insists that these particular instances are a different colour.

The reason why reading and colour naming are lost whereas object naming and number recognition are not seems to be that letters and colours are accessed through the visual modality only; written words and colours have visual but not tactile associations, for instance. So when the informational links between the visual centre of the right hemisphere and the language centre in the left hemisphere are severed in the damaging of the splenium of the corpus callosum, the naming centre cannot access the visual centre. In contrast to this, objects in the world and people may have associations involving other sensory modalities including tactile, auditory and kinesthetic. In the classical pathology of pure alexia without agraphia those portions of the corpus callosum (viz. those portions anterior to the splenium) which carry nonvisual information and in particular tactile information from the right to left hemisphere are intact. So the access of the centre involved in
naming to the tactile centre is unaffected. Hence, naming of objects with tactile associations is possible.

Numbers, of course, do not exist in the world like objects and people. But unlike letters, words and colours, numbers have strong tactile associations—at least according to some neuropsychological hypotheses. Consider (and this can be no more than anecdotal evidence) the role of the fingers in the learning of numbers and how to count. So the retention of the ability to read numbers as well as naming of objects is explained by the continued access of the left hemisphere to right hemispheric nonvisual information.

1.3 Prosopagnosia

Disorders of recognition are called agnosias. As with aphasia, the existence of a general disorder here is open to dispute. More interesting perhaps, since the data is more clear cut in this case, is a particular form of agnosia—prosopagnosia. Prosopagnosics can identify objects, are intellectually intact, and possess normal language functions. They remember the individuals they have known, and can recognise them from their voices, descriptions of their appearance or from some idiosyncratic feature such as moustache, glasses or hat. Where the prosopagnosic fails is in the recognition of the person’s face. Even close relatives or the sufferer’s own face will seem totally unfamiliar. They will often express disbelief at being told the identity of what appears to be a stranger. They may also comment that the individual, even the patient himself, has drastically changed since they last met.

Our chief question with prosopagnosia is: is there a special mechanism which underlies the recognition of faces? Some support for a confirmatory answer comes from studies of the recognition of inverted faces (Gardner 1977 p. 155). Modern tensor network theory also provides a means for such a capacity, in which if the particular state-space sandwich in which facial features are represented is destroyed, there will be a selective impairment of this capacity alone. However, in order for there to be a unique facial recognition mechanism, lesions to this area of the brain would have to impair this capacity alone. But this does not seem to be the case. When it comes to fine discriminations such as distinguishing bird species or kinds of leaves, the prosopagnosic also runs into difficulty.

In defense of the existence of prosopagnosia it might be argued that the ability to make such fine discriminations results from the ability to make

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2See Gardner (1977) ch. 4 for a full discussion.
3See P.S. Churchland (1986) ch.10 for details.
fine discriminations amongst facial features. So the advantage from an evolutionary perspective in the positing of a distinct facial recognition capacity in terms of mother-child relations and conspecific recognition might be generalised to operate in other cognitive tasks. A multipurpose state-space sandwich might well perform such generalised tasks.

2 Split Brain Studies

The human brain is divided into two hemispheres. It is possible through natural malformation and surgery for the connections between these hemispheres to be severed. By severing the corpus callosum and/or the anterior and posterior commissures one can cut the informational access of one hemisphere to the other. When such surgery, called commissurotomy, is carried out, usually in an attempt to reduce intractable epilepsy, there have been some interesting behavioural side effects. Much of the data resulting from experiments performed on these “split brain” patients has produced a popular split brain mythology I will not discuss here. For present purposes, though, I restrict my attention to aspects of split brain studies that have a direct bearing upon the VFT-CSR debate.

A word of caution before we start. Not all split brain patients exhibit the same pathological histories. Some, due to brain lesions early in life, developed significant linguistic skills localised in the right hemisphere (which is rather uncommon for right handers). Moreover, the type of commissurotomy performed varies across patients. Some have the corpus callosum sectioned together with the anterior commissure, while others have that commissure spared. There is also the possibility of information exchange between the two hemispheres via other commissures and, indeed, the midbrain and brain stem at which points there are no hemispheric divisions.4

In human vision (as well as higher vertebrates) there is a division of labour in visual processing. Information from a person’s right visual field is processed by the left hemisphere and information from the left field processed by the right hemisphere. In normal individuals this division of processing poses no problems since the visual information being processed in each hemisphere is relayed to the other hemisphere via the commissures. But in the post-operative commissurotomy patient this information exchange is blocked. Thus, the opportunity to study two independent visual processing systems and the resultant affects upon behaviour this independence may bring about.

4For fuller description of some of the caveats associated with this line of data acquisition see P.S. Churchland (1986) p.174.
The main source of experimental data regarding visual processing comes from the use of the tachistoscope. By having a patient focus on a fixed midpoint on a screen with a projector flashing different images to each vertical side of the midpoint, the image to the left of the midpoint will be restricted to the left visual field (hemifield) and hence restricted to right hemispheric processing, while the image to the right of the midpoint is restricted to the right hemifield and hence to left hemispheric processing.

With this set up the word ‘key’ is flashed to the right hemifield. If asked to identify the visual stimuli, split brain patients have no problem; they would say “key” correctly. But if that word is presented to the left hemifield, and hence restricted to right hemispheric processing, the patient would fail to say “key” and if asked what was seen they would deny having seen anything. This is exactly what we would expect given the account of language production given above. If visual information cannot be transferred from the right to left hemisphere where language functioning in general and speech production in particular takes place then the subject naturally would fail to give a verbal response to the stimuli.

Interestingly, if the subject, having denied the presence of stimuli in the left hemifield, is asked to pick an item from a collection of hidden objects with one’s left hand (remembering that the right hemisphere has motor control over the left half of the body) the subject correctly picks out the key.

It is concluded from these results that the reason the subject fails to verbally respond to stimuli in the left hemifield is due to the left hemisphere’s being the linguistically competent hemisphere. But notice that the word ‘key’ was flashed to the right hemisphere for processing and not, say, a pictorial representation of a key. Thus the right hemisphere would seem to have some language comprehension skills. Otherwise, the subject would be unable to pick the key from the collection of objects.

As a variation on this experiment the patient is asked to draw with his or her left hand a picture of a word flashed. Typically, these patients protest that the request is pointless since nothing was shown on the screen. When coaxed to have a go at drawing something the patient accurately draws a pictorial representation of the referent of the word presented. Upon producing the drawing the patient will exclaim that he or she does not know why it was that particular picture drawn, and hypothesises that something must have been presented on the screen. Clearly, in these cases there must be linguistic comprehension evident on the part of the right hemisphere (Patricia Churchland 1986 pp. 176-7).

There has to be verbal comprehension on the part of the right hemisphere since in each experiment the subject understands a task command.
Assuming that the task commands are received by both hemispheres from auditory information presented to both ears, and that the exchange of auditory information processed by a language input system (wherever it is) is blocked due to the commissurotomy, then if a command is carried out under the control of the right hemisphere, then that hemisphere only could have processed that auditory information. It is not possible for the left hemisphere to have processed the command and then passed on control of the output behaviour resulting from the original stimuli to the right hemisphere since, ex hypothesi, the connections between the hemispheres have been cut. Patricia Churchland claims that task commands are not purely verbal, and that non-verbal cues accompany the command (Patricia Churchland 1986 p. 184). In order for this to count against the right hemisphere's having linguistic comprehension it would be necessary to show that the non-linguistic component of the task command is sufficient for the subject to understand the command. I am unsure that this is the case.

It was initially hoped that split brain studies would provide the basis for distinguishing between particular styles and content domains of cognitive processing at an inter-hemispheric level. In particular it was hoped to show that the left hemisphere contained the centre for language and processed information somehow analytically whereas the right hemisphere was superior in the synthetic processing of spatio-visual tasks. But this hope appears to have been dashed; it might well only be the case, and there even exists contrary evidence for this claim as well, that speech production is localised to one hemisphere only.5 If that is the case then we have good reason to infer that speech production is a cognitive process that can function relatively independently of much other cognitive processing. This is exactly what the VFT predicts.

Another tachistoscopic experiment uses chimeric stimuli (Patricia Churchland 1986 pp. 177-9). Two pictures of half faces are joined together to form a whole face, with each half face being restricted to each hemifield. Thus, the left hemisphere would process the right half face and the right hemisphere would process the left face. Upon presentation of the stimuli, the subject was required to choose from a set of faces the one originally seen. The results showed that faces presented to the right hemisphere were more often identified than those presented to the left hemisphere. Whether or not the left hemisphere performed near a chance level I am unsure. But even on the model of localisation considered here, some recognition capacities might be evident in that hemisphere thus increasing the performance beyond mere chance. If these results are genuine then attempts to localise the major

5See Gardner (1977) Ch. 9 for some counterevidence.
mechanism for facial recognition in the temporal lobes of the right hemisphere are going to be justified. This further supports the claims of the VFT.

The disconnection effect brought about by commissure sectioning can also be simulated in normal individuals. Because not all individuals have motor control for speech localised in the same place, it is necessary to determine which hemisphere possesses this control before any major brain surgery to proximal areas of the brain is undertaken. By injecting a patient with sodium amytal (a suppressor of neural activity) into either carotid artery (which exclusively supply blood to either right or left hemisphere only) it is possible to anaesthetise one hemisphere while the other remains active. If the left hemisphere is anaesthetised in this way the right side of the body normally becomes unresponsive whereas the left side remains active. The tester places an object in the left hand of the subject and asks her to remember it. After the effects of the drug wear off the subject is asked what was placed in her hand. The patient normally looks puzzled and denies that anything was placed in her hand. This is what we would expect since the left hemisphere where language is represented has not accessed the tactile and visual information present in the right hemisphere. But when the subject is shown a group of objects containing the held object she is instantly able to identify it (Patricia Churchland 1986 pp. 193-5).

The interesting point to notice here is that upon the return of an operative language processing hemisphere, the information in the possession of the right hemisphere is not subsequently accessed by the language system even when the subject consciously attempts to retrieve it. This suggests that there are distinct mechanisms controlling language function and visual and tactile functions. Access to previously stored information is restricted to the mechanism alone and not some other mechanism in the cognitive system. The language system can only get access to such information at the time of the information's arrival in the system and not later.

Where does this body of data leave the VFT? Well, I think that Michael Gazzaniga, a leading split brain researcher, has the correct intuitions, at least, regarding the evidence:

Clearly what is important is not so much where things are located, but that specific brain systems handle specific tasks. We begin to see that the brain has a modular nature, a point that comes out of all the data. It is of only secondary interest that the modules should always be in the same place. A correlate of this is that much of split-brain research should be viewed as a technique to expose modularity. That is, it is not important that the left brain does this or the right brain does that. But
It is highly interesting that by studying patients with their cerebral hemispheres separated, certain skills can be observed in isolation. It is a hugely significant point. (Gazzaniga 1985 pp. 58-9)

These words seem a fitting way to end this work. We can see that the original hope of finding interesting localisation of function at the interhemispheric level has given way to a basically cross-hemispheric localisation of particular functions. Cutting the cognitive pie in half is far too coarse a division of function if we are to explain the data of cognition. We also see the real importance of modularity in cognitive theorising; the importance of the neuropsychological data is not the emphasis on localising cognitive functions, but the ascertaining of what those basic functions might be. Split brain studies provide evidence for modularity generally. The degree of grain of domain upon which these modules operate is unclear though.
Bibliography


—(Forthcoming). “Perceptual Plasticity and Theoretical Neutrality: A Reply to Jerry Fodor,” manuscript.


—(IR). “Information and Representation,” manuscript.


—(1988). "Functionalism and Broad Content," Mind XCVII.


Kitcher, Patricia (1980). "How to Reduce a Functional Psychology" Philosophy of Science 47.


—(OWITH). "On What's In the Head," manuscript.


