

ARTICLE WITH PEER COMMENTARIES AND RESPONSE

Cognitive development as an executive process – in part: a homeopathic dose of Piaget¹

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Abstract

Piaget's theory of cognitive development retains its importance through showing us how the exercise of agency is necessary to the development of self–world dualism and to the developing ability to frame explicit judgements about the physical and mental world. I begin by describing the Piagetian position in my own terms (Agency: Its role in mental development, 1996, for a fuller account) and then set it in the context of research on the executive functions. I also argue, however, that the theory lacks the resources to explain second-order mental representation. The theory was insufficiently nativist in general and insufficiently nativist about symbolic capacities in particular. But adopting a nativist view does not preclude one from taking a Piagetian line on the essential contribution of the first-person experience of agency to cognitive development.

I shall be arguing that the true value of Piagetian theory lies in its giving us a philosophically grounded account of those aspects of cognitive growth that are explicable in terms of the development of executive functioning, but that much of the remainder of the theory can be jettisoned because it is insufficiently nativist. The executive element is essential if we are to make sense of the development of self–world dualism and also to understand why children struggle with certain formal tests, though this is a fairly small feature of the theory's complete architecture (cornerstones are small). It is a 'homeopathic' dose in the following senses. First, while it is small it is potent. Second, because applying this dose shows us just how much of mental development remains unexplained this might help us to recover from the illusion bequeathed to us by Piaget: the thought that cognitive development can be explained in terms of one monolithic process. Those in the grip of this illusion think that if mental development is not the interiorization of structured actions, then it is

module maturation, increases in processing efficiency and capacity, network training, 'representational redescription', internalization of socio-linguistic practices, theory development, and so forth – *tout court*.

In the past 20 or so years, Anglo-American developmental psychology has taken up a number of stances towards Piagetian theory. In the first place, varieties of so-called 'neo-Piagetian' approaches have borrowed one element of the theory and moulded it to their ideology. Thus, North American information processors (meaning cognitive psychologists whose hardest-working concepts are memory capacity and operating efficiency) have retained the idea of discontinuous cognitive change while ignoring much of the remainder (e.g. the work of Juan Pascual-Leone, Robbie Case and, to a lesser degree, Robert Siegler). For many (e.g. Karmiloff-Smith, 1993), such approaches are hardly Piagetian at all, although they do put necessary pressure on the theory to attend to what Jean Piaget ignored – processing limitations. In a similar way, those who view development from a neural network perspective want to treat Piaget as a kind of proto-connectionist by focusing on his concern with organism–environment interaction

¹ Some of the material in this paper receives a more thorough treatment in my *Agency: Its role in mental development* (The Psychology Press, 1996).

and his rejection of representational nativism (Elman, Bates, Karmiloff-Smith, Parisi & Plunkett, 1997). While this approach can be very illuminating (e.g. McClelland's (1989) parallel between Piagetian accommodation and learning by back-propagation) it casts into outer darkness Piaget's logico-mathematical structures. This is of course inevitable, given that the formal structuring of thought is exactly what connectionist theories struggle to explain.²

No one can deny that these approaches are fruitful, nor that work such as that of Graham Halford (e.g. 1993), which has a foot in both the neo-Piagetian and the connectionist camps, is rich and provocative. But we are dealing here with fairly specialist enterprises. Contemporary cognitive science is more likely to reject Piagetian theory out of hand as hopelessly empiricist and naively vitalist, while viewing the task of cognitive developmental psychology as that of identifying varieties of innately specified module (e.g. Leslie, 1987, in psychology; and Fodor, 1992, in philosophy). Alternatively, the developmentalist may be agnostic about, or impatient with, the apparatus of Piagetian theory and focus merely on the phenomena he identified (e.g. failure to conserve properties). Or the developmentalist may construct a theory severely anti-Piagetian in intent but which is nonetheless imbued with some of the Piagetian qualities most difficult to admire – such as an over-reliance on metaphor and on protean concepts. Here I have in mind Gopnik's child-as-scientist project (Gopnik & Meltzoff, 1997).

In this paper I will argue for a different approach. It can properly be called 'Piagetian' because it stands squarely by the claim that mental development depends upon the first-person experience of agency and because it says that mental development from infancy to maturity cannot be understood unless we view the developing individual as engaging in progressively more sophisticated forms of mental action (as opposed to, say, module maturation or theory change). It also retains the idea that framing explicit judgements about reality can be viewed as a form of self-regulation. But it rejects Piaget's ideas about the 'constructed' origin of what he called 'the symbolic functions' and rejects his ideas about the role that language acquisition plays in cognitive development and about the mechanisms that underlie syntactic development and logical thought.

This approach takes Piaget's theory and renders it down to a potent residue without which – I shall argue – any general account of cognitive development will be

²See note 12 on how Piaget regarded neural networks of a rather different kind. I discuss the shortcomings of purely connectionist approaches later in this paper.

moribund. Like the neo-Piagetian and the connectionist approaches, this approach also aims to modernize the theory. For it is the theory to which one turns for an account of mental development as progressively more adequate executive functioning³ – meaning the control of physical and mental action. The study of executive functioning is now a staple of cognitive neuropsychology and of information processing psychology (Roberts, Robbins & Weiskrantz, 1996; Rabbitt, 1997; for recent collections). I shall call this approach the 'executive-Piagetian' theory and will later contrast it with the 'historical-Piagetian' view.

The first part of the paper will describe the Piagetian claims about development I take to constitute this 'potent residue', the second will view these more explicitly in the context of executive functioning, while the final section will say something about the failure of the historical-Piagetian theory – of bootstrapping theories more generally – to explain symbolic representation and the second-order thought which this enables.

1 The executive-Piagetian theory

The essential claim is this. No matter how rich the perceptual input and no matter how many and varied are the innately specified modules, subjects will fail to develop mentally if they cannot alter their perceptual inputs at will. This is necessary to the development of self-world dualism and continues to play a major role throughout development. First-person experience of agency is taken to be dependent upon the maturation of the executive systems but itself determines the nature of experience; and so to that extent the theory is interactionist.

It will later emerge that this 'altering at will' is not something that can only be achieved by physical action (such as eye movements) because it encom-

³I should stress that this is very far from being the first paper in which it is argued that cognitive development can be viewed in executive terms. And I am grateful to a referee for pointing out that as long ago as 1962 Herbert Simon argued that Piagetian stages can be regarded as 'increasingly complex and powerful executive structures' (1962, p. 159). More recently, a number of developmentalists have presented general theories of cognitive development cast in executive terms, with Case (1992), Dempster (1992) and Roberts and Pennington (1996) being among the most notable. These theories, inevitably with different places of emphasis, are mainly concerned with the roles of inhibition, working memory and strategy selection. As will be seen, the present approach shares these concerns, especially with the joint demands of prepotent-response inhibition and working memory. Where my approach clearly differs from these is in its concern with the role of agency in self-world dualism, construed as something that continues to develop well beyond infancy. Moreover, my starting point is more Piagetian and it works from broadly philosophical premises.

passes shifts of attention – which the Piagetian will construe as mental action. These can be understood as shifts of perceptual attention or as shifts of cognitive focus from one aspect of a situation or problem to another or from one domain (broadly construed) to another.

What does this ability give the developing subject? With regard to sensorimotor processes, agency provides the subject with two kinds of perceptual sequence: those that are brought about by acting and those that are not. Borrowing Kant's terminology and examples, the subject thereby enjoys perceptual sequences that are both self-caused (e.g. looking at different portions of the front of a house) and world-caused (e.g. watching a ship sail upstream). This is the pivot upon which constructivist theories of cognitive development turn. Consider its implications for object permanence. If – this is a big 'if'⁴ – one takes the view that there can be no conception of an external world unless objects are regarded as capable of resisting the will, one can see how important is the self-caused/world-caused distinction for the attainment of a concept of a mind-independent world. Only a subject who can alter his or her perceptual inputs at will can experience inputs as being *constrained* to be one way or another. Thus, only a subject who is free to look at different parts of a scene can experience the fact of being constrained to see, say, a tree with each rightward glance. If, on the other hand, the subject can change none of these inputs then no constraint can be experienced,⁵ in so far as an experience of constraint would seem to be unavailable to a subject none of whose perceptual inputs are under his or her control.

As we take the next step, more recognizably Piagetian ideas will come into view. As Kant pointed out, the perceptual sequences to which actions give rise are *reversible* whereas those brought about by world-caused events are not.⁶ At this point we need to distinguish between two senses of 'reversible' and appreciate why the first of them is the more interesting and important – and is the one lying at the heart of Piaget's theory. For Piaget scholars they correspond to what Piaget called

negation (as an operation in group theory),⁷ on the one hand, and the use to which he put J.M. Baldwin's notion of 'circular reaction' on the other; 'circular' because the action (e.g. pulling a string attached to a mobile) can be *repeated* at will without the infant's having to wait upon an external trigger.

In the first sense, reversibility means cancelling out – having looked at A then B we look back to A – and the kinds of action that are reversible in this sense are those that have an attentional character. Attention can of course be *drawn* to an aspect of an object or to a location or sound but it can also, and is more usually, directed by the subject. This corresponds to the distinction, made by students of visual attention, between endogenous and exogenous shifts of attention in adulthood (Spence & Driver, 1996) and in infant development (Johnson, 1994). *Endogenous* attention is what is now at issue. Shifts of visual focus have a similar character because, like attentional shifts, they can be negated. Reversibility-as-negation ('R-negation' henceforth) is, then, something that is entirely within the agent's control and it is something that has no effect on the world – only on the experience of it.

I now turn to the other kind of reversibility. At least one of the following must be the case for there to be reversibility-as-repetition ('R-repetition' henceforth): the action is *not* within the agent's control from initiation to fruition; the action *does* have an effect on the world. Take the example of diving into water. This obviously has a physical effect and it is not entirely within the diver's control: the diver launches the action as best she can after which the laws of physics take over. It can be repeated at will, like a circular reaction, but unlike, say, a shift of visual focus from left to right, the same action cannot be performed in reverse (as when a movie of the diver is played backwards). Similarly, signing one's name does have an effect on the world and it can be repeated at will but it is not something which can be reversed in the sense of done *in* reverse – done backwards. Speaking is an action which can be repeated at will, but we cannot speak backwards, and it often has an effect on the world, on sound waves and a distal effect on other agents or speech-synthesizing computers and so forth.⁸

⁴ See T. Baldwin (1995) for a defence of this view.

⁵ The sense of 'constraint' here is supposed to capture the fact of experiencing oneself as being constrained to perceive the world in a certain way; though obviously only a very 'thin' notion of self is in play. I am not saying that a non-agent could never experience forms of constraint. For example, a non-agent could perceive that objects are constrained to behave in certain ways. In fact, this is how one may describe the results of dishabituation experiments – discussed later.

⁶ This term was used to describe Kant's distinction by at least one commentator (Körner, 1955).

⁷ And as Piaget scholars will also know, Piaget himself distinguished between two kinds of reversibility: reversibility by *negation* and reversibility by *reciprocity* (Piaget & Inhelder, 1969, pp. 136–140). My 'reversibility by negation' is meant to encompass these two because the fact of cancelling out is what needs to be emphasized.

⁸ On this criterion, eye movements can have an effect on the world in so far as eye-balls are objects. But of course eyes are organs of perception whereas speech is not.

Addressing myself once more to Piaget scholars, R-negation versus R-repetition maps onto the distinction which Piaget drew, when discussing concrete operational tasks, between the ‘reversibility’ that enables the concrete operational child to overcome mental fixation on a salient variable (e.g. the height of a liquid), on the one hand, and ‘empirical return’ (e.g. pouring the liquid back into the original container) on the other. He viewed the former as the fundamental process. It is a quintessentially *mental* process.

For a psychologist sympathetic to Piagetian theory, there are three major advantages to be gained through focusing on R-negation rather than R-repetition: (1) the ‘Achilles heel’ in the theory is cured; (2) the notion of altering experiences at will can properly be explicated; (3) the case for saying that temporal and spatial concepts are grounded in agency comes to look more plausible.

(1) The apparent Achilles heel in any theory which claims that mental development is action dependent is that intellectual development proceeds normally in children with severe motor impairments (Jordan, 1972). It is not uncommon to see this fact being used as a stick with which to beat Piagetian theory (e.g. Boden, 1978). But Piaget is not concerned – at least the executive-Piagetian is not concerned – with motor development, with how effectively the developing child can manipulate objects, can generally displace his or her body and disarrange and rearrange his or her immediate environment. This is R-repetition. What the executive-Piagetian is concerned with is R-negation, which does not depend upon motor control in the sense used in experimental psychology – in the sense in which endogenous attention is not a form of *motor* control.

(2) Agency was earlier defined as the ability to alter perceptual inputs at will, and the claim was also made that the notion of reversibility illuminates the ‘at will’ in this. It is R-negation that plays the crucial role here, not R-repetition. One might say that R-repetition (the ability to repeat actions on objects and repeat ‘launched’ actions) might engender in the subject a stronger sense of himself as the determiner of his proximal environment, and thereby enriches his experience of objects as refractory entities. But in this case what is happening is that the agent is altering the displacement of objects (including his own body) and *thereby* altering his experience. In R-negation, by contrast, experience is not only being altered directly – while the world remains unchanged – but the agent is in control of these changes from initiation to fruition.

For this reason, R-negation leaves no room for the idea of altering experiences *by accident*. By contrast, in R-repetition we all too often bring about the displacement of objects by accident (dropping things, or falling in rather than diving).

Take a simple example of R-negation. An infant is in a room containing his mother, some furniture and a window. He glances from his mother to the window and then back to his mother, then to the sofa, to the window and back to mother again. Whatever neural processes are responsible for the infant’s achieving this, the psychological result will be, speaking metaphorically, that he will be experiencing himself as responsible for what he is seeing from moment to moment. At the same time, he will experience a form of constraint: every (say) left-ward glance results in his visual field being filled with a window whether or not he desires that input. As I shall later be discussing, having such experiences will drive a wedge between experiential data that, descending still further into metaphor, are ‘my products’ and those that are ‘not my products’. (The metaphor will be explicated later.)

(3) What can R-negation contribute to the infant’s developing conception of time and space? In the first place, consider the kind of temporal experiences that would be unavailable to a subject for whom R-negation was impossible. The flow of percepts in this subject would be in a single direction and so ‘casting the mind back’ would be impossible. For this reason one would naturally think of such a creature as living in the continuous present. To expand, the ability to R-negate overt visual attention (such as glancing from A to B then back to A) is something that can engender the experience of a datum as being predictably available for re-perceiving. While this does not imply that the subject regards A-experiences as being caused by a mind-independent object or location, it does imply at least some awareness of A’s being at an egocentrically defined datum to which it is possible to return. This casting-the-attention-back is an overt analogue of casting the mind back in memory in so far as they both involve gaining independence from present input via an intentional return to a different phenomenology. While a subject who could not engage in R-negation could feel certain mental images that came to mind to be *familiar* (such as an unwilled mental image of X) he could never bring any of them to mind while having the capacity to revisit the present, as it were, ‘in the background’. The subject would therefore lack a conception of the past as that which can mentally be visited and quitted at will. In any event, the executive-Piagetian takes R-negation to cover mental as well as

physical action, and so R-negation covers active recollection.

I turn now to the dependency of spatial concepts on R-negation, keeping in mind the question of whether it is possible for subjects incapable of R-negation nonetheless to infer their location in space relative to other objects. Imagine a subject thus impaired stationed before three objects A, B and C, with B being directly in front of him and with A to his left and C to his right. After seeing B in front of him he turns his head to the right and sees C, but, being incapable of R-negation, is unable to make the equal and opposite movement that will reintroduce B. It is clear that if this action is not R-negatable – I don't of course mean merely through motor incapacity – then the subject cannot have succeeded in representing B as being to the left of C and before his own body. The representation of egocentric spatial relations would seem to depend upon a capacity for R-negation. It would seem to require the capacity for re-experiencing data through actions that are specified in purely egocentric terms, such as by head movements n degrees in an X direction.

Does this, in fact, boil down to no more than the claim that R-negation is a product of an innate allocentric (perspective-independent) conception of a space plus motor skill? Perhaps it means nothing more than that all R-negatable actions are grounded in an allocentric, action-independent representation of space? In answer, spatial knowledge would *indeed* seem to require an action-independent capacity for the allocentric representation of spatial properties. But this is to concede nothing. On the present view there can be no allocentric representation of spatial properties – no conception of a spatial layout that does not include the subject within it – unless there can also be an egocentric conception of space, in the sense of a perspective-relative conception in which the subject knows which data will be experienced if certain actions are taken. For an allocentric experience of space to be *conceived* as such it must be conceived – I am assuming – as the opposite of an egocentrically specified one. R-negation is, for the executive-Piagetian, necessary for the latter.

Let us take a broader view of the A, B, C example – broader in that we consider the subject as not merely incapable of R-negation but incapable of intentionally altering his perceptual inputs in any way at all (with or without reversibility). On the present view, a subject could only infer his position in space from afferent information alone if he *already* possessed some understanding that what he perceived was contingent on where he was currently located in space. Thus, the

subject could only infer from afferent information that B was in front of him and C to his right, and so forth, if he had some *prior conception* of himself as a body which could be spatially located before objects. Imagine further that the subject's perceptual inputs did indeed change and did so in two ways: either the three objects moved from left to right or the subject himself was moved from left to right. Without the ability to affect the afferent data by efferent commands how could this subject distinguish between the two kinds of sequence? All that would be experienced would be changing inputs.⁹

Object permanence

We are now well placed to appreciate why the attainment of object permanence (knowledge that objects continue to exist when they are not currently being perceived) must be understood in terms of the exercise of agency. In a nutshell, the reason is that my knowledge that what there is before me is a mind-independent object causing me to have such-and-such experiences cannot be understood apart from my knowledge of how my experiences can be affected by my actions. Such knowledge depends upon my having what might be called 'structured expectations' about the phenomenal results of actions. Thus:

- (1) If the object does not move then my eye and head movements will cause certain very tightly specified phenomenal changes. If I glance upwards, for example, the object will move downward in my visual field in an equal and opposite direction.
- (2) If the object does not move by itself it can be re-experienced after I have looked away from it.
- (3) There are very strict limits on how I can bring about phenomenal changes in an object. I cannot, for example, make it larger simply by staring hard at it, nor change its colour by blinking.
- (4) I know how to change my physical relation to the object. If I want it to be closer to me I move towards it; if I want to distance myself from an object on my right, I move to the left, and so forth.

These facts bear upon my knowledge that objects continue to exist when they are occluded in the following way.

- (1a) I know that if the object remains behind the occluder and if the occluder does not move then

⁹This is of course only a sketch of an argument; for a full version see Brewer (1992) and Russell (1996, Section 2.4).

(say) lateral movements will eventually cause the object to become visible again; and if this does not happen then the object has moved.

- (2a) I know that certain kinds of re-appearance described in (1a) can be R-negated: that I can re-experience occlusion by reversing the action.
- (3a) I know that the kinds of action that can cause re-appearance are very tightly specified. Thus, moving towards the occluder can never cause re-appearance; moving away from it can cause re-appearance; movements relative to the occluder will eventually cause re-appearance unless the occluder is enveloping the object; if the occluder is enveloping then its manual removal will cause re-appearance.
- (4a) I know that if I wish to see or grasp the object again then there are certain things I have to do and that what they are is constrained by the fact that I am here and that the occluder is between me and the object. I know that the object does not just continue to exist *somewhere*, but has a particular spatial relationship to me and to the occluder.

All this implies that the following three facts cannot all be true of an agent at the same time.

- He knows where the occluded object is in relation to him and in relation to the occluder.
- He is able to move about at will.
- He has no idea what to do to re-experience the object.

Something has to give: one of these three will have to be severely qualified. The empirical implications of this view are clear.

- Infants' knowledge of object permanence should be assessed in terms of their actions on objects, not exclusively in terms of their *reactions* to the behaviour of objects.
- The development of object permanence will turn out to be correlative to the development of agency.

So how does the executive-Piagetian deal with the copious amount of data telling us that very young infants, perhaps as young as 4 months, react in an appropriate way to events in which the principle of object permanence seems to have been violated (Baillargeon, 1994)? In a much-discussed example, infants of 4 months of age dishabituate to the sight of a screen failing to be impeded by a wooden block which has temporarily been occluded by the screen's backward movement (Baillargeon, 1987). It would surely be unwise for executive-Piagetians to dismiss these data as being irrelevant to the development of object permanence or even to commit themselves to the kind of

general scepticism about the familiarization–dishabituation design recently articulated by Bogartz, Shinsky and Speaker (1997)¹⁰ or to more local scepticism about the 'drawbridge' experiment (Montangero, 1991; Wakeley & Rivera, 1997). This is because it is impossible to see how object permanence could develop through the exercise of agency *alone*: some representational capacities must provide the necessary base. I have already mentioned the necessity for action-independent abilities in allocentric spatial coding. To this must be added the nervous system's capacity for maintaining a representation of objects (strictly of 'object-data') for at least a short time beyond the instant in which they become occluded. In fact it is impossible to imagine how a nervous system that could not do this would function. Within the visual modality, such a nervous system would be like a mirror which simply reflected the changing kaleidoscope of visual input.

It might appear that I am dealing with data that seem fundamentally to challenge Piaget's most important claim by 'kicking it downstairs' to the level of subpersonal¹¹ processes. Indeed critics could say that, because I am painting Piagetian ideas about object permanence with the broad-brush of folk psychology, I find it easy to place data which tell us about 'the nervous system' outside this picture. Giving a thorough answer to this objection would require a lengthy excursion into the difference between personal-level and subpersonal-level explanation in this area (Russell, 1996, pp. 121–129). What I will do instead is to say that the goal of the executive-Piagetian¹² is to explicate the notion of agency in subpersonal terms, to marry the foundational claims just outlined to what we know about the neuropsychol-

¹⁰One of the main critical points made by Bogartz *et al.* is that, typically, habituation–dishabituation designs confound responses to impossibility with responses to novelty. They also present data showing that it is no harder to familiarize 5-month-olds to impossible events than to possible but novel events.

¹¹The distinction between personal and subpersonal levels of explanation is owing to Dennett. The personal level is the level on which we explain behaviour in terms of intentions, beliefs, thoughts, reasons, feelings and the like (the level of 'folk psychology'). On the subpersonal level we have, in Dennett's words, the 'behind-the-scenes machinery that governs speech dispositions, motor subroutines, information storage and retrieval, and the like' (1978, p. 216). Neural processes are obviously at the subpersonal level.

¹²Piaget himself was clearly interested in backing up his ideas by reference to the subpersonal level. Perhaps the best example of this is his (Piaget, 1971, pp. 221–223) reference to McCulloch and Pitts's work on neural networks – among the earliest of its kind. They discovered, in Piaget's words, 'isomorphic links with the sixteen functors of the bivalent logic of propositions' (p. 222). McCulloch and Pitts's neural networks are a different kind of entity, however, from those employed by contemporary cognitive modellers.

ogy and neurobiology of action. My next section will say what this means.

2 Executive functioning as ‘what develops’

As the name suggests, the construct ‘executive system’ refers to the system for controlling physical and mental action, for launching goals and bringing them to fruition; it is a system whose brain locus is, broadly but not exclusively, the prefrontal cortices. Executive functioning is impaired after prefrontal lesions, and it is also severely impaired in both schizophrenia and autism, as well as (in different and weaker ways) in some other disorders. The ‘executive functions’, which are normally taken to encompass instigation, inhibition, planning, monitoring, and controlled (as opposed to automatic) processing, ensure that behaviour is not merely a product of habit, encapsulated schemas and salient features of the environment but is determined by novel information emanating from the external environment and original schemas emanating from the internal environment. The executive-Piagetian says that the knowing system – the system of concepts and reasoning – as well as selfhood cannot be understood apart from the functioning of this executive system.

The executive functions are conceptually interrelated, and so it is unwise to think of them as discrete neuroanatomically specifiable systems (and certainly not as ‘modules’). Each is defined in terms of the demands of a certain set of tasks. For some, this fact encourages a rather despairing attitude. Thus Rabbitt (1997, p. 1) writes: ‘it has passed unrecognised that the hypothetical components of “executive behaviour” ... which are, in fact, simply descriptions of task demands, may have poor construct validity because although these demands appear logically different they can be met by identical production system architectures’. But the executive functions (listed above) are *not* ‘logically different’; they are logically, or at least conceptually, deeply related; and this is exactly what one would expect from facets of a single system. Indeed there cannot be one of the listed functions without all the others. One is tempted to go as far as saying, with Piaget’s *groupment* in mind, that they form a kind of ‘agency-group’ with inhibition playing the role of ‘negation’ and sequential planning the role of ‘associativity’. Further elaboration will over-stretch the metaphor but it is an illuminating one nevertheless. Moreover, and as the subsequent chapters in Rabbitt’s edited book testify, we are making substantial progress in understanding the neurobiology and cognitive psychology of the executive functions. It has proved a far from impossible task, in fact, to

investigate which features of the executive system are most impaired in a given disorder or form of neural decay or insult.¹³ This is what I and others have been engaged in with respect to autism (edited volume by Russell, 1997a). This work will be discussed a little later.

The goal is now to consider the prospects for a subpersonal account of the processes fundamental to Piagetian theory – executive processes in the present terms. It is not surprising that the lower the executive processes are the more available subpersonal accounts of them become. One of the basic executive processes is action monitoring. I will now describe its bearing on the processes described in Section 1.

Action monitoring – from lower to higher levels

At the most primitive level, the process of action monitoring is equivalent to the production of an efference copy (Jeannerod, 1997, pp. 168–171) of a motor command for comparison against the afferent changes which result from the movement. Without such a system, creatures as lowly as fruit flies (von Holst & Mittelstaedt, translation 1973) and fish (Sperry, 1943) would fail to distinguish perceptual changes brought about by their own movements from those caused by environmental changes. However, while R-negation obviously depends upon efference copying, a moment’s thought tells us that it is no more than a necessary feature because this mechanism could function – in the case of flies it surely only functions – when the organism is *reacting* to stimulation rather than acting. What is distinctive to R-negation, however, is the fact that it is endogenously caused: it has at least a primitively intentional character. That the phenomenal effect of the action can be cancelled out is testimony to the fact that it is not dependent upon current stimulus configurations. We now need to consider the kind of subpersonal mechanisms that might underpin R-negation, that could underpin intentional motor behaviour in general.

One mark of motor behaviour which is more than reactive is that it involves the nervous system’s making predictions about the afferent results of efferent commands. In the case of the fly, all that is required is that the fly’s nervous system should copy the motor command to move the head n degrees in a certain direction. If the world then moves n degrees in the opposite direction, the

¹³ This statement might appear to contradict what I have just said about the elements of the executive system being conceptually related. There is no contradiction in fact. The elements of arithmetic (addition, subtraction, multiplication etc.) are conceptually related, but it is possible to imagine very specific kinds of impairment in one of these functions. For example somebody might be very good at multiplication but impaired in subtraction.

efferent and afferent changes are cancelled out and the animal's nervous system codes the phenomenal changes as self-caused. However, for actions to be minimally intentional they have to be launched on the basis of a *prediction* about what the world will look like or feel like or sound like when the action is taken. If this could not be achieved then there would be no goals and thus no reasons for acting. Moreover, behaviour could never become increasingly skilful with practice unless the subject knew something about the results of certain actions in advance of launching them. One could never learn from one's errors unless one knew in advance the kinds of actions that could pull one away from error and towards success. For this reason, connectionist models of motor skill commonly include what Jordan (1990) has called a 'forward model', meaning a subcomponent of the network that feeds information from 'action units' to output units that specify predicted environmental outcomes and sensations (Brown, Britain, Elevevåg & Mitchell, 1994, for discussion). This is of obvious relevance to what was said earlier about object permanence and the role of 'structured expectations' in this. These structured expectations are, at least to some degree, something which could be simulated by forward models (Russell, 1996, pp. 150–157, for what more is required).

We are moving therefore some way towards the goal of explicating the executive-Piagetian theory in sub-personal terms. The next step is more difficult.

While the role of efference-copying mechanisms in human motor control is universally acknowledged – the locus of the comparator is likely to be the cerebellum (Stein & Glickstein, 1992) – and while forward models are becoming increasingly important in research on human motor control (Wing, 1996), there is little consensus around my next claim. In making it I shall claim common cause with C. Frith (1991).

In attempting to explain the occurrence of delusions of alien control and of auditory hallucinations in schizophrenia Frith proposed that high-level intentions ('plans' for Frith) and thoughts expressed in inner speech are efference-copied in a similar way to that described above. For every intention and for some kinds of thought¹⁴ a copy is made which comes to be matched against its registered result: there is intention monitoring. If no copy

¹⁴ Some thoughts, as Campbell (1998) points out, cannot be said to share this character with actions. Thoughts often follow a rational sequence such that the content of a thought at time-2 is a function of the logical structure of the thought at time-1, rather than being something which the thinker produces *de novo*. But I have in mind cases unlike this where an emotion or long-term goal might cause one, given certain background conditions and immediate stimuli, to launch a certain thought, as it were, in isolation. Similarly, when there is cognitive effort (e.g. being asked to name a city that begins with the letter S) Frith's parallel between thinking and action would seem to hold.

is made, or if one is made but the flow of information (between whatever centre generates the intentions and the comparator) is interrupted, the consequence will be that the agents will feel themselves to be performing actions which they did not intend, will find themselves thinking thoughts that are not their own, and will find that thoughts which they projected into the minds of other agents (as we all do) are heard as those agents' real, broadcast thoughts. On this view there is a division in schizophrenia between the will and action, and one may add that the will comes to be reified by patients with a religious upbringing as 'God'. (A person with schizophrenia may thus be unable to tie his shoelaces until God has told him to do so.) This hypothesis can account for the fact that schizophrenics confuse their own actions with those of others. For example, hallucinating schizophrenics are more likely than controls to judge others' actions as their own (Deprati *et al.*, 1997) and more likely to say that the experimenter has generated words which they themselves have generated (Bentall, Baker & Havers, 1991).

Frith's hypothesis marries well with the present position. But it is controversial. Deprati *et al.* (1997) present some alternative accounts of the self–other agency impairments in schizophrenia and Campbell (1998) points up some conceptual difficulties with the idea that verbal thoughts are launched like intentions. Indeed for some the very idea that every intention is copied by the brain is simply absurd: why should Mother Nature be so wasteful?¹⁵ But blanket dismissal of the idea simply betrays a failure to appreciate what has to be achieved subpersonally for an action to be intentional. If what we do intentionally, by which I include many (though not all) forms of thinking, is to *be intentional for the agent* it is not merely necessary that the actions be caused by the agent himself. It is not enough that the actions were his responsibility *in fact*. They must also be experienced by the agent *as* his responsibility. Frith's application of the efference-copying model to higher-level intentions would seem to achieve this, although it is clearly not the only possible hypothesis. Finally, it should be stressed that action monitoring is certainly not a form of self *observation*: it is not a kind of reflexive self-awareness. For if an agent has consciously to observe himself to see what he is doing then a feeling of responsibility for his actions is exactly what must be *lacking*.¹⁶

¹⁵ I am thinking of a conference I attended at which a well-known American philosopher protested from the audience: 'Why copy an intention? The intention itself is enough. The whole idea is absurd.'

¹⁶ As O'Shaughnessy expresses it, 'If one is to relate as an observer to anything one has to be "without" it whereas if one is intentionally to do anything one has to be "within" the action we are attempting to observe, in which case we have an entirely empty and self-delusive experience of observation' (1980, p. 31).

What both the low-level and the high-level mechanisms have in common is that they require a form of working memory, a mechanism for keeping intentions on line while the actions that they cause are being executed, together with a mechanism in which 'conditions of satisfaction' (Searle, 1983) are matched against goal-relevant information in working memory. There is nothing especially Piagetian about all this (either 'executive' or otherwise) but in the next two sections I will tie these ideas down to processes in normal development by considering the demands of executive tasks.

The twin demands of executive tasks – developmental examples

A little reflection tells us that there is much more to successful action than holding representations of current intentions on line and comparing the result of generated actions with their conditions of satisfaction. A creature that was an action-monitor and nothing more could thrive only in a world *without novelty* in which objects and events *never evoked inappropriate behaviour*. It could only survive in the psychological version of a frictionless atmosphere. This illustrates that successful action must not only be self-generated and monitored; it must also be flexible, must be responsive to environmental demands made *de novo*. Indeed it can only be flexible if force of habit and the potential for the environment to trigger inappropriate responses can be overcome.

This can be made more concrete by considering what are arguably the two essential features of executive tasks. Two simultaneous demands are made: (1) a response which has been rendered 'prepotent' through past learning or by salient features in the environment must be inhibited at the same time as (2) novel, action-relevant information is held in working memory. Consider a developmental example of an executive task.¹⁷ In the task used by Hughes and Russell (1993) the child is placed before a box with a hole in it. Inside the box and visible through the hole is a desired object, such as a ball, resting on a plinth. The child's task is to retrieve the ball. However, a direct reach will cause the ball to disappear because the reaching hand will break a light beam, thereby triggering a mechanism that causes

the ball to drop down inside the plinth. What must be done before reaching is to throw a switch on the side of the box that turns off the light beam. A successful retrieval of the ball requires, therefore, both (1) inhibiting the prepotent response of making a direct reach while at the same time (2) maintaining in working memory the need to first throw the switch. It is clear that action monitoring is involved in both of these processes. The inhibition of a prepotent response is itself a form of action and so its achievement requires monitoring. But the role of monitoring in the performance of the switch-then-reach sequence is rather more obvious. The child (children of 3 years generally succeed) must maintain this entirely novel goal in working memory and represent its conditions of satisfaction.

In short, the executive system enables not only the launching and monitoring of goal-directed actions but also the inhibition of inappropriate actions with low thresholds of activation and the immediate performance of unlearned actions.¹⁸ In this way, executive tasks distil the kinds of demand that are made in the stream of everyday life. In social interaction, for example, we often have to suppress what we are currently interested in or suppress attention to the information that we happen to find salient in a situation and attend to what somebody else is saying or doing. This information is certainly 'action-relevant' because it determines what we must say and do next. More broadly, the salient information is that given by the here-and-now while the action-relevant information is any self-generated plan.

Piagetian and other developmental tasks construed as executive tasks: executive competence versus executive performance

It is hardly a distinctively Piagetian claim that executive functioning develops with age. Nobody denies this, and least of all those who assume the existence of innate modules, who sometimes argue that success on a certain task comes relatively late in life because development of the executive system has failed to keep pace with the maturation of these innate modules (Leslie & Thaiss, 1992). But when the executive-Piagetian says that executive functioning develops with age he is not simply saying that children's executive difficulties can *mask* a competence which is already there. Rather, the development of executive functioning can bring a form of knowledge to fruition. We have already seen how this works for object permanence in general. Now we need to descend to the finer grain and consider one of Piaget's

¹⁷ One of the classic executive tasks is the Wisconsin Card Sort task. In one version of this the subject, having been presented with a pack of cards, has to sort them under certain categories such as colour, number and shape. When the subject has sorted ten cards under one of these categories the experimenter gives him feedback that he is wrong and that he has to change the category. Frontal patients sometimes continue with the original category in spite of this.

¹⁸ See my brief discussion of the Norman and Shallice (1986). Supervisory Attentional System in the final part of the paper.

search tasks in order to contrast a Piagetian with a non-Piagetian way of saying that the infant fails it 'because of executive difficulties'.

In the A-not-B task, infants begin by retrieving, or by witnessing the retrieval of, an attractive object from behind an occluder. After this has been done a number of times the experimenter transfers the object from behind the initial occluder (A) to a new occluder (B). Infants below about 12 months of age continue to search at A so long as they are not allowed to search immediately after its concealment behind B. This is 'an executive task' both *a priori* and on the grounds of what we know of its neurobiology. In the first place, the task requires the simultaneous inhibition of an action which has become prepotent by virtue of its habitual¹⁹ nature and the holding in mind of novel information (its transfer to B). With regard to the neurobiological justification for calling the task 'executive' we can turn to the work of Diamond and her co-workers (Diamond, 1996; Diamond, Prevor, Callender & Druin, 1997) on children with early-treated PKU (phenylketonuria). Their dietary treatment, while preventing the severe intellectual retardation which would otherwise occur, results in the depletion of dopamine in ascending neural circuits which we know to be responsible for executive control. These children are impaired on the A-not-B task both with visible and invisible displacement, as well as being impaired on a number of tasks with the executive structure described above. Indeed their impairment is specific to these kinds of task (Diamond *et al.*, 1997).

It is necessary to note the strong contrast between what Diamond and others mean by calling this task 'executive' and what it means for the executive-Piagetian to do so. In the former case, the failure is regarded as a *performance error* whereas in the latter case it is regarded as a *competence error*. That is, Diamond assumes that an underlying competence in object permanence is in place, but that it is masked by the performance error which poor executive control encourages. In contrast, while admitting that this kind of error may indeed be a performance error in some cases (in patients with frontal lesions²⁰ – see below on PKU children) the executive-Piagetian says that, in the normally developing child at least, it is a competence error. The claim here

¹⁹ Obviously 'habitual' must have a broad meaning here given that children make the error after watching the experimenter hiding the object at A. One can perhaps say that it is attending to this event that has become 'habitual'.

²⁰ Sometimes frontal patients say that they know what they are doing is wrong but that they cannot help doing it. This is obviously an executive performance error.

is that if we take object permanence to be constituted, at least in part, by having the right kind of structured expectations about the phenomenal effects of actions then one must regard making the A-not-B error as evidence that this knowledge is still undergoing development. The caveat to be entered here is that this is not a conceptual proposal but an empirical one. For while the proposal is grounded in a more or less philosophical view of what it means to have knowledge of the external world, the resulting claim is an empirical one and can be overturned by data²¹ (on which see Russell, 1996, Section 2.7).

I now turn to another task that, while being *prima facie* a test for understanding that false beliefs can lead to erroneous behaviour and thus of 'theory of mind' development, can also be said to have an executive structure. In this task, devised by Wimmer and Perner (1983), the child hears a story about a protagonist who puts a desirable object (e.g. some chocolate or a coin) into a cupboard (A). After the protagonist has departed, somebody happens to move the object to another cupboard (B). The protagonist returns and the child is asked either where the protagonist thinks the chocolate is now located or where he or she will look for it. There are also memory control questions. Broadly speaking, 3-year-olds are worse than chance on this task and 4-year-olds are better than chance. In executive terms, failure consists in the child's telling the experimenter what he himself believes (object at B) or, more broadly, in his framing an answer in terms of what is 'up front in consciousness' (in the phrase of Flavell, Flavell & Greene, 1983). This is the prepotent response which has to be inhibited.²² At the same time the child has to hold

²¹ In a similar way Chomsky produced an empirical theory of language acquisition out of a non-empirical account of the structure of language (at least the only data employed concerned native speakers' grammatical 'intuitions').

²² I will mention two kinds of evidence for this view. First, the false belief task can be regarded as a conflicting belief task: there is a conflict between the subject's true belief and the protagonist's false belief. Three-year-olds (Moore *et al.*, 1995) and children with autism (Russell, Saltmarsh & Hill, in press, Experiment 1) are equally challenged by a task in which they have to answer questions about conflicting desires (self versus other). In so far as desires, unlike beliefs, do not represent the world as being a certain way this can be regarded as evidence that the false belief task is not failed by these subjects because it requires an understanding of the representational mind. Both tasks are difficult, on the executive theory, because the child must suppress the tendency to answer in terms of his or her own mental state (belief or desire). Second, children with autism find a version of the false photograph task (Zaitchik, 1990) of equivalent difficulty to the false belief task (Russell *et al.*, in press, Experiment 2). On the executive theory, this is because it makes similar executive demands to the false belief task despite the fact that it does not test for the possession of mental concepts.

in working memory the fact that the question concerns the protagonist's actions or state of mind and to do so in the context of this prepotency.²³

As mentioned earlier, Leslie and Thaiss (1992) have argued that executive difficulties of this kind mask an underlying conception of false belief. For the executive-Piagetian, on the other hand, the situation is more complex and the failure more profound. For if the 3-year-old is routinely unable to think about another's motives and beliefs whenever they diverge from his own motives and beliefs, then his everyday *conception* of other minds will inevitably be immature. This is better regarded as a competence error than as a performance (or masking) error, as least as long as one takes the view that possessing a concept implies a capacity for doing something with it (on which see Peacocke, 1992). Allied to this is the claim that development of the executive system enables children to think *explicitly and at will* about their own and others' propositional attitudes in relation to a known reality. This is not to say that endogenous changes in executive control *cause* a theory of mind to be acquired, but rather that the acquisition of a theory of mind cannot develop adequately without these endogenous changes taking place. The theory assumes, then, that there is a growth-spurt in executive functioning towards the end of the third year of life and predicts that we will find evidence of a maturational spurt in those areas of the prefrontal cortex that are associated with executive functioning as defined here – such as the dorsolateral prefrontal region (Diamond, 1991).

In Piaget's concrete operational tasks, or at least in the central ones such as conservation and class inclusion, it is again possible to identify the need to inhibit a prepotent response at the same time as holding in mind information relevant to framing the correct answer. In some respects Piaget's actual account of the transition between preoperational and concrete operational thought was explicitly executive. What children have to achieve, for Piaget, is cancelling out the 'centration' of their thought (*qua* their cognitive rather than visual attention) on uni-dimensional changes – on the height of the water level in liquid conservation for example. This is, of course, a cognitive version of what I have been calling R-negation; and added to this there is the

²³ Many other 3–4 transition tasks have this structure. In the appearance–reality task (Flavell *et al.*, 1983), for example, children have to suppress an answer in terms of what is salient to them (e.g. the fact that an apparent stone is really a sponge) and answer in terms of the object's appearance. Moreover, a number of other mental-state tasks may be failed because the child's natural reaction is to guess the location of the object rather than process the relevant language (Moore, Pure & Furrow, 1990).

necessity for prising thought away from potent but misleading information. Studies in which two non-conservers have to agree an answer to a conservation question when viewing the display from symmetrically conflicting perspectives (e.g. Russell, Mills & Reiff-Musgrove, 1990) illustrate the nature of this failure of decentration. It is not that the child cannot conceive that the beaker is also narrower as well as taller or conceive, say, that the other pencil would be judged taller from a symmetrically conflicting perspective,²⁴ but rather that he or she cannot shift the cognitive spotlight from one dimension to another and back whilst framing a judgement about abstract properties like amount or length.

An executive performance account of this failure would be that children really knew the correct answer all along but that weak executive control lured them to error.²⁵ An executive competence account will agree that the non-conserving child does not literally believe that amounts change in reality when they change perceptually,²⁶ but will additionally say that if the child cannot frame the correct answer for reasons of weak executive control then he clearly is unable to think about quantity, area, number, and so forth, explicitly and at will in the same way as an older child. The younger child's knowledge about these abstract properties will be unsocialized because it cannot be accessed in answer to a question; and so it will accordingly lack the intersubjective character of a true concept.

Turning to cognitive development in adolescence, a case can be made for weak executive control underlying the difficulties children typically have, and which many adolescents and adults continue to have, with Piaget's formal operational tasks. What failure at formal operational tasks has in common with failure at concrete operational tasks is its being caused by 'stopping the thought too early' – by being satisfied with the first idea and not considering alternatives to it. We can also predict, as was the case for tasks such as conservation

²⁴ More recently we have found that members of non-conserving dyads are perfectly happy to keep changing their minds – at least five times – about which stick in a length conservation task is the longer of the two as they swap positions at the table (Russell, unpublished data). This experience does not encourage conservation answers on a post-test.

²⁵ Russell and Haworth's (1988) finding that the same kind of peer-interaction data are produced in appearance–reality tasks (where children *know* the reality) is consistent with this view. Mehler and Bever's (1968) studies of conservation in very young (around 2½) children which imply that non-conservation is a kind of false theory that children acquire around the age of 3 years are also consistent with it.

²⁶ It is very unlikely indeed that children believe that perceptual transformations literally increase the amount. We do not see children displacing their sweets so they will have more to eat.

and class inclusion, what this first thought will be. In the balance-beam task the child will tend to ignore the weights' distance from the fulcrum and frame the answer entirely in terms of the relative weights; in the inclined-plane task the child will attend to the angle of the plane and ignore how far up the chute the ball has been placed; in the probability task the child will ignore the relative number of desired versus undesired items within the bags and attend to the absolute number of desired items; in the shadows task the child will ignore the object's distance from the screen and attend instead to its width.²⁷ One can therefore refer to a 'prepotent response' in these tasks, given that one variable is psychologically more potent, and given that exclusive attendance to this variable guarantees failure. Note that McClelland's (1989) connectionist model of the balance-beam task had to build in the fact that relative weight carries more potent information than distance from the fulcrum.

Moving beyond Piagetian reasoning tasks, it is uncontroversial to say that adults' difficulties with hypothesis-testing tasks are also rooted in their tendency to stop their thought too early because they are seduced by vivid information. The most famous example of this is the failure of between 80% and 90% of university students on the Wason Selection Task (Wason, 1960).²⁸ What failing subjects seem to be content to do, according to Evans (1989) at least, is to allow the contents of the question to determine their choice of hypothesis-testing cards.²⁹ In fact this 'stopping too soon' is a more general description of what Johnson-Laird (1983) refers to as the poor reasoner's being content with the first 'mental model' that he or she generates of a problem. Relatedly, one can see older children's and adolescents' tendency to interpret statements that are true or false in virtue of their form (tautologies and contradictions) as empirical statements (Osherson & Markman, 1975; Russell & Haworth, 1987) as a case of processing their salient surface form – of failing to try to 'stand back' from them mentally.

²⁷ See a series of studies by Siegler (1981) on these tasks. Siegler argues that performance is very task-specific, although a study by Ferretti Butterfield, Cahn & Kerkman (1985) reports a healthy correlation between performance on the balance-beam task and performance on the inclined-plane task.

²⁸ In the most common version of this task, subjects have to test the rule that 'If there is a vowel on one side of the card there will be an even number on the other side of the card' by turning over the minimum number of cards. They typically neglect to turn over the card with an *odd* number on it, preferring to turn over the card with an even number.

²⁹ That is, subjects turn over the kind of cards mentioned in the instruction, e.g. 'even number' or '4'.

There is then scope for an executive-Piagetian rather than historical-Piagetian view of the cognitive changes in adolescence. It is this. Even quite young children have a fair idea of what it means to test hypotheses (Giroto, Light & Colboure, 1988) but they lack the executive capacities to consider variables and hypotheses beyond the first one – the one determined by the prepotent information. This is certainly a matter of failing to inhibit, and it is accompanied by a failure to maintain more than a small number of variables in working memory. Is this an executive performance or an executive competence development? This time a performance account looks rather more plausible. Certainly, *adults'* failure on the Wason Selection Tasks looks very much like performance failure: when it is explained to them, they acknowledge their error.³⁰ Some of children's failure on formal operational tasks may also be failures of executive performance.³¹

This general approach to adolescents' and adults' performance on reasoning tasks bears comparison with Duncan's (1995) proposal that general intelligence as assessed on IQ tests (*g*) is, in part, a function of executive control. He reports strong correlations between the level of *g* in normal adults and their performance on a computerizing executive task requiring the maintenance of a simple goal (without requiring reasoning). Goal-neglect is related to low *g*. Broadly, Duncan's theory and the executive-Piagetian theory both emphasize the degree to which successful reasoning is a matter, as it were, of 'keeping the mind in gear'.

The developmental consequences of inadequate executive functioning – the case of autism

It was suggested earlier that the executive-Piagetian theory is not undermined by the fact that early motor impairments do not result in subnormal intelligence. This is because action, as the executive-Piaget takes it to be, is not equivalent to motor skill. But the theory obviously has to predict that early executive dysfunctioning will result in abnormal development of some kind; otherwise it is vacuous.

³⁰ The common experience of running practical classes on this task with undergraduates is that they accept correction. There is no reason to believe that this is simply compliance with a higher authority.

³¹ Training studies are obviously relevant to this question. Siegler (1976) showed that pointing out to the child the non-dominant dimension (distance from the fulcrum in the balance-beam task in this case) improved the performance of even 5-year-old children so long as they were given help with what Siegler called 'constructing and monitoring' (Siegler, 1984, p. 152) the dimension, as opposed to merely having it indicated to them.

How does it fair against the evidence? In the first place, we know that children with early-treated PKU (see above) are likely to have IQ scores in the normal range despite the fact that they perform worse than controls on executive tasks (Welsh, Pennington, Ozonoff, Rouse & McCabe, 1990; Diamond, 1996; Diamond *et al.*, 1997). Such an independence between general intellectual development and early executive functioning would appear to conflict with the theory.

To tackle this question we need to return to the distinction between executive performance and executive competence errors. If it can be shown that it is performance errors that early-treated PKU children are making then the theory has not been weakened. What kind of errors are we dealing with? Executive difficulties shown by PKU children are, as Welsh *et al.* (1990) say, 'subtle' in nature, and this would seem to indicate performance failures. Additionally, Diamond's (1996; Diamond *et al.*, 1997) data on PKU children (discussed above) show them to be lagging only a little way behind matched controls rather than showing clear impairment. For example they only lag on the criterion that they make the A-not-B error at shorter time-intervals between hiding the object at B and being allowed to search than do controls. By 12 months they have caught up, suggesting that this lag is one lying within the Piagetian sub-stage 5. Hood (1996) finds PKU children to be only mildly impaired on a more challenging invisible displacement task (the 'tubes' task) which normally developing children do not pass until age 3. It is much the same story for other executive tasks that are suitable for use with young children. For example, on a Stroop-like task they lag about 6 months behind normal pre-schoolers (Diamond, 1996). In general, if PKU children are, as it were, *trying to do the right kind of thing but not doing it well enough* then we are indeed dealing with executive performance errors. But it goes without saying that while the competence-performance distinction is easy to draw in theory it is difficult to draw in practice.

One population of children whose executive difficulties are not in the least 'subtle' is that of children with autism. Not only are they dramatically impaired on many executive tasks³² (Ozonoff, Pennington & Rogers, 1991; Hughes & Russell, 1993; see reviews in Russell, 1997a), but their everyday behaviour, on which the diagnosis is based, manifests clear executive impairment.

³² They are not impaired, however, on some kinds of task on which children with PKU are impaired (Russell, Jarrold & Hood, 1999). These are, broadly, search tasks. As I argue in the text, this is probably because autism develops after the end of the sensorimotor stage, whereas PKU is present in the early months of life.

The first thing one would expect executively impaired individuals to show is behavioural rigidity, and that is exactly what we find in autism. Around 75% of children with autism are mentally handicapped.

The executive-Piagetian theory seems to have the resources to explain not only the fact that many children with autism are mentally impaired but also why the non-executive symptoms of autism take the form they do. The latter is something which it must do, given that about a quarter of children with autism are of normal intelligence.

One of the core, non-executive features of autism is a profound difficulty with conceptualizing mental states – assessed by 'theory of mind' tasks (Baron-Cohen, Tager-Flusberg & Cohen, 1993). Moreover, the disordered communication found in autism is easily explained in terms of the fact that persons with autism find it difficult to 'read minds' and to reflect upon their own epistemic states and desires. As we have already seen, theory of mind tasks make executive demands,³³ and so it is possible that their difficulty with mentalizing is overestimated by tasks like the false belief task. But they also fail on theory of mind tasks which do not make such demands (Baron-Cohen *et al.*, 1994, Experiment 1), and their difficulties with mental-state reading are so clear that they can be identified without formal testing. This leaves us with the question of whether the executive-Piagetian view can explain these 'mentalizing' difficulties as consequences of early executive dysfunctioning.

I now outline such a theory (see Russell, 1996, Part 3; 1997b; 1998). Its starting assumption is that if children are to develop a *theory* of mind, taking this to mean a network of concepts about mental states and processes, then it is necessary that they enjoy the right kind of *pre-theoretical* experience of their own mentality.³⁴ I have tried to articulate these experiences in terms of agents' 'claiming responsibility' (to be understood in subpersonal terms) for changes in their mental contents that are caused by their own actions, and have taken action monitoring to be of central importance. But whatever aspects of executive functioning one wishes to focus on, the essential claim must be that adequate executive functioning not only guarantees that our goals are

³³ Children with autism show the same pattern of performance, relative to mentally handicapped controls, as do normally developing 3-year-olds, relative to 4-year-olds, on the tasks described in note 23 (Russell *et al.*, in press).

³⁴ A useful way of describing this first-order self awareness is by reference to what Nagel (1974) tried to articulate in his famous article 'What is it like to be a bat?' There is something which it is like to be an infant of, say, 11 months. The representations have a self-locating character without the self being an object of reflection.

generally achieved but also guarantees that *there will be a set of experiences that will be governed by our intentions*. A theory of mind, the hypothesis assumes, must be grounded in the *first-order* experience of controlling, within limits, one's mental life.

One can take this view while being agnostic about whether a theory of mind develops as a module (Leslie, 1987), or is acquired by a process of theory-testing (Gopnik & Meltzoff, 1997), or develops through the acquiring of a theory of representation in general (Perner, 1991). If the first-person experience of being a mental entity is impoverished, an explicit theory of mental life cannot develop adequately, no matter how the process proceeds in the normal case; it cannot flourish on infertile soil.

How does this view fair against the data? Something synoptic will be said with regard to action monitoring. If action monitoring is impaired in autism then it is likely to be at a fairly high level – intention monitoring as compared to the making of efference copies of physical actions. The main reason for saying this is that sensorimotor development is normal in autism, and motor skill is arguably³⁵ normal. Signs of autism often appear shortly before or somewhat after the end of Piaget's sensorimotor stage; and this makes it likely that, if action monitoring difficulties do lie at the heart of autism, these are likely to be difficulties with the monitoring of high-level intentions, not with the monitoring of basic actions. There is some rather indirect evidence that intention monitoring is impaired in autism (Russell & Jarrold, 1998; in press). More important, the kinds of difficulties with imitation that we see in autism (Smith & Bryson, 1994; Rogers, Bennetto, McEvoy & Pennington, 1996) are naturally explicable as the result of intention monitoring impairments (Russell, 1997b). For when subjects have to imitate a model, they must translate how an action appears to them into the kind of intention that might have generated it.

Without the careful spelling-out that a theory of this kind requires it may look more like a hopeful gesture in the direction of a theory than a theory in its own right. But I hope enough has been done to suggest why the syndrome of childhood autism is something that yields to an executive-Piagetian account. What is spared in high-functioning and moderately mentally handicapped children with autism is knowledge of the physical world and what is impaired is knowledge of the mental

³⁵ A study by Hermelin and O'Connor (1975) did suggest that children with autism are impaired in producing visual efference copies of their movements. However, we were unable to replicate this result when a control group of non-autistic, mentally handicapped children was included in the study (Russell & Saltmarsh, unpublished data, 1997).

world.³⁶ According to the theory, this is because autism normally begins towards the end of or shortly after the end of the sensorimotor period, after the time when the fundamentals of physical knowledge have been laid down.³⁷ One of the major developmental processes after that period is children's regulation of their experiences in relation to a secure knowledge of the physical world (overcoming cognitive 'egocentrism' in Piaget's terminology); and if this is impaired then their pre-theoretical experience of being a psychological entity will be impaired – for reasons given earlier.

Finally, this account is a truly developmental one in the following sense. Unlike the theory which says that autism is caused by delay or deviance in the development of a module for mentalizing (the 'theory of mind mechanism' of Leslie and Thaiss, 1992) this theory attempts to relate the nature of the symptoms to the kinds of mental functions developing when the symptoms appear. To make sense of this claim we need to consider the following proposals – some factual and some speculative.

- The kind of executive tasks on which children with autism are impaired are those in which arbitrary³⁸ rules have to be followed in the context of prepotency (Russell, 1997b).
- Arbitrary rules are ideally encoded in a symbolic form. Encoding 'what has to be done' in a novel situation or when the act has no apparent rationale is best done by manipulating symbols. Bluntly: the best way to regulate behaviour is by self-talk, either in natural language or in a language of thought.
- At 12 months of age, according to the data of Xu and Carey (1996), infants appreciate that objects are individuated by their permanent perceptual properties (e.g. being a ball) as well as by their temporary spatio-temporal properties (e.g. being behind this screen). This corresponds to the ability to draw the linguistic distinction between predicates and arguments. The first words are commonly produced at 12 months. Moreover, the infants in the Xu and Carey study who made the distinction just outlined were more linguistically advanced (more likely to comprehend the relevant words) than those who did not. Around 18 months of age we often see a naming

³⁶ See Baron-Cohen (1997) on impairments in folk psychology versus folk physics in autism.

³⁷ The evidence for children with autism being impaired in conservation tasks is contradictory.

³⁸ I am taking an arbitrary rule to be one (a) the following of which has no immediate effect on the physical environment (in contrast to turning a key in a lock) and (b) which, being an *ad hoc* convention, has no rationale (e.g. *why* sort by colour rather than shape in WCST?).

explosion and naming insight. Therefore, it is certainly possible that 12–18 months is the period during which symbolic functioning develops.

- Twelve to 18 months is the period during which autism is most often diagnosed (Bailey, Phillips & Rutter, 1996).
- *The development of symbolic functioning enables the infant to regulate his or her behaviour in terms of symbolically encoded goals.* For example, they begin to search for particular objects, defined by permanent properties, and they become able to set their own goals by manipulating symbols. Pretend play is the clearest example of this.

The present claim, therefore, is that what fails to develop in autism is the ability to regulate behaviour by self-generated symbols. It could be speculated further that the forms of autism in which there is no language and in which self-regulation problems are very severe develop before 12 months and that Asperger Syndrome (a criterion for which is normal language development) develops when some explicit conception of propositional attitudes is developing at about 2½ years (Bartsch & Wellman, 1995). At this point the division between the executive-Piagetian theory and the historical-Piagetian theory is at its widest. It is to the latter that I finally turn.

3 More than executive development: the inadequacies of the historical-Piagetian view and of bootstrapping theories generally

Spiral bootstrapping

According to the historical-Piagetian (somebody who adopts the position of Jean Piaget himself) mental development takes the form of a spiral, a process of reworking forms of knowledge, of re-acquiring them on progressively higher planes of representation. Cognitive development circles back on itself after the fundamentals of knowledge have been acquired on the plane of action. At the base of this spiral sit the classical Kantian categories: object, space, time, causality. Included in this is a ‘logic’ of action as seen, for example, in infants’ ability to search transitively (A behind B, B behind C, so A is behind C). After the end of this sensorimotor period the child gains an insight into the way in which something, perhaps an action or a spoken word, can stand for an object or event – ‘symbolic functioning’. This insight is grounded in what might be called a contemplative relation to the external world, in so far as the world is now something that the child can think about (*représentation mentale*). For the spiral to make

the next turn the child has to internalize the action-based structures (described in terms of group theory) in such a way that they structure contemplative thought about the physical world rather than actions on it. This turn of the spiral takes about 5 years. After this, children are able to represent reality not only on the level of action but also on the level of concepts. They will have in their mental armoury a set of concepts – about number, logic, substance and so forth, which frame their thought rather than merely regulating their actions. This level can be attained without linguistic ability.³⁹ Again, group theory describes the holistic nature of these concepts. Finally, the child must come to re-represent these abstract concepts on a still higher plane: the propositional plane. In this case, a similar structuring process is applied in which propositions rather than physical concepts are the contents. This gives rise in adolescence (though not universally) to the ability to frame hypotheses about propositions whose truth value is unknown, and thus makes possible systematic hypothesis-testing.

Here is an instructive example of how Piaget dealt with the relation of lower to higher levels. Piaget (1971, pp. 221–223) noted that there is an ‘isomorphism’ between the structure of neuronal connections – neural networks according to the work of McCulloch and Pitts – and the 16 binary propositions of formal operational thought (see note 12). How are these levels of subpersonal functioning and of hypothetico-deductive thought related? ‘Thus,’ wrote Piaget, ‘it is not by direct reference to the logic of neurones that the logic of propositional operations – admittedly isomorphic – will be built up, but rather *by an uninterrupted series of constructions* that may have been oriented by structures inherent in nervous functioning but which nevertheless presupposes a series of new instruments’ (p. 223, my italics).

The problem which many contemporary cognitive developmentalists have with all this is the problem that they will tend to have with any purely ‘bootstrapping’ account of development, namely, how is the trick done? How is the transformation supposed to be achieved without utilizing some innate representational capacity? How is it that sensorimotor knowledge is transformed into conceptual knowledge and this into hypothetico-deductive reasoning *merely by doing more of the same kind of thing*? It can hardly be said that Piaget ignored this issue. Indeed he wrote what must be hundreds of pages on the mechanism of ‘reflective abstraction’ but at

³⁹ Piagetians (e.g. Furth, 1966) claim to have shown that deaf children succeed on conservation tasks by recruiting purely non-verbal knowledge. But there are reasons to doubt these conclusions (Russell, 1978, pp. 179–180; Marschark, 1993, ch. 7).

no point does he break free of his own hermeneutic circle or from his own prejudices against nativism.

Piaget was hardly alone in using bootstrapping mechanisms to explain development. We also see them at work in the field of grammar development. Thus Schlesinger (1988) argues that the syntactic categories such as noun and verb are developed out of the semantic categories of object and action by a process of 'semantic assimilation' (see Braine, 1992, for a sophisticated re-statement of the theory). But how? Paraphrasing Pinker, the mechanisms suggested by syntactic constructivists resemble the advice given to somebody on how to play the stock exchange: 'Buy low and sell high'. What are proposed are not mechanisms but *desiderata*. As Pinker (1987) argues, bootstrapping from semantic to syntactic categories does take place but it only succeeds because the child has certain prior expectations about the structure of human languages. The same is probably true of the development of concepts – as argued by Fodor (1976) – at least to some degree.

Piaget's account of the development of 'symbolic functioning' at 18 months illustrates the limits of his bootstrapping arguments. One central fact about symbols is that they bear an arbitrary relationship to their referents. But actions are not arbitrary: they are what people do for reasons, and the form of an action is exhausted by the reason for doing it. Pretend play would appear to be an exception, but there is nothing arbitrary about play actions either. These are not truly symbolic, because the gestures children use with stand-in objects, or in the absence of objects, resemble the gestures one naturally makes with the real object (e.g. 'telephoning' with a banana).⁴⁰ Piaget made similar remarks about the way mental images are supposed to stand for their referents. But again the symbolizing medium is entirely action-based: mental imagery was supposed to be a form of 'interiorized imitation'. The word, or 'sign' for Piaget, was taken to be the perfect amalgam of the pretend gesture and the image whose successful use was supposed to exemplify a state of 'equilibrium' between assimilation and accommodation. Pretend play was one form of disequilibrium (primacy of assimilation) and imitation was another (primacy of accommodation). But a theory which grounds symbolizing in action is doomed to fail for the simple reason that *no matter how*

⁴⁰There would appear to be a contradiction here with what I said earlier about pretend play manifesting symbolic abilities. The claim there was that symbolic play is the culmination of the ability to separate objects (arguments) and their properties (predicates), and thus the child can apply, say, telephone-like properties to objects other than telephones. What I am denying here is that a pretend gesture stands for its object in anything like the way a word stands for its referent.

hard you squeeze an action you will never get a symbol out of it.

A theory with these resources could not explain the developmental facts. On one estimate (Carey, 1978) children must be learning an average of nine words a day between the age of 18 months and 6 years, not to mention the fact that by about 4½ they have acquired the basic syntactic rules of the target language in all their complexity. Not only does the historical-Piagetian theory fail to account for this, but it is able to ignore both the fact that language is an ideal vehicle for representing means and ends and that it is likely to play a major role in the regulation of behaviour (Luria, 1973; Stuss, Delago & Guzman, 1987; and see end of Section 2). Thus, while actions themselves are not symbolic the intentions which determine them are – or are generally – and so symbolic capacities are better regarded as a determinant of later executive development: the causal link cannot be the other way round.

In sum, it is impossible to see how, on the historical-Piagetian theory, the developing thinker could attain this form of second-order representation 'for free': how, that is, the child becomes a second-order representer simply by doing a lot of first-order representation. It seems necessary to suppose then that the human child has some form of extra-executive recursive mechanism for achieving this, whether this is a general-purpose capacity for 'metarepresentation' (Leslie, 1987) or whether it is something intimately linked to the acquisition of natural language itself, as suggested here.

The inadequacies of purely sub-symbolic approaches

In the main part of this final section I shall be outlining the shortcomings of two non-Piagetian bootstrapping theories. These are theories that try to explain how propositional thought emerges out of sensorimotor representations without having recourse to innate symbolic abilities. Before doing so, however, it is necessary to make some reference to connectionist theories of cognitive development, because what these theories deny entails an approach still more empiricist than that of these bootstrapping theories: they deny that propositional capacities require propositional cognitive architectures.

Needless to say, if connectionist models of linguistic and cognitive development, as presented most notably by Elman *et al.* (1996), succeed on their own terms then my complaints about the overly empiricist nature of Piagetian theory would be beside the point. What I will do is briefly review the reasons for thinking that connectionist models do not achieve what Elman *et al.* claim they do, and also discuss how these authors back

away from claiming that connectionist models can capture something that would appear to be distinctive to human cognition – its systematicity.

In the first place I shall do little more than refer to the sceptical points about Elman *et al.*'s claims made in a paper by Marcus (1998). Here are some of his criticisms.

- The models reviewed by Elman *et al.* contain innate representations (on the input and output units), and so what these models actually develop are not representations but 'merely correspondences between sets of representations' (Marcus, 1998, p. 161).
- Rather than demonstrating an alternative to modularity, the models in the book are modules in so far as Elman *et al.* propose 'a separate informationally-encapsulated model for essentially each task that the book tries to explain' (*ibid.*, p. 163).
- The reason that Elman in particular was able to 'train' networks to represent grammatical categories such as noun was that the training corpus was designed 'such that each noun appeared in roughly the same set of contexts' (*ibid.*, p. 164). The same point has been made by Fodor (1997a) who says that what the networks are doing is modelling the *correlates* of grammatical classes, not the classes themselves. Thus, being preceded by the words 'a' and 'the' and preceding words that take '-s' and '-ed' suffixes correlates with being a noun. Networks' being able to pick up these regularities and represent them on distinctive areas of state space is a far cry from representing the class 'noun', simply because these are not features that are definitional of that category, but rather statistical signals of it in English.
- The ability of networks to generalize between data sets is severely limited by the fact that single-network models trained by the back-propagation algorithm and their cognates (this covers all the networks in the book on which the authors base their claims) are localist. This means that changes in the connection between one unit and another are made on the basis of local information, never on the basis of global information about performance. Marcus illustrates this limitation with his own models. Localism is of course a fundamental feature of connectionist modelling. Indeed it can be said to capture the essence of bottom-up modelling. This is fine just as long as one never tries to model the top-down capacities that are profoundly characteristic of human cognition.

In the second place, what is almost as notable as the above facts about Elman *et al.*'s approach is that the authors half-accept one of the fundamental criticisms of connectionist accounts of cognition. This is the complaint voiced by Fodor and Pylyshyn (1988) that networks

cannot capture the systematic⁴¹ nature of thought, two features of which are (the related facts of) recursion and the possession of syntactically composable representations. Elman *et al.* take Fodor and Pylyshyn's criticisms to be 'well-grounded' (1996, p. 103) and then go on to say that networks possess something which van Gelder (1990) had dubbed 'functional compositionality', adding that this gives networks the ability to 'implement rules' without possessing syntactic structures within their architectures. But what van Gelder tells us is no more than the fact that there are connectionist models (e.g. Smolensky's tensor product model) that can manifest the function of composing elements without possessing classical architectures designed to achieve concatenation by manipulating symbols. But the big question remains: do these 'nonconcatentive compositional schemes' *really* provide 'the raw materials for an explanation of the systematicity of cognition' (van Gelder, 1990, p. 383). To assume that they do is to write a very large promissory note indeed,⁴² not least because there is far more to being systematic than having the capacity to combine A and B (see footnote 41).

Two non-Piagetian bootstrapping theories: image schemas and socio-linguistic processes

It is not uncommon for developmentalists to hold the following set of three views: (1) what is innately represented is no more than what connectionism says is innately represented; (2) connectionism is wrong that a thoroughly sub-symbolic theory of cognition is possible; (3) during the course of development children acquire symbolic capacities. This contradicts the present view that innate symbolic capacities must be posited.

Karmiloff-Smith and Clark's (Karmiloff-Smith, 1992; Clark and Karmiloff-Smith, 1993) 'representational

⁴¹ The precise definition of this term is itself controversial. This is one way of putting it. If one can *think* that John likes cheese and Mary likes chutney then one must also be able to think that Mary likes cheese and John likes chutney. Arguments and predicates are freely combinable in thought as they are in language, and having one cognitive capacity entails the ability to exercise an infinity of others (think of Chomsky's remarks on the productivity of syntactic rules). Being systematic and being productive are conceptually related. *Prima facie* at least, it seems that such a capacity depends upon the computations being done over symbols – over representational states with a *sui generis* character. Connectionist networks, however, do not work by calling up, combining and embedding representational states with content in this sense – as is done in classical artificial intelligence. The question is whether this failure is a principal one.

⁴² I will not discuss the question of whether *classical* architectures of the kind discussed by Fodor and Pylyshyn (1988) can themselves model systematicity, or whether, as Matthews (1997) has argued, it is a challenge to both connectionist and classical models.

redescription' theory is a good example of this approach. On this view, if the starting state of the mind is sub-symbolic (understood as a neural network by Clark and Karmiloff-Smith) then, in the absence of innate symbolic functions, development has the task of making the implicit contents of these networks the explicit objects of reflection, extracting them from their various local contexts and giving them the domain-general combinatorial power of symbols – turning holistic states of neural networks into the atoms of thought. What kind of mechanisms might account for this process? Karmiloff-Smith (1992) tends to favour Mandler's 'image schema' theory (1992, 1996) while Clark (1998), like Dennett (1993), opts for socio-linguistic processes.

According to Karmiloff-Smith (1992) Mandler's theory consists of 'the most thoroughly worked out speculations about the way in which young infants *build* representations that are suitable for linguistic encoding' (1992, p. 41, my italics). The core building process is taken to be the 'perceptual analysis' of events, which enables the preverbal child to abstract sets of dynamic, analogue schemas of how objects can move, causally interact, and be spatially related to one another: 'image schemas'. Some of these are PATH, UP-DOWN, CONTAINMENT, LINK and FORCE. Moreover, by combining elements of the PATH and LINK schemas the infant is able, on Mandler's view, to draw the distinction between animate and inanimate kinds of movement. Image schemas are 'condensed redescriptions' of perceptual experiences, which, being analogue, capture continuous rather than discrete states of objects. 'This is not', writes Mandler, 'a nativist position; on the contrary, it is a constructivist position. The mechanism of perceptual analysis I have described makes it unnecessary to posit innate ideas or concepts; *perceptual analysis alone can build up meanings*' (1996, p. 372, my italics).

While it is certainly plausible that such processes might contribute in some way to children's learning the meaning of, say, prepositions and to their being able to draw an adequate distinction between animate and inanimate objects, it is clear that they can ground neither symbolic thought nor semantic development. This is primarily because, despite Mandler's determination to erect an alternative to Piagetian theory, her theory inherits Piaget's great weakness. (The theory is, in fact, rather like a passive, third-person version of Piaget, in which concepts are bootstrapped from perception rather than from action.) The theory fails because symbols are exactly what image schemas (and Piagetian schemes) are not: arbitrary, digital, and capable of being combined into propositions. More generally, imagist theories of meaning have a long and inglorious history; and the

reader is directed to Müller and Overton (in press) for a thorough account of how Mandler's theory fails to avoid their classical problems.

Moreover, as Bowerman has pointed out (1996), image schema theory sits uncomfortably beside the fact that different human languages can make radically different semantic cuts, having quite divergent prepositional systems and words for joining objects together (to give just two examples). This means that the preverbal image schemas must represent a very low common denominator indeed given that all human languages are learnable. Mandler's reply to this objection (Bowerman, 1996, pp. 421–422) is that her theory is designed to account only for the very earliest period of acquiring spatial morphemes, with the language-relative patterns of development being later projections from this. But if so, as Bowerman notes, 'the spatial image-schemas are doing little of the work that has often motivated the postulation that children map words onto prelinguistically established concepts – namely to provide a principled basis on which children can extend their morphemes beyond the situations in which they have frequently heard them' (1996, p. 422). In short – this is one of the classical problems of imagist theories of meaning – image schemas lack productivity: they are designed to *limit* the meanings available to the language learner rather than to explain the vista of possible meanings which actual learners experience and acquire.

I now turn to the claim that the representational capacities of the initially network-like and implicit mind of the young child are augmented by his or her experiences with natural language. This position – it's a recapitulation of Vygotskian theory – has recently been articulated by both Clark (1998) and Dennett (1993). I will discuss Dennett's version. He gives the following illustration (Dennett, 1993). A child hears a sentence which he does not understand attached to a situation he does understand: 'Hot. Don't touch the cooker' says mother. The child keeps these word-strings refreshed on his articulatory loop, argues Dennett; he rehearses them as a kind of self-commentary.

As the child lays down more associations between auditory and articulatory processes, on the one hand, and other patterns of concurrent activity on the other, this would create 'nodes' of saliency in memory; a word becomes *familiar* without being understood. And it is these anchors of familiarity that could give a label an independent identity within the system. Without such independence, labels are 'invisible'. (1993, p. 544, original italics)

Such a process is taken to 'wrest concepts from their interwoven connectionist nets' (p. 547).

Dennett does caution that he is expressing his proposal in 'simple and extreme terms for the sake of clarity'. But could there ever be an articulation of it that is sophisticated and judicious – and plausible? The idea is that the child has concurrently: (1) a semantic representation (perhaps supported by a mental image) of the painful consequences of touching an object and (2) a string of sounds in his head. Where is the bridge? Self-talk is no use without *self-understanding*. The child's semantic representation of the event is in no sense the meaning of the sentence because there is no reason to believe that the elements of one (e.g. the concepts expressed by the English words 'hot' and 'touch', plus negation and the imperative mood) line up in just the right way with the elements of the semantic representation. And these must not only 'line up' but must also have a representational commonality sufficient to support productive thought. In other words: how does each sound find its proper place in the child's 'interwoven connectionist nets'? Without an innate symbolic capacity – without an independent capacity for mapping concepts to arbitrary sounds or gestures – it is impossible to see how a bridge could ever be built between the semantic representation and the sounds in the head. In short, Dennett's proposal runs up against Fodor's (1976) argument that words cannot function as symbols for the language learner unless the learner has a representational format with a proto-symbolic character.

As was the case for the image schema theory, this kind of proposal also accords poorly with some facts about language acquisition. Any theory that places the emphasis upon speech input to the child plus internalization is challenged by the cognitive and linguistic successes of those children who lack any such input.

Most deaf children are born to hearing parents many of whom learn a little Manual English in order to communicate with their children. This is a signing system which follows English word order and which inflects like English. The children, however, do not tend to follow this system: they instead construct for themselves a gestural system with some of the distinctive grammatical characteristics of American Sign Language (a system entirely dissimilar from English) and of other kinds of sign language (Goldin-Meadow & Mylander, 1990; Goldin-Meadow, Butcher, Mylander & Dodge, 1994). For instance, such a child may use a form of systematic noun/verb marking that was not present in the input and, despite having never been exposed to American Sign Language, may pick out different individuals when they sign in terms of their different locations in signing space – as does American Sign Language. At the age of about 6, the language of these

children certainly lacks the recursive richness of the language of normally developing children, but nobody would deny that they are using language to communicate *thoughts*. Moreover, the general cognitive development of congenitally deaf children is often normal (Marschark, 1993, ch. 7).

It seems likely then that an innate symbolic capacity is something we are going to have to posit; and it is clear that Piaget was not alone in being reluctant to do so. Of course positing such a system brings in train a whole new set of problems. In philosophy these cluster around the question of how an innate language of thought might acquire its semantics (Maloney, 1989). In psychology this presents as the so-called symbol-grounding problem (Harnad, 1990). For example, having innate symbols for 'momentum' or 'intention' is no good to you unless you also have a way of finding out what in reality corresponds to them. If a symbol is characterized entirely by its formal properties, how can it be individuated in terms of real-world facts? (also see Russell, 1996, 1.1 (ii)). But in any event, my proposal is not identical to Fodor's language of thought doctrine, in so far as it is concerned with innate symbolic functions (predication, for example) rather than with innate symbols. My claim is that we cannot make sense of mental development unless we suppose that a symbolic function develops early on (between about 12 and 18 months of age). But this is taken to *mature* as part of the innate human endowment rather than being *constructed* in the way Piaget suggested. This idea is vaguer than the language of thought doctrine (which is perhaps vague enough); but my purpose in this paper is to identify the need for a theory of it, not to describe the theory itself.

In any event, turning away from the kind of empiricism under discussion does not require one to turn towards a Fodorian view. The varieties of nativism are only limited by our imagination. On Fodor's view all new knowledge is no more than the transposition of old knowledge; and this is of course about as nativist as one can get. At the other extreme one has the rather tentative nativism of Aristotle in which it is accepted that knowledge acquisition requires the possession of *some* prior knowledge, and in which the notion of innate potential (*dunamis*) does not commit one to innate representations (*à la* Plato) but it does commit one to something more than general capacities for inductive learning (*epagoge*). One can say that capacity X cannot be acquired from experience without committing oneself to what it means *exactly* for capacity X to be innate. What it means may be something which cannot be sensibly discussed without more evidence; there is no onus on the developmentalist to give an *a priori* account of it.

Self-consciousness

I turn finally to second-order thought: thinking about thinking as opposed to second-order representation more generally. As I suggested earlier, the executive-Piagetian view is essential if we wish to explain the development of self-awareness: the theory helps us to understand how a pre-theoretical form of self-awareness is possible. The development of other-awareness would seem to depend upon this, if one thinks that the intentions of others cannot be conceptualized by a subject who is not a successful intender himself (Russell, 1996, Part 3). But thinking about thinking requires understanding, at least tacitly, how propositional attitudes relate to mental contents. Propositional attitudes are better regarded as mental orientations (or perhaps locations in the mental architecture in Fodor's metaphor), but a mental content must surely be something symbolic, either in natural language or in a language of thought. For the historical-Piaget, however, not even concrete operational thought is symbolic (Piaget, 1954).

How could a subject who was unable to represent contents in a propositional form conceptualize any mental content, given that a content represents something as being the case? This would appear to be true despite the logical parallels existing between iconic representations and beliefs (Jackendoff, 1975; Perner, 1991), because icons are infinitely ambiguous (Wittgenstein, 1953, Section 139). Thus, children of 4 years who pass a range of theory of mind tasks are arguably manifesting some conception of what it means for their own minds or other minds to represent reality as being such-and-such. This insight concerns the truth-value of propositions. One does not necessarily have to agree with Perner (1991) that older children's mentalizing ability is wholly explicable in terms of their acquiring a theory of the content-referent relationship,⁴³ to think he is right to insist that no conception of mind could be achieved without it, and that it is an insight that can be applied in non-mental as well as mental domains.⁴⁴ How

⁴³ Perner takes the view that children over 4 years of age are 'metarepresentational theorists', meaning that they have come to know what it means for something to represent something – a picture and what it pictures, a sentence and a situation, a photograph and what is photographed, a thought and the situation thought about, and so on.

⁴⁴ Perner (1998) has more recently described this as 'predication explicit' thought. He refers to the ability to conceptualize an object as capable of attracting a number of descriptions. The fact that 3-year-olds have difficulty with 'aspectuality' tasks (O'Neil & Astington, 1989) and are unable to shift dimensions in a sorting task (an extra-dimensional shift) as reported by Zelazo, Frye and Rapus (1996) is taken as evidence for this claim.

could such a conception emerge from executive ability alone? As I argued earlier, the executive-Piagetian theory argues that executive development is necessary for this knowledge to come to fruition, but we also need to posit the development of a propositional system in which mental contents are represented. Perhaps a propositional version of the naming insight which begins at about 18 months⁴⁵ is something which we see emerging at around 4 years of age.

It may seem that the view being articulated here is that development of the executive systems is something that depends upon the maturation of the 'theory of mind module', as suggested by Carruthers (1996). It is not. Rather, it is the view that the acquisition of a theory of mind, while it may be grounded executively (as described above), requires a second-order mode of thought of a kind that Piaget's all-purpose mechanism of reflective abstraction does not seem capable of explaining. This second-order mode of thought would seem to require a theory-like grasp of the representing relation which could plausibly be linked to the developing language capacity – a later-developing, sentential version of the toddler's naming insight.

A supporter of the historical-Piaget might say that this is far too pessimistic a view of the inadequacies of a purely executive account of second-order thought because it depends upon a very narrow definition of executive functioning. For example Norman and Shallice's (1986) influential model of the executive system which distinguishes between the 'contention-scheduling' of encapsulated action packages ('schemas') on the one hand and a Supervisory Attentional System (SAS) on the other is surely an existence-proof of what I said could not be achieved: a purely executive account of second-order thought. In answer: how could the SAS do its work (of overseeing goal-directed activity and of producing novel 'off the cuff' actions, for example) unless it had a complex network of *symbolic* representations. It is instructive that the only attempt so far to produce a computational model of the SAS function was a *hybrid* of the sub-symbolic and the symbolic (Cooper, Shallice & Farrington, 1995). This is probably because when one is dealing with representations of representations, as in the SAS, symbols are what is required (see Fodor, 1997b, for reasons why). The historical-Piaget had nothing persuasive to say about how this might be possible.

The basic shortcoming of the historical-Piagetian theory is, in short, that it was insufficiently nativist. There are

⁴⁵ One might say that the insight at 18 months is about the status of arguments and that which arrives at about 4 years is about the status of argument-predicate relationships (see previous note).

many things that it was insufficiently nativist about in addition to the capacity for reflexive self-consciousness just discussed. But as this capacity is a good candidate for being 'what makes us human' this is indeed a considerable shortcoming.

In the past, those who have come to a similar conclusion have been too quick to dismiss the whole Piagetian enterprise on that basis, and to replace it with the essentially a-developmental view that the child comes into the world with an armoury of modules whose development depends on no more than environmental triggering and maturation. Such theories typically avoid the tricky question of how entities with *sui generis* representations are supposed to interact (Jackendoff, 1996, for an exception). There is nothing wrong with the idea of innate mental modules – the syntactic capacity would seem to be a good candidate for being one – but if what I have been saying in the main body of this paper is even half true a view of mental development as module maturation, and that alone, has to be wrong. The fundamental features of mentality – the conception of an external world and the first-order experience of being a mental entity set apart from that world – are grounded in agency. As Jean Piaget said.

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