

CONTENT AND PSYCHOLOGICAL EXPLANATION

by

SARAH CHARLOTTE PATTERSON

B.A., Oxford University
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Submitted to the Department of
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in Partial Fulfillment of the Requirements
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Signature of Author _____
Department of Linguistics and Philosophy
June 17, 1988

Certified by _____
Ned Block, Professor of Philosophy
Thesis Supervisor

Certified by _____
Joshua Cohen, Professor of Philosophy
Thesis Supervisor

Accepted by _____
Professor George Boolos
Chair, Departmental Graduate Committee

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ABSTRACT

In giving psychological explanations in science and in everyday contexts, we explain how mental states cause actions in terms of the contents of the states. This thesis consists of three papers, each of which explores an issue raised by this fact.

The first paper, 'The Anomalism of Psychology,' addresses Donald Davidson's claim that cognitive psychology is anomalous among the sciences, since its use of content as an explanatory notion makes it incapable of producing strict laws. His argument against psychophysical laws is based on the claim that the rational cannot be lawfully correlated with the rational. So construed, the argument does not appear to succeed. His argument that there cannot be strict laws within psychology relies on the claim that we cannot have access to all the causal influences acting in the psychological domain. Examination of these arguments suggests that the features of psychology which, for Davidson, are signs that it is incapable of producing serious laws, are actually consequences of the fact that it explains by functional analysis rather than by subsumption under strict causal laws. Since these are features shared by other special sciences employing this explanatory strategy, psychology is not anomalous among the sciences.

The second and third papers are contributions to the debate about whether mental content is individuated individualistically, so that it depends only on features of the individual thinker. Tyler Burge has argued that physically and functionally identical thinkers may have different thoughts if they occupy different linguistic environments. The second paper, 'Constraints on Content,' disputes this claim of Burge's as he applies it to everyday discourse about mental states. Cases are presented which show that in giving common sense explanations of actions, thoughts are individualistically individuated. The conclusion is that thoughts are sometimes individuated with respect to linguistic environment, sometimes individualistically, depending on the purposes of the report.

The third paper, 'Individualism and Semantic Development,' takes issue with Burge's claim that content individuation in scientific psychology is uniformly non-individualistic. The paper outlines current models of semantic development and presents a thought-experiment showing that psychologists in this field do not individuate psychological states with respect to linguistic environment. Possible Burgean objections to the individualistic interpretation of the thought-experiment are considered, and comparison is made with Burge's non-individualistic analysis of Marr's theory of vision.

Thesis Supervisor: Dr. Ned Block

Title: Professor of Philosophy

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The Anomalism of Psychology.

1 Introduction.

Davidson holds that 'there are no strict deterministic laws on the basis of which mental events can be predicted and explained' (ME:208). This claim, the Principle of the Anomalism of the Mental (PAM), is one of the premises he uses to argue for his version of the identity theory, anomalous monism, and as such it has been the topic of much recent discussion. But PAM is also the basis of Davidson's views on the scientific standing of cognitive psychology, and in this capacity it has received relatively little attention. Davidson appears to regard the potential of a science for producing strict laws as diagnostic of its scientific standing; he writes that

'by evaluating the arguments against the possibility of deterministic laws of behaviour, we can test the claims of psychology to be a science like others (some others)' (PP:230).

Adhering as he does to PAM, which entails that there is no possibility of deterministic laws of behaviour, his verdict is that

'the study of human action, motives, desires, beliefs, memory, and learning, at least so far as these are logically tied to the so-called 'propositional attitudes,' cannot employ the same methods as, and cannot be reduced to, the more precise physical sciences' (PP:240).

This quotation could easily be read as disparaging psychology,

as relegating it to the status of a second-class science incapable of precision; but Davidson subsequently disavowed this interpretation. In his replies to comments on the paper quoted above, he maintains that though it may sound as though he is 'making some sort of attack on psychology generally, or at least on its right to be called a science,' that was certainly not his intention (PP:240). His point, he says, is that 'psychology is set off from other sciences in an important and interesting way' (PP:241), not that it is inferior to them.

Davidson's final verdict, then, seems to be 'separate but equal.' However, I believe there are reasons to doubt his claim that he is not stigmatizing psychology. His view of scientific explanation makes it difficult to see how any enterprise incapable of producing strict laws could pass muster as science. For Davidson, the paradigm of scientific explanation is explanation in the physical sciences, which he conceives of as deductive-nomological in form. That is, an 'ideal explanation' consists of 'a description of antecedents and a specification of laws such that the explanandum can be deduced' (HEA:263). If the laws specified in such an explanation are to permit the deduction of the explanandum, they must be strict and exceptionless; and the stricter the laws are, the closer the explanation in which they figure to the ideal. Given this identification of scientific explanation with subsumption under strict or 'serious' laws (to use Davidson's term), it follows that where there can be no serious laws, there can be no serious science.¹ If it is true that as

long as cognitive psychology invokes mental states with intentional properties, it cannot produce strict laws, and if strict laws are the sign of respectable science, cognitive psychology must be deficient as long as it deals with such states.

The first step towards assessing this conclusion is to attempt to articulate the arguments for PAM. In doing so, I hope to show that Davidson's objections are informed by a particular view of science: genuine science aims to discover precise, exceptionless causal laws which are used to predict and explain particular events by deductive subsumption. If this is so, there are two questions we must ask when assessing the impact of Davidson's comments on psychology. First, are the features of psychology which cause it to fall short of the ideal unique to the field, or will other special sciences fail for the same reasons? Second, is Davidson's model of ideal scientific explanation appropriate to practice in cognitive psychology? If we can answer the first of these questions, we will see whether Davidson has made good his claim that psychology is anomalous among the sciences; if we can answer the second, we will see whether the anomalism of mental events has any consequences for the status of psychology. I shall argue that the features of psychology which lead Davidson to regard it as anomalous are consequences of the fact that it explains by functional analysis rather than by subsumption under strict causal laws, which may differentiate it from physics but does not set it apart from other special sciences

which employ the same explanatory strategy.

2 Psychophysical Laws.

PAM states that 'there are no strict deterministic laws on the basis of which mental events can be predicted and explained' (ME:208). Let us try to get a better grasp of what this claim involves. Since for Davidson 'events are mental only as described' (ME:215), PAM will be established if it can be shown that there can be no strict laws employing mental (or psychological) descriptions. A description of the form 'the event that is *M*' is a mental description, and hence picks out a mental event, if and only if the expression replacing *M* contains a verb of propositional attitude used so as to create a nonextensional context. The occurrence of a mental event, then, is the acquisition (or loss) of a propositional attitude. For present purposes, the noteworthy feature of this criterion is that it makes processes such as believing, intending, desiring, remembering, and perceiving paradigmatic of the mental, and that these can plausibly be regarded as the processes studied by cognitive psychologists. PAM, if established, would thus rule out the possibility of strict laws in the domain of cognitive psychology.

To show that there can be no strict laws containing mental predicates, Davidson must argue against both psychophysical laws (laws containing mental and physical predicates) and purely psychological laws (laws containing mental predicates alone).² Before turning to his arguments

against strict psychophysical laws, it will be useful to say a little about Davidson's notion of strictness and about the types of psychophysical laws against which his argument is directed. This may be accomplished by considering a distinction that is central to Davidson's arguments against strict psychophysical laws, the distinction between homonomic and heteronomic generalizations.

To describe a statement as homo- or heteronomic is to comment on the degree to which it is lawlike, and to do this is to say something about the relationship between the statement and its instances. 'All emeralds are green' is a lawlike statement--that is, its instances confirm it (ME:218). 'All emeralds are grue,' by contrast, receives no inductive support from its instances; it is not lawlike. Homonomic and heteronomic statements fall somewhere in between. Both receive enough support from their instances to act as 'rude rules of thumb' which are reliable enough for us to use 'in our daily traffic with events and actions' (ME:219). But the instances of a homonomic generalization G also give us reason to believe that a strict law could be attained by adding refinements stated in the same general vocabulary as G; homonomic generalizations point 'to the form and vocabulary of the finished law' (ME:219). The instances of heteronomic generalizations, by contrast, give us reason to believe that there is a strict law at work, but one that can be stated only in a vocabulary different from that of the original generalization (ME:219).

Homonomic generalizations, then, can be refined to give strict laws, but heteronomic ones cannot. But what is it for a law to be strict? Strict laws, in contrast to 'rude rules of thumb,' are not only lawlike but 'precise, explicit, and as exceptionless as possible,' and they are not exceptionless through containing ceteris paribus clauses (ME:219). Such psychophysical generalizations as we have are not strict, as the following passage makes clear:

'if an event of a certain mental sort has usually been accompanied by an event of a certain physical sort, this often is good reason to expect other cases to follow suit roughly in proportion. The generalizations that embody such practical wisdom are assumed to be only roughly true, or they are explicitly stated in probabilistic terms, or they are insulated from counterexample by generous escape clauses.' (ME:219).

As one might expect, Davidson holds not merely that psychophysical generalizations are not strict, but that they cannot be made strict; they are not homonomic, but heteronomic. The details of this argument will occupy us shortly; the point I want to stress here is that the discussion of homo- and heteronomicity shows that Davidson's use of 'psychophysical law' is not as unproblematic as some have assumed it to be.

Often when Davidson's anomalism is discussed it is taken to be obvious that in speaking of psychophysical laws he is referring to the putative bridge laws which would be needed to effect a classical reduction of psychology to, say, physiology—the bridge laws which would identify types of mental events with types of brain events.³ Sometimes Davidson must indeed be

referring to these; for example, he assumes that showing that there cannot be strict psychophysical laws is the same as demonstrating 'the irreducibility of the mental' (ME:215), using the two formulations of the thesis interchangeably (e.g. BBM:154). Further, he says that in denying such laws he is claiming that 'no purely physical predicate, no matter how complex, has, as a matter of law, the same extension as a mental predicate' (ME:215); he is denying the 'reducibility [of mental properties] through law or definition' (ME:214). Such remarks are surely motivated by the classic view of theory reduction via bridge laws.

However, it is hardly plausible that our practical heteronomic wisdom includes rough generalizations associating brain events of a certain sort with mental events of a certain sort--that is, rough bridge laws. The rough psychophysical generalizations Davidson has in mind are causal generalizations, important for 'the support they lend singular causal claims and related explanations of particular events' (ME:219). And if (per impossibile, for Davidson) they could be refined and made strict, they would yield exceptionless causal laws, not the type-identities expressed by bridge laws.

It is to be expected that Davidson would wish to rule out both causal and bridge psychophysical laws; to establish PAM, he must show that there are no laws which can be used to explain or predict psychological events, which at least involves ruling out laws of these two types. But if we bear in mind that the argument against psychophysical laws must apply

to both causal and bridge laws, we may become suspicious of the move from the heteronomicity of rough causal correlations to the nomological irreducibility of the mental. Is the impossibility of psychophysical bridge laws supposed to follow from the failure of causal laws? Or is the argument intended to rule out both types of psychophysical law simultaneously?

When we turn to the examination of Davidson's arguments against psychophysical laws, there seems to be little doubt that their impossibility is linked to the importance of radical translation or interpretation in the ascription of propositional attitudes (i.e., the description of events as mental). But Davidson's reason for ruling out psychophysical laws is not, as has been claimed (e.g. Smart 1985:178), that claims about the mental are indeterminate while claims about the physical are determinate. The reason Davidson gives for the impossibility of psychophysical laws is that the rational cannot be lawfully correlated with the nonrational. The following crucial passage indicates how it is that the heteronomicity of psychophysical generalizations 'traces back' to the indeterminacy of translation:

'This irreducibility [of psychological concepts] is not due, however, to the indeterminacy of meaning or translation, for if I am right, indeterminacy is important only for calling attention to how the interpretation of speech must go hand in hand with the interpretation of action generally, and so with the attribution of desires and beliefs. It is rather the methods we must invoke in constructing theories of belief and meaning that ensures the irreducibility of the concepts essential to those theories. Each interpretation and attribution of attitude is a move within a holistic

theory, a theory necessarily governed by concern for consistency and general coherence with the truth, and this it is that sets these theories forever apart from those that describe mindless objects, or describe objects as mindless.' (BBM:154; emphasis added).

In a gloss on this passage, Davidson emphasizes that even if indeterminacy of interpretation were somehow eliminated, the irreducibility of psychological concepts, and hence the impossibility of psychophysical laws, would remain (RLQ:348-9). Indeterminacy is important, he claims, because it draws attention to the fact that a theory of someone's propositional attitudes is holistic and necessarily governed by considerations of rationality and truth.

Davidson explicates his claim that rationality is constitutive of the mental by drawing a parallel between the attribution of intentional mental states to agents and the assignment of length to physical objects. The possibility of assigning numbers to measure length depends on the existence of a binary relation which is transitive, asymmetric, and holds throughout the domain of application. This relation is given an empirical interpretation in terms, for example, of some procedure. Unless this relation holds throughout the domain, it will be impossible to demonstrate an isomorphism of structure between the arithmetic of numbers and the empirical procedures used to measure length, and thus impossible to justify the use of a ratio (or even an ordinal) scale. The theory of fundamental measurement thus limits the domain within which numbers can meaningfully be used to register length to that domain where the empirical criteria apply transitively--the

domain of rigid objects. As Davidson puts it, 'the whole set of axioms, laws or postulates for the measurement of length is partly constitutive of the idea of a system of macroscopic, rigid, physical objects' (ME:221).

Just as certain conditions must be fulfilled if there is to be a meaningful assignment of numbers to measure length, there are requirements which must be satisfied if attitudes with propositional contents are to be attributed to an agent:

'The assignment of numbers to measure [length] assumes that a very tight set of conditions holds...Just as the satisfaction of the conditions for measuring length or mass may be viewed as constitutive of the range of application of the sciences that employ these measures, so the satisfaction of conditions of consistency and rational coherence may be viewed as constitutive of the range of applications of such concepts as those of belief, desire, intention and action.' (PP:236-7).

In the case of the attribution of subjective values and probabilities--the decision-theoretic analogues of desires and beliefs--the analogy with fundamental measurement in physics can be made fairly precise. Decision theory as Davidson describes it defines a rational pattern of preferences as follows: the preferences must be transitive, asymmetric, and connected in the set of alternatives, and preferences among risky alternatives must systematically reflect the assumption that an alternative is valued in proportion to its perceived likelihood of producing a valued outcome (HEA:268). As Davidson remarks, it seems intuitively plausible to say that a pattern of preferences satisfying these requirements is rational; 'if someone has a set of preferences that is not rational [as here

defined], it is possible to make book against him in such a way that whatever happens he will lose out by his own standards' (HEA:268; see FTV:145-6 for an example). In addition, though, their satisfaction ensures that the preference pattern 'can be proved measurable in the sense of an interval scale' (FTV:153; see 153-4 for discussion). The enterprise of assigning numbers to measure the relative strength of subjective values and probabilities thus commits us to reading rationality into the pattern of an agent's preferences:

'to the extent that we can see the actions of agents as falling into a consistent (rational) pattern of a certain sort, we can explain those actions in terms of a system of quantified beliefs and desires' (TT:160).

Just as we can use numbers to measure length only to the extent that the relation 'longer than' is transitive, so we must strive to interpret the relation 'preferred to' as transitive if we are to assign numbers to register the relative strength of desires and beliefs (HEA:273).

Let us grant that it must be possible to find a certain pattern in an agent's choices before quantified beliefs and desires can be ascribed, and that the pattern could justly be described as a rational one. But Davidson evidently holds to a further claim, namely that conditions of rationality must be fulfilled if propositional attitudes are to be ascribed at all, even where the use of numbers to measure their strength is not in question.⁴ The analogy between measurement in decision theory and fundamental measurement in physics seems to be based

on the necessity of setting up a numerical scale in each case; so what is the argument for the constitutive role of rationality where it is intentional contents, not numbers, which are being assigned? Events described as, for example, believings are described in intentional terms because they are individuated by their contents, and it is this identification by content, he claims, which requires that constraints of rationality be satisfied.⁵ Davidson writes that

'The cogency of a teleological explanation [of an action] rests...on its ability to discover a coherent pattern in the behaviour of an agent. Coherence here includes the idea of rationality both in the sense that the action to be explained must be reasonable in the light of the assigned desires and beliefs, but also in the sense that the assigned desires and beliefs must fit with one another...We weaken the intelligibility of attributions of thoughts of any kind to the extent that we fail to uncover a consistent pattern of beliefs and, finally, of actions, for it is only against a background of such a pattern that we can identify thoughts.' (TT:159).

This, presumably, is the reason why propositional attitudes are 'intrinsically holistic' (RA:318), since we can ascribe a single belief or desire to a person only if we also ascribe a rationally coherent system of attitudes; 'to have a single propositional attitude is to have...a pattern of beliefs that logically cohere' (RA:321). But why does the identification of thoughts require a consistent pattern of beliefs and actions? According to Davidson 'it is this pattern that determines the subject matter of the belief, what the belief is about' (TT:168), and he writes that

'Since the identity of a thought cannot be divorced from

its place in the logical network of other thoughts, it cannot be relocated in the network without becoming a different thought. Radical incoherence in belief is therefore impossible' (RA:321).

The claim that radical incoherence in belief is impossible may seem overbold, but it has a counterpart in the theory of measurement in physics; if the predicate ' $O(x,y)$ ' gives the empirical content of 'longer than,' the requirements of measurement entail that 'there do not exist three objects a , b and c such that $O(a,b)$, $O(b,c)$ and $O(c,a)$ ' (ME:220). Davidson holds that the assumption of rationality is built into the attribution of intentional states just as the assumption of transitivity is built into the application of the concept of length.

Up to this point, then, Davidson has argued for the similarities between psychological and physical theories. The application of both mental and physical concepts is governed by 'certain regulative and constitutive elements' (MM:254), certain interrelated conditions which must be fulfilled, and both types of theory are holistic:

'The nomological irreducibility of the mental does not derive merely from the seamless nature of the world of thought, preference and intention, for such interdependence is common to physical theory.' (ME:222).

Thus far physics and psychology are parallel; why, then, are psychophysical laws impossible? The reason Davidson gives is the difference between the constitutive elements of psychological and physical theory. As we have seen, beliefs, desires and intentions cannot be ascribed individually, on the

basis of 'local signs,' but must be ascribed 'within the framework of a viable theory' (ME: 221); and

'in inferring this system from the evidence, we necessarily impose conditions of coherence, rationality, and consistency. These conditions have no echo in physical theory, which is why we can look for no more than rough correlations between psychological and physical phenomena.' (PP:231).

Elsewhere he states that 'there cannot be tight connections between the realms if each is to retain allegiance to its proper source of evidence' (ME:222; cf. PP:239). In particular, there cannot be tight nomological connections between the mental and physical realms; he claims that 'there are no psychophysical laws because of the disparate commitments of the mental and physical schemes' (ME:222).

It appears, then, that Davidson's argument against psychophysical laws depends on his claim that the attribution of mental states with propositional contents is necessarily governed by considerations of rationality and truth, while the application of physical concepts is governed by a different set of conditions in which rationality plays no part. But how is the claim supposed to support the conclusion? Why is it that the disparity in constitutive elements reveals to us a priori that there cannot be genuine psychophysical laws? Davidson writes that 'nomological statements bring together predicates that we know a priori are made for each other--know, that is, independently of knowing whether or not the evidence supports a connection between them' (ME:218); elsewhere he claims that 'it

is a relation between predicates that makes a statement lawlike' (EON:226). 'All emeralds are grue' is not lawlike, in that it receives no inductive support from its instances; 'emerald' and 'grue' are predicates which are not suited to one another. This would suggest that the reason psychophysical generalizations cannot be lawlike is that mental and physical predicates are not suited to one another; and presumably it is our knowledge of the disparity in the constitutive commitments of mental and physical theory which is supposed to indicate to us that this is so.

However, Davidson does not want to claim that mental and physical predicates are completely ill-suited. As we noted earlier, psychophysical statements are not completely unlawfullike; they are heteronomic--roughly reliable, but not refinable into strict laws. Davidson does not say why it is that the disparity between the constitutive elements of mental and physical theory renders psychophysical laws lawlike enough to be reliable, rather than completely unlawfullike. But he does indicate why it is that these rules of thumb are not homonomic--that is, why they cannot be refined to yield strict laws.

Davidson states that

'confidence that a statement is homonomic, correctible within its own conceptual domain, demands that it draw its concepts from a theory with strong constitutive elements (ME:220)

and that

'the existence of lawlike statements in physical science

depends upon the existence of constitutive (or synthetic a priori) laws...within the same conceptual domain' (ME:221; emphasis added).

These remarks are puzzling, for they appear to indicate two different reasons for the heteronomicity of psychophysical laws. The second quotation suggests, as expected, that it is the fact that psychophysical generalizations bring together predicates from different domains with different constitutive elements which is responsible. But the first ties it to the fact that the theory from which the predicates are drawn lacks 'strong' constitutive elements. Now, Davidson evidently holds that physical theory has such elements, since their presence is responsible for the existence of homonomic generalizations in physical science. He also stresses the parallel between the constitutive role of rationality in the mental realm and that of measurement theory in the physical realm, and nowhere suggests that the constitutive elements of psychology are weaker than those of physical theory. It therefore seems likely that both mental and physical theory have strong constitutive elements, and correspondingly unlikely that the argument rests on the claim that it is the lack of such elements in psychology which renders psychophysical generalizations heteronomic. Perhaps we should reconcile the two quotations by concluding that constitutive elements can be strong only if they are unified--that is, only if they are not disparate. This is the reading I shall adopt.

I see three problems with the claim that psychophysical

generalizations cannot be made strict because they contain predicates drawn from domains with disparate constitutive elements. The first is that it is simply not clear why generalizations must contain concepts drawn from a theory with strong constitutive elements if they are to be homonomic. At ME:220 Davidson offers an 'illustration' of this claim, the illustration being the exposition of the 'laws or postulates for the measurement of length' (ME:221; discussed above, pp. 7-8). But the difficulty is to see how the illustration supports the conclusion. The exposition illustrates what strong constitutive elements are, and shows that physical science has them; but it does not show that this fact is responsible for the existence of lawlike statements in the physical sciences, nor that the absence of such elements entails heteronomicity.

The second problem with this argument is its scope. If, as Davidson suggests, it is the disparity in constitutive elements which is responsible for the heteronomicity of psychophysical laws, the specific nature of the disparity--the fact that one set of elements involves rationality while the other does not--is not crucial. Since it seems probable that the concepts of (at least some) other special sciences are drawn from theories with constitutive elements which differ from those of physical theory, the argument will render other sciences besides psychology nomologically irreducible. Davidson seems to accept this implication, thinking it likely that 'the concepts of biology are nomologically irreducible to the concepts of physics' (PP:241) and that 'most of our practical

lore (and science) is heteronomic' (ME:219). The fact that the argument, if sound, implies that we cannot expect strict laws in other areas does not make it unsound; but it does cause trouble for Davidson's claim that psychology is 'set off from the other sciences in an important and interesting way' (PP:241). He writes that 'the argument against strict psychophysical laws provides the key to psychology's uniqueness' (PP:241); but psychology evidently cannot be unique in being nomologically irreducible in virtue of having constitutive elements which differ from those of physics. If this line of argument can secure the anomalism of the mental, it will do so at the cost of rendering strict laws in many other domains impossible and thus blurring the desired contrast between psychology and the other sciences (cf. Lycan 1981:29).

The third problem, alluded to earlier, arises from the fact that the argument for the heteronomicity of causal psychophysical laws is also supposed to rule out psychophysical bridge laws (thus securing the irreducibility of the mental). The difficulty is that even supposing that the argument from disparity in constitutive elements to heteronomicity succeeds, it is very difficult to make sense of the notion of heteronomicity as applied to bridge laws, rather than causal laws. To see this, suppose that we have found a rough statistical correlation between decisions to act (events of type D) and firings in the premotor area of the brain (events of type P), and we propose $(x) (Dx \leftrightarrow Px)$ as a candidate bridge law. Davidson claims that this cannot be a homonomic

generalization, since it is psychophysical; it must be heteronomic. If it is heteronomic, its instantiations will give us reason to believe that there is a strict law at work which could be stated in a different vocabulary; this is part of the definition of heteronomicity. There are two candidates for this vocabulary. If it is that of the ultimate physical theory which will yield 'a standardized, unique description of every physical event' (ME:223), the law will be a statement of self-identity and so not a law at all. If it is the vocabulary of what we broadly think of as physical science (which includes biochemistry and physics), the precise law will state, say, that some complex biochemical property and some complex physical property are always coextensive. But surely the concepts of biochemistry and physics are not drawn from a unified theory with the same constitutive elements; if 'most of our science' is heteronomic, it is highly probable that statements linking biochemical predicates with those of mathematical physics are just as 'conceptually hermaphroditic' (PP:241) as psychophysical generalizations. In that case we are forced back towards the first option, and thus towards the conclusion that the law supposedly at work cannot be made strict. If this is so, there cannot be strict bridge laws underlying rough psychophysical bridge laws, and it is therefore hard to see how the latter can truly be described as heteronomic. Davidson could revise his claim and hold only that psychophysical bridge laws are not homonomic (not refinable into strict laws), or he could drop the requirement that strict

laws underlie heteronomic laws; but a more basic problem would remain. That problem is that the argument from disparate commitments seems to show that no bridge law can be strict, since all such laws are of their nature conceptually hermaphroditic. If this is the case no special science will be reducible to physics (at least via strict bridge laws). It seems unwise to accept an argument with such a strong conclusion, unless further support is given for the claim that strong constitutive elements are required if strict laws are to be attainable.

Perhaps the central difficulty with Davidson's argument against psychophysical laws is that raised by the first objection--the fact that he offers so little discussion of why disparity in the constitutive elements of two theories gives us reason to rule out the possibility that statements linking them could be strictly lawlike. We are apt to feel that this is just not the sort of consideration that could justify us in disqualifying such an eventuality in advance. This feeling is surely not due to the fact that we have a good grasp of the sort of evidence that would justify such a decision, but to the fact that, as Davidson puts it, 'in general the grounds for deciding to trust a statement on the basis of its instances will...be governed by theoretical and empirical concerns not to be distinguished from those of science' (ME:216). To this Davidson responds as follows:

'If the case of supposed laws linking the mental and the physical is different, it can only be because to allow

the possibility of such laws would amount to changing the subject. By changing the subject I mean here: deciding not to accept the criterion of the mental in terms of the vocabulary of propositional attitudes.' (ME:216).

When set beside Davidson's claim that there cannot be tight connections between the mental and physical realms 'if each is to retain allegiance to its proper source of evidence' (ME:222), this remark suggests a strategy for deriving the impossibility of psychophysical laws from disparity in constitutive commitments. The strategy (which has a long history) is to argue that admitting the possibility of psychophysical laws is admitting the possibility that propositional attitudes could be attributed on the basis of physical evidence; bridge laws would allow attitudes to be ascribed on the basis of brain states, causal laws on the basis of physical causes. But then considerations of rationality would no longer have primacy in the ascription of attitudes, and the theory of an agent's attitudes is necessarily governed by concern for rationality and coherence (BBM:154). Therefore there cannot be strict psychophysical laws; to accept that there can be is to be guilty of 'changing the subject' (ME:216) by allowing the considerations of rationality which are constitutive in the psychological realm to be overridden by physiological evidence.

It is surely true that we would search for candidate psychophysical laws by first attributing mental states (in accordance with rationality) and then looking for physical events that regularly accompanied the occurrence of those

states. In this sense, considerations of rationality would have primacy in our attempt to identify the physical correlates of psychological events. But the argument purports to show that such correlations as we might propose could not hold up. Let us imagine that we have repeatedly found mental events of type I_1 to be accompanied by (or caused by) neurological events of type N_1 . Now, it is always possible that this correlation should break down--if, say, we find a subject whom we interpret as showing I_1 without N_1 . The Davidsonian point is that in such cases the correlation breaks down because we do not allow the physical evidence to condition our intentional attributions; our ascriptions of reasons and intentions are governed by 'considerations of overall cogency' (ME:223), by the requirement of finding the agent rationally intelligible, and not by observed regularities in brain events. Perhaps this qualifies an instance in which the disparity in the commitments of the mental and physical schemes can be used to argue that observations of psychophysical correlations do not confirm exceptionless psychophysical laws.

Insofar as this argument relies on the logical possibility that the putative law might break down, it is implausible; a law sets limits on what is physically possible, not on what is logically possible. It is not enough that the uniformity might break down; to be successful, the argument must persuade us that no matter how often we observed the correlation, we would never have reason to believe it more than roughly true. (Recall that psychophysical generalizations are

supposed to be heteronomic, not unlikelike.) It seems somewhat implausible that if the correlation were observed often enough, we would not eventually come to regard it as a genuine psychophysical law. But let us suppose that the one-one correlation does break down as envisaged. We would surely conclude from this that mental events of type I_1 are physically instantiated in different ways in different brains (or in the same brain at different times). If this were the case for mental events in general, we would not be able to give necessary and sufficient conditions for the occurrence of a mental event in physical terms.

Would this mean that there could be no strict lawlike links between the mental and the physical? It does not seem impossible that at some point in the future our knowledge of psychology and physiology might reach a point at which we could demonstrate, say, that any system capable of solving complex problems as quickly as we do would have to be composed of neuron-like material. Such laws would impose physical constraints on possible instantiations of our cognitive capacities, but they would not be bridge laws enabling us to reduce psychology to physics. The possibility of such instantiation laws thus does not threaten Davidson's claim that psychology is nomologically irreducible to physical science. But it does suggest that the Davidsonian objections we have considered do not suffice to rule out the possibility of strict lawlike links between the mental and the physical.

The conclusion towards which the discussion has so far

been tending is that it is difficult to find a construal of Davidson's argument against psychophysical laws which does not face substantial problems. The argument seems to render other special sciences nomologically irreducible; and though it may draw attention to the possibility of multiple physical realizations of mental events, it leaves open the possibility of psychophysical instantiation laws. However, even if an argument could be constructed which would show psychology to be anomalous among the sciences in being irreducible, the question of the anomalism of the mental would remain. The mental is anomalous only if both psychophysical and purely psychological laws are impossible. Let us now turn to Davidson's arguments against the latter.

3 Purely Psychological Laws.

If psychophysical laws are ruled out by the nomological incompatibility of psychological and physical predicates, might we expect laws employing psychological predicates alone? Davidson's remark that generalizations which draw their concepts from a theory with 'strong constitutive elements' may be expected to be homonomic makes the prospects seem favourable. The claim that psychology has constitutive elements is an integral part of the argument against psychophysical laws, and though it is not clear what the force of 'strong' is here, Davidson does not suggest that the constitutive elements of psychology are weaker than those of physical science. However, statements which are homonomic must also draw their

concepts 'from a comprehensive closed theory' (ME:219), and psychology does not meet this requirement:

'the mental does not, by our first principle [at least some mental events interact causally with physical events], constitute a closed system. Too much happens to affect the mental that is not itself a systematic part of the mental.' (ME:224; cf. PP:231, 241).

Davidson uses instances of action and perception to illustrate the interaction of mental and physical events. Perception shows that, as he puts it elsewhere, 'psychological events and states often have causes that have no natural psychological descriptions' (PP:241).⁶ This non-closure of the mental is the sole reason which Davidson gives for the impossibility of psychological laws; conjoined with the argument against psychophysical laws, it is supposed to secure the anomalism of the mental. But the reason seems a peculiar one. It is hard to see why the interaction of psychological and physical events should have such a sweeping consequence; if it did, we would have an a priori argument against the possibility of there being laws of any special sciences (i.e. sciences other than physics). The fact that psychology deals with the realm of the rational would play no part in securing the anomalousness of psychological events. Any special science deals with the restricted domain of events describable in its specialized vocabulary, events which interact with others not describable in this way; for example, the proliferation of cancer cells (a biological event) is sometimes caused by ultra-violet irradiation (a physical event without a natural biological

description). Of course the interactions of psychological events with physical events could not be explained or predicted on the basis of purely psychological laws, but this in itself seems to provide no reason for doubting the possibility of such laws.⁷

Though Davidson's official reason for ruling out purely psychological laws appears problematic, we may be able to gather some clues to his objection to them, and to his notions of comprehensiveness and closure, by looking at his discussion of laws linking reasons and actions. Davidson does not explicitly designate such laws as psychological, and the remarks we will consider are intended to contribute to the argument against psychophysical laws by demonstrating the pervasiveness of considerations of rationality in the psychological sphere. But reasons and intentional actions clearly fall within the realm of the mental for Davidson, so laws linking them can justly be described as purely psychological.

The conclusion of Davidson's discussion is, as might be expected, that 'there are no serious laws of this kind [sc. reason-action laws]' (PP:233). He tells us that what rules out such laws is also what prevents us from giving necessary and sufficient conditions for acting intentionally (PP:233). To see this, we must consider the form which Davidson's reason-action laws would have to take if they were to be serious. That form is as follows: 'whenever a man has such-and-such beliefs and desires, and such-and-such further conditions are satisfied, he

will act in such-and-such a way' (PP:233). Davidson says elsewhere that a law of this form 'would be understood as a causal law, of course, but it would not need to mention causality' (FA:80). If we had such laws, we would be able to give causal deductive-nomological explanations and predictions of roughly the following form:

(L) Whenever a man has such-and-such beliefs and desires and such-and-such further conditions are satisfied, he will act in such-and-such a way.

(C) A at t has such-and-such beliefs and desires and such-and-such further conditions are satisfied.

(E) A will act in such-and-such a way at t.

(L) here is a schematic causal law, the antecedent of which must be filled in to give the conditions which are causally necessary and sufficient for an intentional action to occur. But what does the problem of filling in the conditions under which a reason will cause an action have to do with the problem of giving a reductive analysis of the concept of intentional action? The connection is that in Davidson's view an action is intentional in virtue of its causal history.

According to his causal analysis of intentional action, to act intentionally is to act on a reason, and an agent acts on a reason only if the reason causes the action (I:87). This is intuitively plausible; I might have many good reasons for acting as I did, and yet not have acted thus for those reasons. The reason for which I acted as I did is the reason which caused me to act as I did. It is a necessary condition, then,

for an action to be intentional that it be caused by a reason. Is this also sufficient? Davidson offers a case to show that it is not.

'A climber might want to rid himself of the weight and danger of holding another man on a rope, and he might know that by loosening his hold on the rope he could rid himself of that weight and danger. This belief and want might so unnerve him as to cause him to loosen his hold, and yet it might be the case that he never chose to loosen his hold, nor did he do it intentionally' (FA:79).

Here the belief and desire are such as to make the climber's loosening his hold a rational thing to do, and they do cause him to loosen his hold, yet they do not cause the loosening in such a way as to make it intentional. For an action to be intentional, it must not only be caused by a belief and a desire which provide a reason for doing it, it must also be caused by them 'in the right way, perhaps through a chain or process of reasoning that meets standards of rationality' (PP:232). But if Davidson is right, we cannot fill in the further conditions mentioned in (L) in a non-questionbegging way. Our attempts to fill (L) out are not 'serious laws' because their antecedent conditions cannot be specified in a way which makes it possible to test whether they are fulfilled in advance of the occurrence of the event to be explained. 'What is needed, if reason explanations are to be based on laws, is...a test...of when a person's reasons--his desires and beliefs---will result, in the right way, in an action' (HEA:267). Without an account of how reasons are weighed, we

cannot predict which one will be acted upon, although we can retrospectively identify some reason as the cause of an action which has occurred. He writes that

'Explanation by reasons avoids coping with the complexity of causal factors by singling out one, something it is able to do by omitting to provide, within the theory, a clear test of when the antecedent conditions hold.'
(PP:233).

So it seems that if we understood 'the way in which desire and belief work to cause the action' we would know the complex causal conditions for intentional action, we would have a clear test of when the conditions held, and we could generate some serious psychological laws and use them to predict actions.

However, Davidson appears to rule out such an understanding:

'What is needed in the case of action, if we are to predict on the basis of desires and beliefs, is a quantitative calculus that brings all relevant beliefs and desires into the picture. There is no hope of refining the simple pattern of explanation on the basis of reasons into such a calculus' (PP:233).

Elsewhere he supports this claim by pointing to the explanatory poverty of the practical syllogism:

'Any serious theory for predicting action on the basis of reasons must find a way of evaluating the relative force of various desires and beliefs in the matrix of decision; it cannot take as its starting point the refinement of what is to be expected from a single desire. The practical syllogism exhausts its role in displaying an action as falling under one reason; it cannot be subtilized into a reconstruction of practical reasoning, which involves the weighing of competing reasons'

(ARC:16).

This passage suggests that though the practical syllogism is too crude, decision theory, which 'offers a more sophisticated way of dealing with reason explanations' (HEA:268), might provide serious laws for predicting actions.

Davidson does not state explicitly why it is that decision theory cannot yield such laws, but his discussion suggests an argument which might be offered to support the claim. Recall that decision theory, as Davidson describes it, specifies a set of conditions on a rational pattern of preferences. If a subject S has such a pattern of preferences, numbers can be assigned to measure the relative value and subjective probability which various outcomes have for S. The theory also claims that a subject with a rational pattern of preferences always chooses an outcome such that no other has a higher expected value (HEA:268). Why is it that such a theory of decision is not a 'scientific theory of behaviour' (PP:235)?

Davidson's discussion of decision theory suggests that the problem is that the theory suffers from the same defect as the reason-action laws mentioned earlier; we do not have a clear test of when it holds. To determine whether decision theory is true of some subject S, we must discover whether S has a rational pattern of preferences. To do this, we need to give what Davidson calls 'a clear behaviouristic interpretation' to the claim 'S prefers A to B' (PP:270). The problem is that our standard for accepting a proposed

interpretation is that the assignments of subjective probability and value it generates make S's choices appear rational. We thus cannot determine subjects' values and probabilities independently of deciding to see them as acting to maximize expected value (HEA:272). But if we can only say that the theory applies wherever we can make it apply, we evidently cannot state its conditions of application in a way which does not beg the question. And if we cannot give a clear test of when the theory applies, we cannot use it to predict actions.

If this is Davidson's objection to strict purely psychological laws, and hence to a scientific psychology, we should note that its scope appears to be rather limited. It seems that it casts doubt on the possibility of a scientific psychology only if a scientific psychology is identified with a set of laws for explaining and predicting action, and only if decision theory is our best prospect for producing such laws. But these are premises which Davidson simply assumes; they are not argued for. In fact, all the arguments against purely psychological laws discussed above are informed by a specific conception of the form a genuine scientific theory should take. Let us pause to illustrate this before inquiring whether this form is the form taken by psychology.

Davidson's official reason for ruling out strict purely psychological laws is that psychological events interact with non-psychological events, but his remarks on the practical syllogism and on decision theory seem to indicate a

different reason. His objection to reason-action laws stresses the impossibility of specifying 'the way desire and belief work to cause the action' (PP:233), while the objection to decision-theoretic laws turns on the impossibility of gaining independent access to the values and beliefs which cause choices. Despite their differences, however, all three arguments could be taken to suggest that psychology does not constitute a 'closed, comprehensive' theory. The 'non-closure' argument points to the fact that the events which psychology attempts to explain and predict are affected by events which do not fall within the domain of psychological theory, while the other two arguments imply that explanation in terms of beliefs and desires (or beliefs and values) is incapable of taking into account all the causal factors contributing to intentional action. Together, the arguments purport to show that there are psychological and non-psychological influences on psychological events which cannot be taken into account in psychological explanation. Perhaps the fundamental objection against purely psychological laws is that they could not be strict and predictive as long as the events they aim to predict are subject to causal influences which are not characterizable or quantifiable within the theory.

Uncovering this underlying objection not only reveals the links between closure, comprehensiveness, and strictness, but also shows that the objection against purely psychological laws is based on an extremely restrictive view of what constitutes genuine science. Basic physics is genuine science, for

Davidson, since it does provide a closed, deterministic system (ignoring quantum indeterminacy, as he notes) of precise, exceptionless causal laws (ME:224). The precision of these laws means that they can be causal without mentioning causality; Davidson writes that if we had genuine reason-action laws

'We would simply say, given these (specified) conditions, there always is an intentional action of a specified type. This would be understood as a causal law, of course, but it would not need to mention causality. Unavoidable mention of causality is a cloak for ignorance; we must appeal to the notion of cause when we lack detailed and accurate laws' (FA:80).⁸

The notion of causality and explanation at work here is instantly recognizable in this passage from Hempel:

'the everyday conception of causal explanation is rather narrow and vague and...at least in physics it has been replaced by the more general and precise conception of an explanation by means of a deterministic theory...[G]iven the "state" of a closed system...at some time, the theory determines the state of the system at any other time and thus permits the explanation of a particular state of the system by reference to an earlier one.' (Hempel 1965:487n).

Given a closed system and a deterministic theory, on this view, there is a 'structural equality of explanation and prediction' (Hempel 1965:234); an event (a change in the system) is predicted or explained by being deduced from the prior state of the system using the laws. The fact that the PAM states that there are no laws on the basis of which mental events can be predicted or explained, suggests that Davidson too regards

these as symmetrical; elsewhere he writes that psychological events are not 'amenable to precise prediction or subsumption under exceptionless causal laws' (PP:239). There is little doubt that for Davidson a genuinely scientific theory is one which provides precise, exceptionless causal laws which are used to predict and explain particular events by deductive subsumption. The laws of such a theory are exceptionless because the theory takes into account all the causal factors affecting events in its domain, not (as in psychology) because they are insulated from counterexample by ceteris paribus clauses.

4 Conclusions.

If Davidson's criticisms of psychology are based on the assumption that it attempts (without success, of course) to provide a closed, deterministic theory yielding strict causal laws of behaviour, we need to ask whether this is what psychology aims to do. We may suspect that it is not when we note that research in cognitive psychology is typically directed to explaining capacities, such as memory, language comprehension, and problem-solving, rather than to predicting when and how people will act intentionally. Moreover, there is a plausible model of psychological explanation which portrays it not as subsumption under causal law, but as functional analysis. My aim here is not to defend the details of this account; that is ably done elsewhere (Cummins 1983, Haugeland 1981), and I shall only give a brief sketch of the ways in

which it differs from the subsumptive model. But the sketch will, I hope, suffice to suggest that the features of psychology which for Davidson indicate that it is not genuine science are rather consequences of the fact that it explains by the analysis of capacities rather than by the subsumption of events. If this is so, he cannot construe them as indications that psychology is an inferior science without giving us convincing reason to believe that this type of explanation is itself inferior.

To say that psychology explains cognitive capacities by giving a functional analysis, is to say that the capacities are explained by showing how simpler functional units could be organized to form a system with the cognitive capacities in question. If it were possible to see how the simpler functions could be carried out by a physical system, it would be intelligible how the overall cognitive capacity could be physically instantiated. Such a theory would enable us to show how the capacity is instantiated in a particular type of system, rather than giving us causal laws subsuming changes of state in a particular type of system, but it would still yield explanations of psychological phenomena. Though we would not be able to predict psychological events with complete certainty (for reasons to be discussed shortly), we would be able to understand how they could come about by understanding them as the results of the exercise of a cognitive capacity, and explaining how the capacity was instantiated in the system.

The hypothesis that psychology yields functional

explanations would show why we cannot expect to find psychophysical bridge laws. Even though we might be able to discover how some cognitive capacity is instantiated in a particular type of physical system (our brains, say), it would remain impossible to give necessary and sufficient physical conditions for the instantiation of the capacity in question. Physically diverse components, organized in different ways, could carry out the functions necessary to endow a system with the cognitive capacity in question. But we cannot give necessary and sufficient physical conditions for being a clock, either, for the class of clocks is functionally defined⁹ (they are used for something--to tell the time), and functional definitions do not translate into first-order physical terms. As Cummins remarks, 'multiple instantiation blocks reduction because there are truths about the instantiated property that are not truths about the instantiations' (1983:23). But then any science which uses the method of functional analysis will produce theories which are 'irreducible' in the sense of not yielding biconditional bridge laws; psychology is no worse off than, say, biology.¹⁰ (Davidson remarks at one point that he suspects that biology also cannot be a 'closed science,' but that he does not know 'how to show that the concepts of biology are nomologically irreducible to the concepts of physics' PP:241.) Davidson's stress on psychological methodology was at least suggestive.

Seeing psychological explanation as functional analysis also reveals the sense in which psychology does not constitute

a closed system and cannot produce strict laws. We are supposing that psychology explains by giving analyses of complex capacities into subcapacities, and repeating this process for the subcapacities until the level of physical instantiation is reached. Now, suppose that we had accomplished a full functional analysis of some psychological capacity C, and explained how it was instantiated in some physical system S. It would seem that we could use the analysis to predict the effects of some input on S given its initial state. But of course our predictions would hold only as long as the system was functioning correctly; they would not themselves allow us to predict and allow for physical changes in S which would stop it from instantiating our analysis of C (cf. Lycan 1981:30). Such changes could be predicted and explained at the physical level (though not, of course, as malfunctions) but they could not be taken into account at the psychological level. Our predictions would always be subject to the proviso that the system be functioning correctly; and it might well be that the only way for us to tell that it is functioning correctly is to see whether it behaves in accordance with our predictions. In this sense we cannot 'determine in advance whether the conditions of application [i.e. normal functioning] are satisfied' (PP:233). But this limited predictive power is not a failing of the explanatory strategy of psychology; the aim of the strategy is to see the behaviour of cognitive systems as manifestations of capacities which we understand, not to predict when those systems will cease to possess those

capacities.

Perhaps our conclusion should be that what Davidson has been pointing to is the anomalism of the functional--the fact that sciences employing the strategy of functional analysis are not reducible and do not yield strict causal laws. But this type of anomalism cannot be construed as a defect of the sciences in question, since they do not attempt to explain the phenomena in their domains by subsuming them under such laws. Though they may not yield strict causal laws, they can still yield satisfying explanations by showing how complex biological and psychological capacities could be instantiated in matter.

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Footnotes.

- 1 It might seem that this reasoning would show that quantum physics is not a serious science, because it deals in probabilistic rather than exceptionless laws--a conclusion Davidson would wish to avoid. But the laws of physics, though probabilistic, are what Davidson calls 'serious'; he writes that 'the statistical laws of physics are serious because they give sharply fixed probabilities, which spring from the nature of the theory.' (PP:233).
- 2 Some terminology: in this paper I shall follow Davidson in using 'mental' and 'psychological' interchangeably. By 'psychological laws' I shall mean any laws employing some mental vocabulary. The category of psychological laws thus includes both psychophysical and purely psychological laws.
- 3 Some examples: Haugeland 1981:252; Elgin 1980.
- 4 This further claim may not be required to establish Davidson's desired conclusion, but he does appear to endorse it.
- 5 Earlier it was noted that for Davidson an event is mental iff it has a description containing at least one propositional attitude verb, used non-extensionally (ME:210); the feature which he takes to be distinctive of the mental is that 'it exhibits what Brentano called intentionality' (ME:211). Yet it is apparently rationality, rather than intentionality, which sets the mental apart from the physical. Davidson therefore needs to show why it is that the intentional can be assimilated to the rational.
- 6 Davidson's reasons for holding that the mental is not closed are curious when viewed as part of an argument for monism. Until monism is established, the fact that some mental and physical events interact causally should suggest that neither the mental nor the physical constitutes a closed system, so the passage from ME:224 quoted in the text does not appear to justify the asymmetry. The second formulation (from PP:241) fares better only if it is plausible that physical events and states do not have causes which have no natural physical descriptions. (To take this as obvious is surely to assume monism.) The plausibility of the claim depends on which descriptions are 'natural'. If my arm moves because I want it to, is there a natural physical description of the cause of the movement? It seems most natural to say that it was caused by my desire. To say that the movement is caused by some physical event in my nervous system may be natural for an identity theorist, but it begs the question.

- 7 The Principle of Causal Interaction plus the impossibility of psychophysical laws would yield the conclusion that there were some psychological events which psychology could not predict or explain (e.g. perceptions). But this is weaker than the Principle of the Anomalism of the Mental, which states that no psychological events can be predicted or explained (under their psychological descriptions). The weaker claim reveals a sense in which the psychological does not constitute a closed system; but one still wants to know why this lack of closure prevents purely psychological laws.
- 8 Compare: 'Strict laws do not deploy disposition terms nor do they use causal concepts' (RS:246). The ineliminable reference to causality in our talk of the relationship between mental states and actions is a result of our inability to state 'the causal conditions of intentional action...in a way that would eliminate wrong causal chains' (FA:80), and is a symptom of the impossibility of making reason-action generalizations strict by stating their antecedent conditions precisely.
- 9 The horological example is McGinn's (1978:199; 1980:180). McGinn's construal of Davidson is in some respects similar to mine, but I am not persuaded of his conclusion that 'mental states enjoy no sort of real essence whatsoever' (1978:201).
- 10 Perhaps I should say that in general any science invoking properties with variable physical instantiations will be irreducible to physics. Perhaps this would explain why the physical terms of our 'practical lore' (e.g. 'window', 'rock', ARC:16) are heteronomic with respect to the predicates of physics.

SECOND PAPER

CONSTRAINTS ON CONTENT

Constraints on Content.

1 Introduction.

1.1 Burge's Thought-Experiment.

Some recent studies of how the content of intentional mental states is individuated in ordinary discourse have caused much debate. These studies are striking because they are taken to show that the contents of a person's thoughts are not determined by (do not supervene on) his or her phenomenological, functional or physiological states. The contents of a person's thoughts, it is claimed, may be affected by features of the external environment of which he or she is entirely ignorant. The arguments which are taken to lend support to this conclusion are described in Putnam (1975) and Burge (1979). The argumentative strategy is to test claims about the determinants of mental content by describing thought-experiments in which physically and functionally type-identical subjects occupy different environments. It is then argued that our practice of thought attribution dictates that such subjects have propositional attitudes with different contents, since different belief ascriptions are true of them.¹ The topic of this paper is the ingenious thought-experiment described by Burge (1979) and the conclusion he draws from it. Burge's conclusion is that the linguistic practices of the community to which a person belongs partly determine the contents of his or her intentional mental states. The thought-experiment held to support it can be described as follows.

Burge invites us to consider an English-speaker--let us call her Jane--who misuses the word 'arthritis', applying it to rheumatoid diseases in the bones as well as in the joints. This, Burge argues, does not prevent us from reporting her beliefs using ascriptions in which the word 'arthritis' occurs in the content-clause. Suppose that Jane says to her doctor 'I have arthritis in my thigh,' that the doctor replies 'You can't have arthritis in the thigh; arthritis is a disease of the joints,' and that Jane accepts that her belief was false. It seems that the natural way for us to report the belief she expressed is with

(1) Jane believes that she has arthritis in her thigh. We then consider what beliefs she would have had if she had been a member of a different linguistic community in which the word 'arthritis' was correctly applied to rheumatoid diseases of the bones as well as the joints. The counterfactual condition differs only in that in it correct use encompasses Jane's actual misuse; Jane's functional and physical constitution remains the same. Let us call Jane in this counterfactual condition 'Jane2.' Jane2 also utters the words 'I have arthritis in my thigh'; but in so doing, Burge argues, she expresses a belief not about arthritis but about the more inclusive group of rheumatic conditions. This being so, we cannot report this belief in the same way we reported Jane's; Jane2 does not believe that she has arthritis in her thigh. Thus Jane has at least one belief which Jane2 lacks. Since

Jane and Jane2 are physically and functionally identical and differ only in the linguistic communities to which they belong, the difference in their belief contents must be attributed to the difference in their linguistic environments. Burge concludes that 'propositional attitudes depend partly for their content on social factors independent of the individual asocially and non-intentionally construed' (1979:85).

Burge's conclusion challenges what he calls the 'individualistic presuppositions' of many traditional views of the mind (1979:94). According to such views, the thoughts a person has are determined by intrinsic properties of that person--properties he or she has taken in isolation from the external environment. Let us say that thoughts which thus supervene on individual constitution have individualistic or narrow content, and let us call the individuating practice which picks them out individualistic individuation. We may then say that Burge takes himself to have shown that our everyday practice of propositional attitude ascription is non-individualistic, and that the thoughts we attribute to one another in ordinary discourse have non-individualistic or 'wide' content.

1.2 The Continuity Thesis.

Burge's conclusion that the content of propositional attitudes is partially determined by linguistic environment, if warranted, casts doubt on any theory of propositional attitudes which represents them as having individualistic content. In particular, it casts doubt on the thesis (which I shall call

the Continuity Thesis) that common sense explanations of behaviour in terms of beliefs, desires and other propositional attitudes will be vindicated by cognitive psychology, in that cognitive psychology will invoke explanatory states with the properties of common sense mental states. If we make the plausible and widely-held assumption that the psychological states of cognitive psychology are individualistically individuated, in that they are not dependent on linguistic usage in the community to which the subject belongs, it is hard to see how the Continuity Thesis can be true. According to Burge, propositional attitudes are non-individualistically individuated; so his conclusion, if warranted, would show that the psychological states invoked by cognitive psychology and the intentional states ascribed in common sense discourse have different identity conditions. Two people with identical psychological states might yet have different propositional attitudes if they belonged to different linguistic communities. Burge's conclusion indicates a substantial dissimilarity in the explanatory states invoked in cognitive psychology and in common sense; the prospects for the Continuity Thesis seem dim.

I should note that this is not a conclusion Burge would accept, since he holds that the explanatory states of cognitive psychology, like those of common sense, are individuated with respect to the environment. He has argued that Marr's theory of vision invokes representational states whose content depends on the nature of the subject's physical

environment. But this claim does not suggest that any part of cognitive psychology attributes states which are sensitive to subjects' linguistic environments. Nor does the argumentative strategy Burge there employs readily generalize to the linguistic case. He relies on the claim that the content of a representational state of type R in an environment E depends on what features of E normally cause tokens of R (Burge 1986:32). He concludes from this that changes in environmental features can change the contents of representational states. But in the linguistic case, the norms of usage which supposedly affect the contents of a subject's beliefs are not the causes of those beliefs, so the assumption of a causal theory of content is no help here. In any case, the question we are concerned with is whether Burge's thought-experiment should lead someone who believes that cognitive psychology is individualistic to abandon the Continuity Thesis, so I shall assume that cognitive psychology does individuate intentional states individualistically.

Reactions to the challenge presented to the Continuity Thesis by Burge's conclusion have, by and large, taken two forms. One response has been to accept that Burge has shown that psychological states and propositional attitudes are individuated in very different ways. I shall say that proponents of this response adopt the Schismatic View, since they concur in embracing the idea that the propositional attitudes and psychological states attributed in common sense contexts and in cognitive psychology respectively have

different identity conditions. They concur in little else, however, for some holders of the Schismatic View champion cognitive psychology as the true theory of the mental (e.g. Stich) while others suggest that cognitive psychology has been shown to be unable to capture an essential feature of the mental (e.g. Pettit and McDowell 1986:14).²

The other response to Burge's challenge to the Continuity Thesis consists in the adoption of some variety of what I shall call the Ecumenical View. This is the view that we can extract a notion of individualistic content from common sense practice as well as the non-individualistic notion to which Burge draws attention. If the Ecumenical View can be defended, there is at least a possibility that the Continuity Thesis may be reinstated, for content individuation in common sense and in cognitive psychology will have been shown to have individualistic elements.

The aim of this paper is to argue that content individuation in common sense discourse is not uniformly non-individualistic. I shall defend a version of the Ecumenical Thesis, not out of allegiance to the Continuity Thesis, but because I doubt that Burge's thought-experiment gives us reason to reject the Ecumenical View in favour of the drastic Schismatic alternatives. In fact it seems that the Ecumenical View provides a more accurate picture of our common sense practice of attitude ascription. I shall present some examples which suggest that when giving common sense explanations of

action we do not individuate thoughts with reference to agents' linguistic environment, as Burge maintains we do. If mental states are individuated in the same way (i.e. individualistically) in common sense and in scientific explanations, the challenge supposedly presented to the Continuity Thesis by Burge's thought-experiment is rebuffed.

Before elaborating on these claims, I shall examine the two lines of argument most commonly offered to support the introduction of a common sense notion of individualistic or narrow content. Determining the ways in which these arguments fall short will point the way to a more satisfactory defence.

2 Arguments for Individualistic Content.

2.1 Narrow Content and Causal Powers.

A common argument for the claim that Jane and Jane₂ should be taken to have mental states with the same content runs along these lines: the subjects are functional duplicates, so they will produce the same outputs (behaviour) when in the same internal state and receiving the same inputs. But mental states are invoked to explain behaviour; so Jane and Jane₂ should be ascribed type-identical mental states to explain their type-identical behaviour.

However, this argument begs the question by assuming that the behaviour of the two is to be described in the same way. It is far more plausible that according to Burge's interpretation of his thought-experiment, Jane and Jane₂ perform different actions as well as having different beliefs, desires, and intentions. After all, his thought-experiment is

supposed to tell us about the individuation of intentional entities in general, including intentional actions. As a member of our speech community, Jane can be described as fearing that her arthritis has spread to her thigh, and as going to the doctor with the intention of discovering whether her fear is justified. Jane2, as a member of the other community, does not have that fear, nor does she perform that action. If the thought-experiment shows that beliefs are individuated non-individualistically, it would also seem to show that actions are individuated in the same way. Simply stipulating that the behaviour of Jane and Jane2 is the same is not sufficient to cast doubt on this obvious implication of the thought-experiment.

The proponent of narrow content might now object that there is no means by which the difference in the content of Jane's and Jane2's mental states could produce different actions. The argument here is that since the two are physiologically and functionally the same, there is no physical difference between them which could underwrite the difference in the causes of their actions. But to say this is just to reiterate the point that when thoughts and actions are individuated in the way that Burge's case seems to show that they are in common sense practice, they do not supervene on individual constitution. Surely claims about the relationship between the mental and the physical are constrained by what we can discover about the nature of mental states. The fact that

what the thought-experiment tells us renders individualistic supervenience inadequate as a theory of that relationship does not motivate the introduction of an individualistic notion of content.

Fodor (1987:39) argues for the introduction of narrow content along lines similar to those just described. He claims that individuating psychological states with reference to linguistic environment would be acceptable to science only if there were a causal mechanism by which differences in environment produced differences in the physiological states assumed to realize psychological states. This requirement clearly is not met in Burge's case, since the difference in Jane's and Jane2's linguistic affiliation does not cause any difference in their physiological states. But this argument cannot motivate the introduction of individualistic individuation as part of our common sense practice, since Fodor is surely right in assuming that that the constraints on individuation which rule out non-individualistic individuation apply (if they apply at all) only to individuation in science. In other words, cognitive psychology can perhaps be plausibly held to be antecedently committed to individualistic supervenience, but our common sense practice of action explanation cannot.

2.2 Narrow Content and Point of View.

While the arguments discussed in the previous section sought to motivate the ascription of shared thoughts on the basis of physical and functional similarity, the present argument

attempts to do so on the basis of phenomenological similarity. In arguing that there is a sense in which Jane and Jane2 share beliefs, it is often claimed that they have the same point of view, or that they see the world in the same way. This line of argument seems to inform these remarks of Blackburn's:

'On twin-earth there are people who love XYZ, who bathe a lot, fish and own yachts, and wake up looking forward to a day on the XYZ, or giving themselves kinaesthetic pleasures imagining the soft cool XYZ trickling over their bodies. Such a twin-earthier is just like a water-loving earthmen: in fact, if he were suddenly transported here he would not notice the difference...Our psychologies are determined by the way we react to what we are aware of. Since the features whereby we are aware of things are universal (water...can look or sound or taste or appear the same as other things of the same kind or even different kinds, so are psychologies.' (Blackburn 1984:326).

Since Jane2 is simply Jane in a different environment but with the same phenomenological experiences, ex hypothesi there is no difference she notices between the two linguistic environments. Presumably, then, Jane and Jane2 are aware of the same things and have the same thoughts (this is what Blackburn means by having the same psychologies).

But this argument is too swift. It slides from a claim about the phenomenological similarity of subjects' qualitative experiences to a claim about the propositional content of their beliefs. If what is being claimed is that Jane and Jane2 represent the world in the same way, then the slide would be motivated; but then the premise cannot be assumed to be true without begging the question against Burge's thought-experiment. If the premise is that the experience of Jane and

Jane2 is qualitatively identical, it is not controversial; but then it is quite unclear how the conclusion about the intentional contents of their thoughts is supposed to follow. The argument does not establish that Jane and Jane2 are similar in their representational rather than in their qualitative or functional aspects.

2.3 A Different Approach to Individualistic Content.

What moral is to be drawn from our examination of these arguments for the Ecumenical View? Both lines of argument discussed above fail because they beg the question against the view of propositional attitudes derived from Burge's thought-experiment. Neither approach produces convincing evidence that common sense practice involves individualistic individuation, and the only reason given for introducing such a practice is that doing so would make the Ecumenical View true. If these defects are to be remedied, the claim that our everyday practice of thought attribution has individualistic elements must be independently motivated.

Recall that the aim of arguing that our common sense practice has individualistic elements was to defend the Continuity Thesis against the challenge presented by the conclusion Burge draws from his thought-experiment. Let us look a little more closely at how the thought-experiment is supposed to challenge the Continuity Thesis. It seems that common sense explanations of behaviour cannot be vindicated by science if science and common sense individuate mental states differently. But this conclusion follows from the thought-

experiment only if the thought-experiment shows that the mental states ascribed in giving common sense explanations of behaviour are nonindividualistically individuated. If the thought-experiment is to undermine the Continuity Thesis, it must justify the claim that explanation in common sense invokes states which are non-individualistically individuated. It has been generally assumed that it does; but we need to ask whether this assumption is warranted.

However, it may be objected that we have already made the assumption that Jane and Jane2 perform different actions in discussing the narrow content theorist's claim that their behaviour is type-identical (Sec. 2.1). If two individualistically identical agents perform different actions in virtue of belonging to different linguistic communities, and different explanations are required for those actions, then common sense action explanations are non-individualistic. This objection is misplaced; the point of the earlier discussion was that we cannot simply stipulate that Jane and Jane2 perform the same intentional actions in the face of Burge's purported demonstration that they do not. But that is not our project here. The appropriate method is not to stipulate, but to investigate exactly when we do and when we do not individuate thoughts and actions non-individualistically. We need to look more closely at our everyday practice of belief ascription to discover whether we individuate thoughts in a manner which is sensitive to linguistic environment when giving common

sense explanations of people's actions.

2.4 B-individuation.

Burge's thought-experiment presents a particular case in which our attribution of belief contents is influenced by the subject's linguistic environment. But how far can this result be generalized? To answer this question, we must look more closely at the aspects of our practice of belief individuation emphasized by the thought-experiment. For convenience, I shall call the mode of content individuation to which Burge draws attention 'B-individuation.' What are the salient features of B-individuation, and when and why do we B-individuate beliefs?

One of the most important features of the thought-experiment is the fact that opaque belief ascriptions containing the word 'arthritis' in the content clause can be truly applied to Jane despite her misapprehension of the term's extension.³ In ascribing beliefs to her we employ the words she has uttered with their customary meaning; we do not reinterpret them to capture her idiosyncratic understanding. She is taken to have a grasp, imperfect though it may be, of the concept of arthritis. Burge acknowledges that there are some circumstances in which we do not accord a subject's words their customary interpretation; the cases he mentions include those in which the speaker is a child, a foreigner, a speaker of a dialect, or the victim of a slip of the tongue. Here the subject either does not have full command of our standards of usage (child, foreigner), is not bound by them (dialect), or has full command but fails to manifest it because of a

performance error (slip of tongue). In each case the subject is excused from being taken at his or her word; it is assumed that the speakers did not say what they meant, or did not mean what they said (except in the dialect case, where the subject did not say what we thought he said). In the case of Jane, by contrast, we accord her words their customary interpretation, despite her misunderstanding, because she accepts that she said and believed something false. The crucial point for the thought-experiment is thus not, as Burge sometimes seems to say, that she is prepared to defer to communal usage by altering her use of 'arthritis'; the foreigner will do that too, yet we do not take his words literally in ascribing him beliefs. It is that Jane is prepared to have her words construed according to their socially accepted meaning, even though this puts her in the wrong.

This partial list of cases in which we take subjects at their word in attributing beliefs shows that there is still much that is unclear about our practice of B-individuation. How, for example, do we decide when someone is a member of our linguistic community? If Jane were prepared to alter her linguistic usage (say, for purely pragmatic reasons) but not to admit that her belief was false, should we still ascribe her the belief that she has arthritis in her thigh? If we were attempting a thorough account of B-individuation, we would need to discover how radical a subject's misunderstanding can be before B-individuation becomes inappropriate, how linguistic

communities are differentiated and what the conditions for membership of them are, and so on. But our aim here is merely to get a rough grasp of what B-individuation consists in, and of when and why we do it, which will allow us to determine whether this is the mode of belief individuation we employ when giving common sense psychological explanations. It seems that B-individuation consists in taking subjects to have the attitudes expressed by their utterances, where these are interpreted according to the standard usage of the linguistic community to which the subject belongs. And though it is far from clear what constitutes the relevant sort of membership in a linguistic community, as we have seen, the sorts of difficulties that arise indicate something about what our purposes are in B-individuating.

When we B-individuate we hold people responsible for the opinions their utterances express, even though they may not fully understand the meanings of the words they utter. The difficulty raised above is that it is not yet clear whether we hold people thus responsible even when they themselves maintain that this involves a misconstrual of their claims. Presumably our practice in such cases often depends on whether we see the speaker's response as a disingenuous attempt to avoid criticism, or as a claim made in good faith. But the fact that this is the sort of consideration that guides us suggests that our interest in B-individuating is in intellectual responsibility; our purpose is to determine which beliefs a speaker is committed to defending in debate. If our interests

in B-individuating have this specific focus, it would not be surprising if we found that we individuate mental states differently when our purpose is to explain a person's actions.

The sections which follow present some examples which illustrate how we individuate beliefs when giving common sense psychological explanations of action. I argued in the previous section that the alternative mode of individuation here illustrated, which I shall call E-individuation, is the one which should command our attention if we are interested in comparing the individuation of explanatory states in common sense discourse and in cognitive psychology. The examples show that when we give common sense explanations of actions we may attribute mental states which differ from those counselled by B-individuation. We shall begin with a case in which we B-individuate one belief, but E-individuate two.

3 Some Cases of Incomplete Understanding.

3.1 The Case of Paul.

Let us consider a thought-experiment inspired by an example from Loar (1987). Suppose that Paul is an English speaker who is misinformed about arthritis in just the same way as Jane; and let us suppose that he says 'Arthritis is spreading from my knees to my thigh.' Given this utterance, the principles of B-individuation dictate that we should describe him as believing that he has arthritis in his knees and in his thigh. He moves to France and learns French, hearing of an ailment called 'arthrite' which, he learns, occurs only in joints. No-one

tells him that 'arthritis' and 'arthrititis' refer to the same disease, and not surprisingly (in view of his original misconception) this is not one of his beliefs. Then a French doctor tells him (in French) that he has arthrtis in his knees, and he believes her; he asserts 'J'ai arthrite aux genoux.' Paul is a competent French speaker who uses 'arthritis' correctly, so the obvious way forus to report the belief he thus asserts is

(2) Paul believes that he has arthritis in his knees. As the principles of B-individuation dictate, we report the belief Paul asserted, interpreting his words according to their standard meaning.

The ascription (2) is thus counselled twice by the practice of B-individuation; we apply it on the basis of Paul's French assertion and on the basis of his English one. But suppose that a French friend tells Paul of a miracle treatment for arthritis, and that Paul applies the treatment to his knees and believes it has succeeded. He will continue to search for a cure for the other disease he believes still afflicts his knees and thigh; if asked whether there is anything wrong with his knees, he will still say that there is. How are we to explain this behaviour? It seems to be intelligible only on the assumption that he has two different beliefs about what is wrong with his knees, even though the unambiguous B-ascription (2) fails to distinguish them. If we B-individuate beliefs, we do not have the resources to explain Paul's behaviour; but surely we can understand perfectly well why he behaves as he

does.

On its most plausible interpretation, the story of Paul shows that in giving common sense explanations of others' actions we may need to individuate beliefs more finely than we do when B-individuating. In explaining Paul's behaviour we invoke two beliefs differing in content, even though B-individuation delivers only one ascription to describe his mental situation. How might a theorist who regards B-individuation as adequate for common sense psychological explanation--a person I shall describe as a Burgean--respond to this example?

3.2 Burgean Responses Considered.

3.2.1 First Response.

A Burgean might simply deny that the univocal B-ascription is inadequate to explain Paul's behaviour. But this is very implausible; it means that Paul's failure to act as we would expect given the belief ascribed to him by (2) can only be explained irrationality or carelessness. We would have to say, for example, that he continues to seek a cure for arthritis because he has forgotten that he had applied the French treatment. Intuitively this is not the cause of his behaviour, and this move locates his problem in entirely the wrong place. His failure to act as expected is due rather to lack of information; he does not know that the disease called 'arthritis' and the disease called 'arthrite' are one and the same. As a result of this, he does not draw the apparently

obvious conclusion that there is only one thing wrong with his knees.

3.2.2 Second Response.

The reply to the preceding objection suggests a different response. The idea would be to maintain that Paul has just one belief about what is wrong with his knees, as reported by (2), and to attribute his unusual behaviour to his failure to recognize that the words 'arthritis' and 'arthrititis' have the same meaning. Paul is ignorant of this metalinguistic truth; but can this fact, in conjunction with the B-ascription (2), suffice to explain his actions? We are assuming that he acts as follows: he applies the French miracle cure for arthritis to his knees, and believes that it has succeeded, yet still continues to search for a cure for the ailment he thinks afflicts his knees and thigh. This train of events would most naturally be explained by saying that Paul begins by believing that he has two ailments in his knees, comes to believe that one has been cured, and desires to find a cure for the one he believes still remains. According to this account, his initial and final mental states differ. According to the metalinguistic Burgean response, however, Paul's assertions at the beginning and end of the story support a single unambiguous B-ascription. How can Paul's metalinguistic ignorance be used to remedy this?

The Burgean's most promising move appears to be to claim that although Paul has a single belief, its content being that he has arthritis in his knees, his metalinguistic ignorance leads him to think that he has two beliefs differing in

content. So he starts out thinking he has two beliefs and ends up thinking he has one. This accommodates our intuition that Paul's initial and final mental states are different; but does it explain why he acts as he does? To explain this the Burgean will have to say something along these lines: that Paul thinks he has a belief he formulates as 'I have arthritis in my knees,' he thinks he has a belief he formulates as 'J'ai arthrite aux genoux', he thinks the two beliefs are different, and he thinks the application of the cure gives him reason to give up one but not the other.

But the Burgean who takes this line is effectively conceding that the B-ascription (2) is inadequate to accounting for Paul's behaviour, and is making all the distinctions among psychological states that we are claiming are required for psychological explanation. The difference between the taxonomy of belief contents offered by B-individuation and the taxonomy required for explaining actions is being recognized; the distinctions are simply being made at the level of second-order beliefs (beliefs about beliefs). But the claim that Paul is acting out of beliefs about what he believes is very implausible. There is no independent reason to suppose that the beliefs Paul acts on are beliefs about what his beliefs are, rather than ordinary object-level beliefs. In short, the proposal is ad hoc.

3.2.3 Third Response.

A Burgean could deny that we would B-individuate beliefs as I

have described, denying in particular that we would report the belief Paul expresses in French using (2). But given the principles of B-individuation described above, this seems implausible. A competent French speaker who says 'J'ai arthrite aux genoux' surely expresses the belief that he or she has arthritis in the knees, and Paul is a competent French speaker who understands the word 'arthrite' perfectly. It would be more plausible to claim that (2) is not a correct report of the belief Paul acquired in England, in view of his misunderstanding of the English word. But to claim this is to abandon the fundamental principle of B-individuation--the principle that speakers' words are to be interpreted according to their standard use, and not according to how they understand them, when giving belief reports.

3.3 Conclusions from the Case of Paul.

The implausibility of these attempts to reinterpret the example of Paul shows that we have here a strong case for the claim that E-individuation--the individuation of thoughts required by common sense psychological explanation--sometimes compels us to discriminate belief contents more finely than does B-individuation. In the next section I shall argue that the converse is also true; that is, that the demands of psychological explanation sometimes lead us to discriminate belief contents more coarsely than they are picked out by B-individuation.

3.4 The Case of Alfred.

The example I shall use is a variant of one employed by Burge

in an earlier paper in which he himself draws attention to two sets of intuitions about how beliefs should be individuated. Burge (1978) introduces Alfred, who misunderstands the word 'fortnight', applying it to periods of ten rather than fourteen days. Alfred also believes that Bertrand will be gone for ten days; we may suppose that Bertrand has told him this, and that Alfred believes him. Alfred now utters the words 'Bertrand will be gone for a fortnight.' It is plausible that Alfred thereby asserts that Bertrand will be gone for a fortnight; he is a competent speaker of English bound by its conventions. But does he believe that Bertrand will be gone for a fortnight?

The principles of B-individuation would of course dictate that Alfred does have this belief, since that is what he asserted. Indeed, Burge reports that many people, 'desiring to maintain a close relation between sincere assertion and belief', hold that the following is true:

- (3) Alfred believes that Bertrand will be gone for a fortnight.

These informants were evidently attributing beliefs according to the principles of B-individuation. However, Burge also notes that there is a widespread intuition that Alfred does not believe that Bertrand will be gone for a fortnight, despite the fact that this is what he asserted. Holders of this view maintain that Alfred holds only the belief given by

- (4) Alfred believes that Bertrand will be gone for ten days and not that given by (3). The attractions of this view are obvious if we are interested in explaining Alfred's actions.

Let us suppose that Alfred has to meet Bertrand's train; we have no hesitation in predicting that he will go to the station in ten rather than fourteen days' time, his reason being that he believes that Bertrand will return in ten days. By contrast, describing Alfred as believing that Bertrand will return in a fortnight does nothing to explain why he goes to the station after ten days have passed. As Burge remarks, 'defenders of this view...tend to mention the fact that Alfred had in mind a period of ten days...Alfred's actions will be largely based on his belief that Bertrand will be gone for ten days. His linguistic mistake is irrelevant for such purposes as meeting the train' (1978:133). Holders of this view were evidently E-individuating, attributing beliefs with an eye to common sense psychological explanation.

As before we must deal with objections from the Burgean, who in this case will want to deny that the demands of common sense psychological explanation lead to the rejection of (3). But if the Burgean adheres to Burge's procedure, which is to attempt to accommodate as much as possible of our ordinary practice, the intuitions of the many speakers who withhold (3) because of its irrelevance to explaining Alfred's actions must be respected. However, the Burgean may attempt to argue that the practice of these speakers is inconsistent. The strategy here would be to take advantage of Burge's claim that both those who affirm and those who reject (3) agree that

(5) Alfred believes that a fortnight is ten days.

If proponents of E-individuation accept (4) and (5), it seems that they can hardly deny (3); for if Alfred believes that Bertrand will be gone for ten days, and that a fortnight is ten days, he is likely to conclude that Bertrand will be gone for a fortnight. Indeed, it seems that we will want to hypothesize just such a train of reasoning to explain why Alfred says 'Bertrand will be gone for a fortnight.' Consistency thus forces proponents of E-individuation to accept (3) if they affirm (5).

The weak point in this argument is the premise that proponents of E-individuation are committed to accepting (5). It is true that one might well explain Alfred's utterance by saying 'He believes that a fortnight is ten days'; but we are equally likely to say something like 'He says 'fortnight' when he means ten days.' In everyday discourse people are generally lax about the distinction between using and mentioning a word, so it is not surprising that they should be indifferent between those formulations which portray Alfred's mistake at the object level (as does (5)) and those which portray it as metalinguistic. Given the laxity of everyday speech, it does not seem that there is any great obstacle to regarding (5) as a loose rendering of

(6) Alfred believes that 'fortnight' means ten days. This analysis affords us a way of accommodating a curious feature of E-individuative practice which is revealed by Alfred's case. Burge notes that 'on the negative [i.e. E-individuative] view, we withhold attribution of belief with

terms...misunderstood by the believer, except in attributions like [(5)] which are the natural means of identifying his mistake' (1978:134). On the present account, the attribution in question should be interpreted as crediting the speaker with a belief about a word (i.e. (6)), not with the concept the word expresses. Alfred's utterance is easily explained by citing (4) and (6); since Alfred believes that Bertrand will be gone for ten days, and that 'fortnight' means ten days, he will think that one way to convey his belief is to say 'Bertrand will be gone for a fortnight.'

Here, though, the Burgean might complain that our metalinguistic rendering of (5) is 'an ad hoc piece of special pleading, undermined by the evidence we actually use for deciding whether a thought was metalinguistic' (Burge 1979:97). The claim here is that when presented with our account, involving (4) and (6), of the reasoning leading to his utterance, Alfred may protest that his reasoning did not fix upon words (ibid.). But given that on any reasonable common sense theory metalinguistic beliefs must surely be implicated in the production of utterances, whether we are aware of them or not, we have an independent reason not to accord much weight to Alfred's response in this case.

The conclusion we have reached, then, is that a consistent explanation of Alfred's verbal and nonverbal actions can be given if we hold (4) and (6) rather than (3) and (5). Furthermore, this is the preferred method of attributing

beliefs to Alfred to explain his actions; so the claim that one can also give a consistent explanation involving (3) and (5) is, though true, not really relevant. The question is how people do individuate attitudes when giving psychological explanations, not how they might consistently do so. The case of Alfred indicates that when explaining actions (i.e. when E-individuating), we may prefer belief ascriptions which capture subjects' idiosyncratic conceptions to ascriptions which credit subjects with the concepts standardly expressed by their words. But we have yet to show that E-individuation is coarser than B-individuation; for that we need a case in which we B-individuate two beliefs but B-individuate one.

3.5 A Thought-Experiment.

Let us imagine a counterfactual case in which Alfred remains exactly as before, insofar as he is nonintentionally described, but where he is member of a linguistic community in which the word-form 'fortnight' is standardly used to refer to a period of ten days. In other words, the counterfactual condition differs only in that correct use is the same as Alfred's actual misuse. Let us call Alfred in this condition 'Alfred2.' Alfred2, like Alfred, is told that Bertrand will be gone ten days, utters the word-forms 'Bertrand will be gone for a fortnight', and goes to the station after ten days have elapsed. We may agree with Burge that Alfred2 did not assert that Bertrand would be gone for a fortnight; since he belongs to a different linguistic community, we cannot interpret his words according to the meanings they would have in our dialect.

But let us concentrate on Alfred2's action of going to the station. What is his reason for doing this? The most natural way for us to explain his action is to say that he wants to meet Bertrand and believes that he can do so by going to the station after ten days; that is, Alfred2 believes that Bertrand will be gone for ten days. Thus we give the same explanation for the actions of Alfred and Alfred2 despite the difference in their linguistic environments.

In the case of Alfred2, attribution of beliefs by content expressed--B-individuation--will lead us to withhold the ascription 'Alfred2 believes that Bertrand will be gone for a fortnight' and will permit 'Alfred2 believes that Bertrand will be gone for ten days.' So in this case, B-individuation and E-individuation will counsel the same belief attributions. But this does not cast doubt on the the reality of E-individuation. There are other cases, as we have seen, where the two individuating schemes yield different ascriptions; and furthermore, we can see why the two schemes, elsewhere divergent, would coincide in the case of Alfred2. They coincide because his usage of the word 'fo.'tnight' matches its standard usage in his linguistic community. In fact, B-individuation and E-individuation will diverge only when agents' unexpected behaviour reveals that their grasp of communal concepts is imperfect, forcing us to differentiate between the actual meaning of the words they use and the meaning they attach to them. In the majority of cases, agents'

use of terms is close enough to standard use for B- and E-ascriptions to coincide.

3.6 E-individuation and Expressibility.

It is one thing to show (as do the cases discussed above) that when we are giving common sense psychological explanations, we may not individuate beliefs according to the non-individualistic scheme we have been calling B-individuation; it is another to show that the individuating scheme we employ when picking out explanatory states can properly be called 'individualistic.' But our discussion of these cases provides good reasons to believe that E-individuation can be so described. When we E-individuate belief contents we are concerned not with the standard use of the words the subject utters, but with the way the subject uses them; we want to know the meaning the individual attaches to them, not the meaning the community gives them. Thus the same E-ascriptions can be true of individuals who use a symbol in the same way, even though they belong to linguistic communities in which that symbol is used differently (cf. Alfred and Alfred²); and where a subject uses two symbols differently, two beliefs will be E-individuated even though the symbols in question may be used in the same way in the two linguistic communities to which the subject belongs (cf. Paul). It is the way the individual uses a symbol, not the way the community uses it, that is important for E-individuation.⁴ Beliefs thus individuated supervene on features of the individual taken in isolation, rather than depending on norms in the communities to which they belong.

When we individuate thoughts individualistically, we may withhold that-clauses containing words the subject uses or understands in a nonstandard way, and instead employ terms whose standard meaning captures the meaning the subject attaches to the misunderstood word. Thus we use 'ten days' but not: 'fortnight' in describing Alfred's beliefs individualistically. It follows that individualistic content is at least sometimes expressible; that is, that we can at least sometimes pick out individualistic beliefs by producing opaque that-clauses which directly display their content, as we do in the cases of Alfred and Alfred². In the case of Paul this is not so easy, as there is no readily available expression in English which captures the notion Paul first acquires (the notion of a rheumatoid disease which can occur in both bones and joints). But we can construct a case which is parallel to Paul's in that we E-individuate two beliefs and B-individuate one, but differs in that the contents of the two E-beliefs are expressible.

Let us suppose that Mary is an English speaker who mislearns the word 'fortnight' in America, applying it to periods of ten days. She then moves to France and learns French, acquiring the word 'quinzaine' which she correctly applies to fortnights. In America she read in an encyclopaedia the sentence 'The period of the third moon of Saturn is a fortnight,' and believed it; now she reads in a French encyclopaedia that the period of the same moon is "une

quinzaine." She is puzzled and wonders which book is right. How are we to explain this sequence of events? It seems obvious that Mary entertains two thoughts about the period of this moon; she believes (at least tentatively) that it lasts a fortnight and she believes that it lasts ten days. She knows that these thoughts cannot both be true, because they differ in content; so she wants to find out which is correct. Thus we E-individuate two attitudes:

(7) Mary wonders whether the period of the third moon of Saturn is ten days,

and

(8) Mary wonders whether the period of the third moon of Saturn is a fortnight.

By contrast, the principles of B-individuation would lead us to hold that (8) alone is true and (7) false. The two sentences Mary reads have the same socially accepted meaning; both sentences state that the period of the third moon of Saturn is a fortnight, and neither concerns periods of ten days. A competent French speaker who reads and speculates on the truth of the statement in the French encyclopaedia would naturally be described as wondering whether the period in question is a fortnight; and when we are B-individuating Mary's beliefs, we use ascriptions with 'fortnight' in the content clause despite her misunderstanding of the word. But (8) alone does not provide a satisfactory explanation of Mary's puzzlement; if she acquired the same thought from each encyclopaedia, why does she wonder which one is right?

The case of Mary differs from that of Paul in that there

is a readily available expression (namely, 'ten days') which captures Mary's idiosyncratic understanding of the word she misuses, while there is no such readily available expression to characterize Paul's misconception. But what does 'readily available' mean here? It is extremely unlikely that a rational, competent speaker of English (as we are imagining Mary to be) would not be familiar with the words 'ten' and 'days'; and it is equally unlikely, given the frequency with which they are used, that she would misapply or misunderstand them. Our willingness to use 'ten days' to express the contents of her E-beliefs surely owes much to the fact that the expression is almost certainly one she uses, and one she uses correctly. Moreover, we have good reason to believe that it accurately captures the notion Mary has in mind, since she applies the word 'fortnight' exclusively to periods of ten days. By contrast, it is quite possible that Paul would not be familiar with expressions which are candidates for capturing his misconception; he may not know, for instance, what 'rheumatoid condition' means. We are also less confident, in his case, that our candidate expressions accurately capture his concept.

What does the comparison of the cases of Mary and Paul tell us about the constraints on the expressibility of E-individuated or individualistic contents? It is important to note that the cases illuminate only a restricted aspect of our practice; in each case we are concerned with the expressibility

of E-beliefs in English, and part of the evidence (in addition to nonverbal behaviour) for the attributions comes from English sentences read or uttered by the subject. The cases can thus only tell us about the expressibility in a language L of beliefs acquired from statements in L or attributed (at least partly) on the basis of utterances in L. The constraint seems to be that an E-belief is expressible in L under the conditions stated just in case the expressions of L used in the content clause of the E-ascription capture the subject's concept and are likely to be used and understood by the subject.

This rule is rough and imprecise, but if it provides at least a partial description of our ascriptive practice, it shows that there are limits to the expressibility of individualistic thought contents. But the fact that we can sometimes express the contents of such thoughts shows that it is not in principle impossible to capture a person's individualistic notions, even though it may on occasion be difficult to determine which of several related notions a person employs, or to know which expressions are available (in the sense discussed above) for characterizing them. The point to be stressed is that the contents of individualistic thoughts do not seem to be essentially inexpressible, as some have maintained. Whether or not they are expressible depends on accidental facts, such as the expressive resources of the ascriber's language and the likelihood of finding a paraphrase couched in terms with which the subject will be familiar. Since the expressibility of individualistic contents does

depend on extraneous considerations, the fact that sometimes the contents of subjects' individualistic thoughts are inexpressible is surely not good grounds for denying that they have such thoughts.

4 Explanatory States in Common Sense and Cognitive Psychology.

4.1 Conclusions.

Let us sum up the conclusions reached thus far. I have argued that the cases of Paul, Alfred and Mary support four claims.

These are:

(a) When we are giving common sense psychological explanations in terms of mental states with propositional content, our individuating practice may differ from the non-individualistic practice exemplified by Burge's thought-experiment.

(b) The mode of thought individuation we there employ can be called individualistic, in that it varies not with communal norms but with individual use.

(c) Where individual and communally accepted use coincide, the two modes of individuation pick out the same thoughts.

(d) The contents of individualistically individuated thoughts are sometimes expressible, and their expressibility depends on extraneous (i.e. non-psychological) features of the subject's situation.

The question we must now ask is: what are the implications of these results for the issues discussed in the Introduction? Recall that one of our motivations for examining content individuation in common sense contexts was to assess the status

of the Continuity Thesis. This is the view that the explanatory states invoked by cognitive psychology share important properties of the attitudes attributed in explanations in common sense. Burge's claim that propositional attitudes are individuated with reference to linguistic environment, taken together with the widespread view that the explanatory states of cognitive psychology are not individuated in this way, appeared to cast doubt on the Thesis. But it is the individuation of explanatory states that is relevant to the truth of the Continuity Thesis, and, as claims (a) and (b) show, these are individuated individualistically. The examples discussed in this paper indicate that Burge's thought-experiment is not representative of how we attribute thoughts when giving common sense psychological explanations. The challenge supposedly presented to the Continuity Thesis by Burge's thought-experiment is rebuffed.

4.2 The Ecumenical View.

What, then, of the two reactions to Burge's challenge, the Schismatic View and the Ecumenical View? Proponents of the first of these hold that cognitive psychology individuates psychological states individualistically while common sense discourse attributes non-individualistic mental states. Holders of the Ecumenical View maintain that rather than being purely non-individualistic, as Burge implies, content individuation in common sense discourse also has individualistic elements. Insofar as adoption of the Schismatic View is motivated by Burge's claim that propositional attitudes

are non-individualistically individuated, that motivation is removed by (a) and (b), which show that such individuation is not universal in common sense.

The Ecumenical View seems to provide a more accurate depiction of common sense practice. The picture we glean from the examples is that our common sense practice of attitude ascription is complex; we individuate thoughts in one way when explaining others' actions, in another when identifying the opinions they are committed to defending. In the context of explanation we attempt to capture as closely as possible the meaning individuals attach to the terms they use, even though, as (d) indicates, this this may be difficult to express. In the context of debate, we hold them to the socially accepted meaning of their words, even though they themselves may misunderstand the words they use. Of course, as claim (c) above indicates, the two modes of individuation may (and perhaps frequently do) converge on the same ascription. In this case the attitude which is causally explanatory of action and the attitude which is the object of intellectual criticism have the same content.

Footnotes.

[1] The content of a mental state or event is given by the that-clause of the ascription used to attribute the state or event to a thinker. For our purposes we may, like Burge, remain neutral as to precisely what contents are; our interest is in patterns of propositional attitude attribution.

[2] The relevant passage is this: 'if we leave the communal environment...out of consideration, with a view to focusing on the topic of intra-individual psychology, then our picture will contain nothing at all that is recognizable as a subject of mental states. That casts suspicion on the label 'intra-individual psychology'; for there will be no obvious reason in that case to accept that the findings of such a discipline would have any constitutive relevance to the mind' (Pettit & McDowell 1986:14). Since cognitive psychology does ignore the communal environment in individuating the content of psychological states, it can presumably only offer an 'intra-individual psychology' without relevance to the mental. The aim here is to challenge the claim that common sense psychological explanations require vindication by scientific psychology.

[3] Strictly speaking, it is not the belief ascription itself which is opaque or transparent, but the belief construction as it occurs in a particular context. For convenience, I will speak of a belief ascription's being 'opaque' when terms in the that-clause are not open to substitution by coreferential expressions *salva veritate*.

[4] I should stress that these remarks are not intended to imply that meaning reduces to use, or that use is responsible for meaning. The claim is that the cases discussed earlier indicate that when individuating beliefs with an eye to action explanation, we treat subjects' use of words as important indicators of the content of their beliefs. It is one thing to say that use provides evidence for content, another to say that use determines content.

[5] The notion of expressibility at work here, common though it is in current literature, is not easy to capture briefly. We must distinguish between expressing a thought content and describing it. As a rule, a singular term of the form 'X's belief that p' describes X's belief rather than expressing it if replacing 'that' by 'which' produces a meaningful expression. Thus 'Paul's belief that [which] he would express in English with 'Arthritis is painful'' picks out a belief by describing it, while 'Paul's belief that [which*] snow is white' picks out a belief by expressing it.

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Individualism and Semantic Development.

1. Introduction.

In the last few years Tyler Burge has published a series of papers in which he argues that the content of a person's intentional mental states (such as thoughts, beliefs, and desires) is partially determined by features of that person's physical and social environment (Burge 1979, 1982a, 1982b, 1986b, 1986c). In a recent paper (Burge 1986a) he advances the same claim about the content of the representational states attributed in scientific psychology. The aim of this paper, like that of his earlier work, is to argue against the view he calls 'individualism.' This is the view that, as Burge puts it, 'the mental natures of all of a person's...mental states (or events) are such that there is no necessary or deep individuating relation between the individual's being in states of those kinds and the individual's physical or social environments' (1986a:4). Burge claims that 'individualism is prima facie wrong about psychology, including cognitive psychology' (1986a:9). In his view, there is a "necessary or deep" individuating relation between the intentional states attributed in psychology and the environment of the person to whom they are ascribed. His thought-experiment illustrates the individuating relation which he believes to hold between mental states and social environment; he interprets it as showing that 'propositional attitudes...depend for their content on social factors that are independent of the individual...For if the social environment had been

appropriately different, the content of those attitudes would have been different' (1979:85).

According to this view, a person's being in a certain psychological state (a psychological state with a certain content) necessarily involves her occupying a certain kind of social environment; had she been the same in all non-intentional respects, but occupied an environment that was appropriately different, she would not have been in that psychological state.

Ostensibly Burge's target is "pan-individualism", the view that all of psychology is individualistic (i.e. attributes mental states which are individualistically individuated). He writes that 'in questioning the view that psychology is individualistic I am not thereby doubting whether there are some subparts of psychology that conform to the strictures of individualism' (1986a:10). Despite this guarded formulation, however, the burden of Burge's paper is that no part of psychology in which content is ascribed to psychological states is individualistic (1986a:9). He makes it clear that those subparts of psychology which may be individualistic are those which employ a "formalistic" level of description intermediate between the attitudinal and the physiological (1986a:22-3). In his view, where the level of description is intentional, psychological states are individuated with respect to the subject's environment.

The claim of this paper is that Burge's "pan-anti-individualist" view of content in psychology is wrong. 1 do

not intend to take issue with Burge's interpretation of Marr's theory of vision, in which he argues that the content of the perceptual states attributed in the theory depends on the perceiver's physical environment. My concern will be with Burge's claim that there is a necessary or deep individuating relation between psychological states and social environment. I shall describe a psychological research project in which the attribution of intentional states plays a central role, and present a thought-experiment which shows that the theory attributes intentional states which are not individuated with respect to social environment. Contrary to the pan-anti-individualist view, content individuation in this part of cognitive psychology is predominantly individualistic.

Burge holds that 'individualism as applied to psychology must be revisionistic' (1986a:9). He writes:

'I have not tried to argue for non-individualistic psychological theories from a standpoint outside of psychology. The heart of my case is the observation that psychological theories, taken literally, are not purely individualistic, that there are no strong reasons for taking them non-literally, and that we currently have no superior standpoint for judging how psychology ought to be done than that of seeing how it is done' (1986:24).

I agree that the appropriate method for judging whether psychology is individualistic is to look at how it is done. The heart of my case is the observation that when we do look at how psychologists explain semantic development, we find that their approach is individualistic. There is no need to resort to revision, supplementation, or special pleading to support the individualistic individuation of psychological states; it

already occurs in an important part of psychology.

2. Semantic Development.

2.1 Over- and Under-Extensions.

There are three aspects of psychological research on semantic development which make it a suitable area in which to assess Burge's claims about content individuation in psychology. The first is that modelling the acquisition of semantic knowledge clearly involves attributing states with representational or intentional properties. It is, after all, the development of an intentional state that is being studied. Since the level of description of psychological states is intentional, Burge cannot dismiss evidence of individualistic individuation in semantic development on the grounds that the theory describes states at a "formalistic" or sub-intentional level.

The second notable aspect of work on semantic development is that it has a feature which Burge regards as indicative of non-individualistic psychological theories; it is 'not success-neutral,' as Burge puts it (1986a:30). What he means by this is that the theory 'attribute[s] states that are subject to practical...evaluation by reference to standards partly set by a wider environment' (1986a:25). Theories of semantic development attempt to explain how children succeed in learning the meanings words have in the surrounding linguistic community; on Burge's analysis they aim to explain the success of the process of semantic acquisition, where the standards of success are set by the linguistic environment. If linguistic

environment plays a role in determining the content of mental representations, we would expect that role to be evident in the study of the development of children's knowledge of word meanings. Not surprisingly, Burge himself predicts that thought-experiments purporting to show that mental contents are individuated with reference to linguistic environment 'would be more relevant to social and developmental psychology, to concept learning, and to parts of "higher" cognitive psychology' (1986a:26; emphasis added).

The third point which makes semantic development an apt testing ground for Burge's anti-individualist views is that the literature on semantic acquisition contains many examples of the phenomena of over- and under-extension, examples which directly parallel the cases of misuse exploited in Burge's thought-experiments. Children at various stages of development frequently apply a word to a category of objects which is either more inclusive or more restricted than the category to which the word is standardly applied. If the word is used too restrictedly, the child is said to underextend it; if the use is too inclusive, the word is overextended. There is an obvious similarity to the cases Burge uses to generate his thought-experiments--cases in which speakers overextend 'arthritis' or 'sofa,' or underextend 'contract' or 'brisket'. Considering how developmental psychologists treat this phenomenon provides a test of whether or not they attribute concepts with reference to linguistic practices in the child's community, as Burge's interpretation of his thought-experiment

(and his pan-anti-individualism) would predict.

The theoretical treatment of semantic development is not yet as highly developed as the theory of vision which Burge describes in 'Individualism and Psychology.' Current hypotheses are still at the stage of modelling the changes which occur in children's representations of word meanings as their knowledge develops. As yet there is no consensus on the course of these changes, and there are no detailed proposals about the computational mechanisms responsible for them. But the competing models of development are set within the same theoretical framework--a framework which, I shall argue, motivates an individualistic treatment of under- and over-extensions. In the next section I will sketch the shared theoretical aims and assumptions of current models of semantic development.

2.2 Models of Semantic Development.

Current models of the development of knowledge of word meanings are set within the framework of a representational theory of mind: a child's attaching meaning *M* to some word *w* is conceived of as the child's having an entry for *w* in his or her mental lexicon which represents the meaning as *M*. The aim of a theory of semantic development is to explain how the child constructs such lexical entries for words on the basis of her mental representations of the linguistic and nonlinguistic contexts in which she hears them (Carey 1982:347).

To achieve this aim the theorist must describe the general

form that lexical entries take, the sequence of changes they go through during development, and the mechanisms responsible for their undergoing that sequence. As mentioned above, current models are directed mainly to the first two tasks. These models conceive of representations of word meanings as being composed of semantic features which specify criteria of application for the word in question. Classical models, such as Clark's Semantic Features Hypothesis, assume that the list of features composing a lexical entry provide necessary and sufficient conditions of application; an object must have all and only the features specified by the list if the word is to be correctly applied to it. Recent Prototype models (such as Bowerman's 1978, 1980) assume that the object need only have some subset of the features listed for the word to be applicable. There may be no one feature which it is necessary that all referents share, though the features may be weighted so that possession of some features--those exhibited by the "best exemplar" or prototypical referent--may be more important than possession of others (Bowerman, 1978:280).

Within each category of models, there are disagreements over whether the features of which early lexical entries are composed are predominantly perceptual or functional, and over whether the features composing early lexical entries are the same as those found in the adult's lexicon. There are also differences over whether lexical entries develop from the general to the specific (as in Clark's hypothesis) or from the specific to the general (as in Bowerman's hypothesis).

According to the "general to specific" view, the child's early lexical entry for a word typically consists of a subset of the features which compose the adult entry for the same word.

Early words will therefore tend to be overextended, since their lexical entries will underspecify the category of objects to which the word applies. Clark gives the following example:

'suppose that the child has learned the word dog; however, he only uses one feature to characterize the meaning of this word, so the set of objects that he will put into the category named dog will be larger than the set in the adult category. For instance, he might have characterized the word dog as meaning four-legged; the sets of objects referred to as dog, therefore, might include cows, sheep, zebras, llamas, [and] dogs...'
(1973:193).

On this view, the dominant process in semantic development is that of adding more specific features to lexical entries, which has the effect of narrowing the categories of objects to which words are applied.

According to the "specific to general" view, early words are initially associated with a detailed representation (typically constructed from features) of an object to which the child has heard the word applied. Early words will therefore tend to be underextended, since they will be applied only to objects which match the specified referent. Bloom (1973:72) describes an example of such usage; her daughter used 'car' only for moving cars seen from her window, not applying the word to stationary cars, cars that she was in, or pictures of cars. On this view, semantic development involves a process of generalization; the child gradually abstracts some features

from the detailed representation and reorganizes the lexical entry to give these greater weight in the application of the word (Bowerman 1978:281). Words which were initially underextended will later be correctly extended or overextended.

This brief sketch of current approaches to modelling semantic development is oversimplified and incomplete. But it does illustrate the crucial importance of evidence of over- and under-extension in choosing between different models. All the models view lexical entries as actively constructed by the child out of semantic features, and discovering the ways in which children over- and under-extend words can provide vital insight into the process of construction.

The experimental investigation of Clark's Semantic Features Hypothesis provides an example of the importance of over- and under-extensions as evidence and of the methods used to test for their presence. Clark originally advanced her hypothesis on the basis of evidence of widespread overextensions in young children's spontaneous utterances and in their comprehension of speech (1979:199-221). She proposed that

'when the child first begins to use identifiable words...he has only partial entries for them in his lexicon, such that these partial entries correspond in some way to some of the features or components of meaning that would be present in the entries for the same words in the adult's lexicon...The principal difference between child and adult categories at this stage will be that the child's are generally larger since he will use only one or two features criterially instead of a whole combination of features.' (Clark 1973:193).

Clark, then, adopts a Classical view of concepts and a "general

to specific" view of semantic development. She predicts both that children will overextend words at first because they will initially construct incomplete lexical entries, and that pairs of words which are not synonyms will often be synonymous for the child. This comes about because the partial meaning the child constructs for word A may correspond to the complete meaning constructed for word B. For instance, children commonly overextend 'brother,' taking it to be synonymous with 'boy'; the meaning attached to both words is the same, namely [+ Male] [- Adult] (Clark 1973:218-20).

Clark also makes specific claims about the features which compose the adult meanings of words, and the order in which children acquire those features. She proposes that the features which comprise the adult representations of word meanings are hierarchically ordered from the general to the specific, and predicts that children acquire the more general features first (1973:196). For example, she proposes that the meaning of 'big' is represented as [Adj] [comparative] [+ Dimension (3)] [+ Polar], where the features are presented in order of increasing specificity. The meaning of 'tall' is given by the same feature list, with the addition of the more specific feature [+ Vertical], which indicates the dimension of comparison. If children acquire general features before specific ones, they will first acquire the features which the two words have in common; only later will they acquire the feature which shows that 'big' and 'tall' are not synonymous

(1973:212).

As this example suggests, the domain of comparative spatial adjectives provides a good testing ground for the Semantic Features Hypothesis. Clark drew on evidence that children go through a stage at which they overextend 'tall' and 'short,' understanding them as meaning the same as 'big' and 'little' respectively. Evidence offered to support this claim includes the fact that children offer 'little' as the opposite of 'tall,' and indicate either the shortest or the thinnest object in an array when asked to choose the shortest one. Clark hypothesized that the child's lexical entries for 'tall' and 'big' were the same, both consisting of the features [+ Dimension (3)] and [+ Polar], as predicted by her theory; the entry for 'tall' did not yet specify the dimension of comparison as being vertical (Clark 1973:212).

However, Carey (1978) showed that when individual children's pattern of responses was studied across different tasks, evidence for a synonymy between 'tall' and 'big' was lacking. Children did make errors suggesting that they did not yet understand 'tall' correctly, but the pattern of errors made by each child indicated that the lexical entry for the word was always more complex than just [+ Dimension (3)] [+ Pole] (Carey 1978:285-6). Carey suggests that the child's early representations of dimensional adjectives are restricted to particular uses, in that they contain information about particular objects to which the word is applicable (1978:286). She agrees with Clark that children's lexical entries for these

words do not contain the feature which specifies the dimension of comparison to which the adjective applies (e.g. [vertical] for 'tall' and 'high,' [horizontal] for 'wide' and 'fat'); but she proposes that they contain information about which variation of which kinds of objects the adjective applies to. Thus a child might know that 'tall' used of people 'picks out bigness along the axis from head to toe' and yet not know how to apply the word to other kinds of objects (Carey 1982:371).

This series of results illustrates two points about how representations of word meanings are attributed in the study of semantic development. The first point is that children are described as attaching idiosyncratic meanings to words, meanings which differ from those standardly attached to the word in question. As the experiments outlined above testify, discovering individual children's lexical entries for particular words can provide a crucial test for hypotheses about the principles underlying the construction of word meanings.

The second point is that psychologists specify lexical entries in terms of the semantic features out of which the child is believed to have constructed them. They are not specified in terms of how they deviate from the meanings the words in question have in the surrounding community.

3. A Thought-Experiment.

With this brief sketch of the developmental psychologist's methods and assumptions in mind, let us consider a case

analogous to Burge's thought-experiment. Suppose that Amy is a four year-old child brought up in an English-speaking community, and suppose further that she participates in a study of spatial adjectives. In the course of the study she is asked questions to map her understanding of various words. The experimental protocol is designed to probe for lexical entries containing information about the dimensions of particular objects, of the type hypothesized by Carey (1978).

Amy, like many other children of her age, uses the word 'tall' slightly differently from adults. Records of her spontaneous utterances show that she applies the word only to tall people, not to other tall objects that she sees around her. When shown a picture of a tall man or of a short man and asked, 'Is this a tall man?' she answers correctly. She also chooses the tallest woman from a pictured array differing only in height when asked, 'Is one taller than the others, or are they all the same in tallness?'. She is not distracted by variation in other dimensions when the stimuli are pictures of people. Confronted with a pictured series of men of equal height but differing girth, she says that they are all the same in tallness; and she responds correctly when asked to pick the tallest person from a group of people differing in both height and fatness. But when shown pictures of tall buildings and tall trees and asked 'Is this a tall building?' or 'Is this a tall tree?' she answers, 'No, a big one' (cf. Carey 1978:288).

What are we to say about the meaning Amy attaches to 'tall'? If we describe the situation in the way that Burge

describes his original thought-experiment, the question at issue is whether Amy's mastery of 'tall' is close enough to standard use for her to be attributed a grasp, imperfect though it is, of the communally established concept of tallness (or, as I shall put it, the concept tall). Indeed, psychologists sometimes seem to be concerned with this question, saying that tests like those given to Amy are a way of investigating the child's concept of tall, and thereby apparently implying that the child has this concept. However, what is usually claimed is that the child's concept of tall differs from the adult's concept, which is hardly consistent with the view that talk about the child's concept of tall is to be interpreted as showing that the child is being attributed the adult or communal concept. What seems to be meant by saying that these studies reveal the child's concept of tall is that they reveal the concept the child attaches to the word 'tall,' a concept which is frequently different from that which the adult attaches to the word (which is assumed to be tall).

As we have seen, psychologists attribute idiosyncratic concepts to children on the basis of their production and comprehension behaviour, concepts which are specified in terms of lists of features. The conclusion that would be drawn from Amy's responses would be that she represents the meaning of 'tall' as [Adj] [comparative] [+ polar] [_ person, head to toe] (as at Carey 1975:286). Her production and comprehension data show that she underextends the word, and the underextension is

explained by attributing to her a lexical entry more specific than the adult's.

Now consider the case of Amy2, a little girl who is Amy's double but lives in a linguistic community in which the standard use of the word-form 'tall' is slightly different from its use in English. 'Tall' in her community is correctly used only for the comparative height of people. We may suppose that this is the only difference between our language and the dialect spoken in Amy2's community. This difference in usage does not, however, impinge directly on Amy2; Amy and Amy2 hear exactly the same sentences, uttered in indistinguishable contexts, up to the time at which their comprehension of spatial adjectives is tested. At this time Amy2 is brought into the laboratory of a psychologist in our linguistic community, alongside Amy, and is given exactly the same tests. She of course responds in exactly the same way as Amy, insofar as her responses are nonintentionally described; and like Amy, she spontaneously applies the word 'tall' only to tall people. But Amy2's production and comprehension behaviour reflects an appropriate use of the word 'tall,' relative to the norms of her home community.

How would Burge predict that the psychologist should attribute concepts to Amy and Amy2? (We must, of course, assume that our psychologist knows of the difference in the dialects spoken in the girls' home communities.) On Burge's view, the two girls should be attributed different concepts, since they belong to different linguistic communities; Amy

should be attributed an imperfect grasp of the concept tall, while Amy2 is held to have a better grasp of a different concept, that expressed by the word 'tall' in her community's dialect.

But this is surely not how our psychologist will proceed. We have already seen that Amy's use and comprehension of 'tall' will be explained by attributing to her the lexical entry [Adj] [comparative] [+ polar] [__ person, head to toe]. Since psychologists attribute idiosyncratic word meanings to children, and do so by specifying lists of features, Amy2 will be credited with exactly the same combination of features as her lexical entry for 'tall,' despite the difference in usage in her linguistic community.

It might be objected that the psychologist who ignores the difference in the two girls' linguistic communities is neglecting important information.¹ Information about the linguistic environments in which children have learned words is of course useful and important; but it is useful and important because it usually gives information about the ways in which the child has heard the word applied. This does not mean that it determines what lexical entry should be attributed to a child, but that it is useful to know the input contributing to the child's construction of the lexical entry attributed to her. Thus even though the psychologist knows of the difference in the dialect spoken in Amy's and Amy2's home communities, he or she is not thereby obliged to attribute different lexical

entries to them.

Furthermore, psychologists from both communities will agree on this description of the two girls' representations of the meaning of 'tall' (assuming, of course, that developmental psychology is practised in the same way in Amy2's community). The difference in the meaning of 'tall' in the children's linguistic environments does not result in their being attributed different representations of the meaning of the word, as the anti-individualist predicts.

In the following sections I consider ways in which a proponent of pan-anti-individualism in psychology--a person I will call the Burgean--might object to the individualistic interpretation of the thought-experiment.

4 Burgean Objections Considered.

4.1 First Objection.

One way in which the Burgean might respond is to argue that content ascription is inappropriate in the case of Amy, on the grounds that her linguistic behaviour is not systematic enough to form the basis of an intentional attribution. According to this view, the case of Amy should be assimilated to that of a foreigner who hears and repeats English words without understanding them. The claim is that the child, like the foreigner, lacks the competence in using the word 'tall' which is required for us to take her utterances of it seriously as expressions of thoughts, or to credit her with any comprehension of sentences containing it. If this is true, her case cannot legitimately be presented as illuminating content

individuation.

There are two possible ways in which the Burgean might argue for this view. The first strategy is to appeal to features of the specific example to support the claim that Amy should not be attributed the concept tall. The Burgean who adopts this strategy will have to appeal to an intuition that Amy's behaviour with respect to the word 'tall' is too unsystematic for content attribution to be appropriate. But intuitions concerning this case surely accord with the psychologists' practice; it seems reasonable to argue about what Amy means by 'tall' but not about whether she means anything by it.

The second strategy is to argue from a more general claim about the propriety of attributing concepts to children on the basis of their linguistic behaviour. The claim would be that rather than attributing concepts to young children individualistically on the basis of their linguistic behaviour, psychologists should not attribute any concepts on the basis of such evidence. The argument for this claim would be that young children like Amy have too poor a grasp of language in general to warrant attributing to them one lexical entry rather than another on the basis of their utterances. Of course supporters of this view must provide some alternative (and convincing) explanation of the behaviour which psychologists explain in terms of the use of semantic knowledge. Presumably children's utterances will be explained as resulting largely from their

desire to imitate the sounds of adult speech, not of their attaching some meaning (idiosyncratic or otherwise) to them which they desire to convey. Children's reactions to verbal instructions or questions will be viewed as determined predominantly by non-verbal cues or by some predilection for particular kinds of response (or both).

But these explanations are very implausible. We may suppose that Amy, like many other four-year-old children, is capable of initiating conversations by spontaneously asking apparently meaningful questions, and of making what appear to be cogent inferences from the answers she receives. Moreover, children of this age are capable of conversing in this way on what appear to be quite abstract topics (see e.g. the studies cited in Carey 1985:26-28 which recount children's questions about what happens after death). Such apparently intelligent behaviour is far more plausibly explained as the result of children's attaching meanings to words than as the result of imitation and responses to non-verbal cues. Rather than Amy's general linguistic competence being so poor as to cast suspicion on her apparently systematic use of 'tall', her behaviour supports the view that the word has meaning for her.

In addition to their individual weaknesses, these two lines of argument are problematic for a common reason; they flout the practice of psychologists. They are therefore unacceptable to a Burgean who accepts Burge's methodological point about how claims about individuation are to be assessed. That point is expressed as follows:

'What we know about individuation is derived from reflecting on explanations and descriptions of ongoing cognitive practices. Individuative methods are bound up with the explanatory and descriptive needs of such practices' (1986:18).

Given that the practice of developmental psychologists is to attribute semantic knowledge to children like Amy, the Burgean who claims that this practice is misguided and should be discounted assumes a substantial burden of proof. That burden becomes even heavier when we consider that developmental psychologists explicitly distinguish cases like that of Amy, in which idiosyncratic usage indicates that the child attaches a different concept to a word from the adult, from cases in which children's responses are taken to indicate that they do not attach any concept to the word. In Carey's replication of a classic study of the word 'alive,' for instance, some four-year-olds could not give a definition of the word and sometimes could not produce any examples of living things. When asked whether various pictured objects (animate and inanimate) were alive, they gave random judgements and inconsistent or irrelevant justifications (Carey 1985:17ff). Such children were judged not to have mapped the word onto a concept.² The situation with Amy is quite different. She uses and responds to 'tall' perfectly systematically; she simply does not do so in the manner standard in our linguistic community. She is therefore interpreted as attaching to it a concept different from that which it is standardly used to express.

It is part of developmental psychologists' practice to

distinguish between cases in which children's behaviour does and does not support the attribution of intentional states, and Amy falls on the "content" side of the line thus drawn. The Burgean who holds that she falls on the "no content" side must therefore argue that the line as psychologists draw it is wrongly drawn. Doubtless psychologists' practice is not sacrosanct; there may be reasons to revise it. But as Burge notes, the philosopher who proposes to revise scientific practice must provide good reasons for the revision (1986:10). The two arguments outlined above do not provide such reasons.

4.2 Second Objection.

An initially more promising Burgean objection would be that although Amy is competent enough as a language user (her usage is systematic enough) to support content attribution, it is to be expected that the contents attributed would be individualistically individuated. The argument here would be that children like Amy are not full members of the linguistic community and are not responsible to its norms of usage, and so we would expect that their utterances would be reinterpreted when they are ascribed beliefs or concepts. But this response is of use only to the Burgean who is prepared to modify the view that content individuation in cognitive psychology is non-individualistic. The modified view would be that cognitive psychology individuates individualistically in situations in which common sense does so (e.g. when the subject is not responsible to the linguistic norms of the community), and otherwise individuates content non-individualistically. The

claim that content is individuated individualistically in developmental psychology is to be expected, on this view, if children's utterances are reinterpreted when reporting their beliefs in ordinary discourse.

One obvious problem with this response is that it constitutes not a defence but a rejection of pan-anti-individualism. If developmental psychology individuates intentional states individualistically, as this response concedes, then an important part of the anti-individualistic case collapses.

In addition, the claim of the modified view that children are not full members of the linguistic community is problematic. Burge often suggests that membership is established by patterns of deference to other's usage (e.g. 1979:101), and children surely defer to corrections by adults. It is therefore not clear that reinterpretation of children's utterances is to be expected on Burge's view of common sense practice. The Burgean might try to use this point to challenge the psychological treatment of the thought-experiment, arguing that psychologists should not reinterpret children's utterances precisely because children are members of the linguistic community and should be taken at their word. But this, of course, begs the question; why should a successful scientific practice be revised to accord with a philosophical reconstruction of nonscientific practice? To echo a remark Burge makes in another context, the argument rests on a sketchy

and unclear conception of linguistic community which is unsupported by scientific practice (cf. Burge 1986a:23).

4.3 Third Objection.

This objection is suggested by Burge's treatment of Marr's theory of vision. In that account Burge argued for the appropriateness of non-individualistic content individuation not from the perceiving subject's deference to the linguistic community, but from the assumption that the subject's perception of the surrounding physical environment is veridical. Presumably Burge would offer the claim that the theory of vision assumes that visual perception is veridical in support of his view that the "presuppositions" of psychology are not (purely) individualistic. If perception is veridical, a certain relation holds between visual representations and the environmental objects they represent; and the fact that psychology is concerned with the relations between perceiver and environment 'helps motivate non-individualistic principles of individuation,' in Burge's view (1986a:12). Essentially Burge argues that since the theory assumes that people successfully perceive what is in their environment, it will attribute different visual perceptions to physical duplicates who inhabit different environments.

The Burgean might base an argument for non-individualistic individuation in the case of semantic development on an analogous claim about the successfulness of the process of learning word meanings. The claim would be that the aim of a theory of semantic acquisition is to explain how children

succeed in learning the meanings words have in their linguistic environment. The theory thus presupposes that children do succeed in learning those meanings. The analogous success-based argument for the linguistic case would run as follows.

The theory assumes that children succeed in constructing accurate representations of the meanings words have in their linguistic environment, just as the theory of vision assumes that people succeed in constructing accurate representations of the objects in their physical environment. The aim of each theory is to explain this success. In his discussion of Marr, Burge presents what he calls a 'natural corollary' of this assumption of success, namely that the content of tokens of a representational type R constructed in perception is individuated in terms of the 'distal causal antecedents' that such tokens are about. The analogous corollary for the semantic case is presumably that the content of a lexical entry L constructed in semantic acquisition is individuated in terms of the meaning of the word, uses of which lead to the construction of L.

According to this view, psychologists should specify Amy's lexical entry for 'tall' in terms of the communal or adult concept the word expresses, accommodating her underextension of the word by saying that she incompletely grasps or understands the concept.³ Describing the meaning she attaches to the word in terms of the adult concept is presumably specifying it by how it deviates from that concept. Thus children who over- or

under-extend words should be attributed the adult concept plus or minus some features. But this is not how lexical entries are usually specified. As indicated by the earlier discussion of tests of Clark's hypothesis, lexical entries are specified in terms of the features the child has extracted; and some of these may be different from the features which compose the adult entry for the same word. As Carey's model illustrates, the relationship between the meanings which children and adults represent may not be as straightforward as the Burgean proposal assumes. Until the child has abstracted that 'tall' picks out size along the vertical dimension, she may apply the word on the basis of features quite different from those listed in the adult lexical entry.

The Burgean might argue that the child's lexical entry can still be specified with reference to the adult's, even if the former contains features that the latter does not, if the child's representation is described as the adult's with some features added and others subtracted. But this route can only be followed if the features of which the child's lexical entry is composed are known, in which case psychologists would simply specify the child's feature list directly. (It also presupposes that the adult's feature list is known, which may not be the case.)

Let us consider a more promising success-based argument for non-individualistic individuation in developmental psychology. Suppose that, as the Burgean claims, theorists of semantic acquisition are interested in how word meanings are

successfully learned, and that the standards for success are set by the linguistic community to which the child belongs. Suppose further that there is some slack in what counts as success in learning, in that in each community there is a range of slightly different uses of a term within which one must fall to be considered to have successfully learned it. Given these assumptions, the Burgean may generate a thought-experiment as follows.

Consider two linguistic communities C1 and C2. Members of C1 consider their children to have successfully learned the meaning M1 which a certain word-form W1 has in their dialect iff their usage of W1 falls within a range of uses R1. Members of C2 require that a child's usage of a certain word-form W2 fall within a range R2 if he or she is to be considered to have learned the meaning M2 of W2. As it happens, word-forms W1 and W2 are identical; and there is a child in C1, A, who uses W1 in just the same way that B, a child in C2, uses W2. In fact A and B have heard the word-form in exactly similar circumstances and are for our purposes individualistically identical. Furthermore, A's use of W1 falls within R1 and B's use of W2 falls within R2. If success in learning is evaluated with respect to the standards of the local community, A will be considered to have mastered meaning M1 and B to have mastered meaning M2. But then mental contents are non-individualistically individuated, for A and B use an identical word-form in exactly the same way, and yet are attributed

mastery of different meanings.

This interpretation of the thought-experiment may truly represent an aspect of the common sense treatment of word learning, but it does not accord with psychological practice. As the earlier discussion of work on semantic development showed, psychologists usually attribute children representations of idiosyncratic meanings, not communally established meanings; and these representations are specified in terms of the list of features which the child is believed to have constructed. In view of the last point, child A's lexical entry for W1 and child B's lexical entry for W2 will probably be specified in terms of features, not in terms of the meanings M1 and M2 which are current in the children's respective communities. Since psychologists attempt to discover which features the child's lexical entry is composed of by examining spontaneous utterances and responses on comprehension tasks, and the two children's responses on such tasks are identical, they will most likely be ascribed the same idiosyncratic understanding of word-forms W1 and W2.

It is true that theorists of semantic acquisition assume that most children do eventually construct lexical entries which represent the socially accepted meaning of words, and that they are interested in how such children succeed in doing this. But although psychologists assume that the learning mechanisms responsible for the child's construction and revision of lexical entries are adequate to the task of learning the meanings words have in the child's linguistic

community, this does not commit them to describing the lexical entries constructed during the learning process in terms of communally accepted meanings.

4.4 Fourth Objection.

The Burgean might object that the experimental results described in Section 3 do not warrant the conclusions about children's representations of meanings which the psychologist would draw from them. The objection would be that there are explanations which do not involve the assumption that children attach idiosyncratic meanings to words, but account equally well for children's over- and under-extensions. The general point I want to make in response, which I shall attempt to substantiate for each specific alternative, is that psychologists are aware of such alternative explanations, and attempt to determine experimentally whether they are appropriate in a particular case. If their results are not consistent with the alternatives, they are committed to giving explanations in terms of idiosyncratic meanings. Merely invoking the possibility of alternative explanations is therefore not sufficient to cast suspicion on explanations in terms of idiosyncratic lexical entries.

For instance, a child who applies the word 'dog' to, say, a sheep may know the correct meaning of the word, and therefore know that her use of it is incorrect; but she may use it anyway simply because she wants to communicate and does not yet know the correct name for a sheep. Perhaps children attach the

correct meanings to words, but overextend them because their vocabularies are limited.

This explanation of overextensions was proposed by Bloom (1973) and Clark (1983). As a result psychologists have recognized that it is important to test for whether a child's overextension of some word in production reflects the meaning the child assigns to the word, or the child's communication strategy. If the misuse is due to vocabulary limitations alone, the child's responses on comprehension tasks involving the word will be correct. If the word is also overextended in comprehension, it is probable that the child attaches a different meaning to it. Some overextensions are found in both comprehension and production (e.g. Anglin 1977).

It has also been suggested that children's spontaneous overextensions, like slips of the tongue, are the result of errors in accessing correct lexical entries. For example, Huttenlocher claims that it is more difficult for children to retrieve the correct name when confronted with an object than it is for them to retrieve information about the correct referent of a word they hear (1974:366). Because of this, she argues, the child may produce the wrong word for an object even though his or her lexical entry for that word is correct. This proposal can also be tested by comparing production and comprehension, to see whether the overextension still occurs when the word is provided as a cue.

A third alternative explanation is that children who over- or under-extend words may attach the correct meaning to them,

but misapply them because they have difficulty in determining whether the conditions of application for the word are satisfied. For example, a child may know that square A is correctly described as bigger than square B if and only if the area of square A is larger than that of square B; but if the child computes the areas of the squares inaccurately, he or she may still make the wrong choice when asked to pick the biggest square. The point is a familiar Davidsonian one: the misapplication of words may be due not to idiosyncratic word meanings and correct beliefs about objects, but to correct word meanings coupled with idiosyncratic beliefs about objects.

This explanation for misuses of words is mentioned by Carey (1982:36-7), who notes that variation in the difficulty of tasks may explain apparently inconsistent responses to different questions involving the same word. Psychologists may have a methodological preference for attributing children's incorrect responses on comprehension tasks to idiosyncratic word meanings, whereas in the case of adults the preference may be for attributing idiosyncratic beliefs (though the explanation favoured probably depends on the nature of the tasks and responses as well as on the age of the respondent). But again the alternative explanation is open to confirmation or disconfirmation by the results of independent tests for the hypothesized idiosyncratic beliefs. In the case of the example given above, a child's difficulty in computing or comparing areas should be revealed by discrimination tasks involving

different verbal instructions.

The point which emerges from the examination of explanations of under- and over-extensions which do not involve attributing idiosyncratic lexical entries is that these alternatives have been anticipated by experimenters and are open to experimental test. Over- and under-extensions which survive these tests are considered to be best explained as the result of the child's attaching an idiosyncratic meaning to the over- or under-extended word.

4.5 Fifth Objection.

In the presentation of the thought-experiment in Section 3, it was noted that psychologists sometimes say that tests like those given to Amy and Amy2 provide information about the child's concept tall. Since psychologists also say that responses such as Amy's and Amy2's indicate that the child's concept tall differs from the adult's, it seems problematic to interpret psychologists as attributing the adult (i.e. communal) concept of tallness to the child. Therefore in the discussion in Section 3 it was suggested that when psychologists make remarks such as these, they are actually speaking elliptically about the concept the child has attached to the word 'tall.' (Indeed, this is all that comprehension tests ⁴ can reveal.)

The Burgean may object to this metalinguistic interpretation of the psychologists' remarks, insisting that their use of a word used deviantly by the child in describing the child's concepts shows that they individuate the child's

mental representations in terms of communal concepts. With particular reference to the thought-experiment, the Burgean might argue that psychologists would explain Amy's behaviour by saying that she attaches the concept of tallness of people to 'tall,' or that she thinks of tallness as a property only of people.

Psychologists might well describe Amy's situation in this way. But they will also describe the concept Amy attaches to 'tall' as [Adj] [comparative] [+ pole] [_ person, head to toe], and this suggests that remarks such as those mentioned above do not have the deep significance which the Burgean attaches to them. Consider this quotation from Carey's defence of her proposal about children's lexical entries for comparative spatial adjectives:

'Suppose the child first learns 'deep' and 'shallow' as applying to ends of pools. If he can use the words correctly faced with novel swimming pools, not confusing depth with length or width of the pool, then certainly he has the concept of depth of swimming pools. But he may not see the similarity between the way that the deep end of a swimming pool is deep and the way that bowls, holes, and puddles are deep. He may not know that 'deep' can apply where there is no contrast between two parts of a single object, or that it does not require a liquid medium. Each of these, plus many other irrelevant features, may be part of his unanalyzed conception of the depth of pools' (1978:287).

The purpose of this passage is to point out that although the child may be said to have the concept of depth of swimming pools (or tallness of people), this does not mean that the child possesses the adult or communal concept of depth (or tallness). The child has not yet developed the feature system

in terms of which the adult concepts of depth and tallness are represented. Thus the concept represented by the child's feature list is only roughly conveyed by words which express the adult concepts the child has not yet acquired.

The primary importance of the feature list as a description of the concept the child attaches to a word is illustrated by the following observation about the thought-experiment described in Section 3. Psychologists from our and Amy2's speech communities may both use the word 'tall' in describing the semantic competence of the child from their community; but of course the word will have a different meaning in the mouths of theorists from the two communities. The Burgean wants to use this difference to argue that a difference in linguistic environment makes for a difference in the children's mental contents. But psychologists will agree that both children's lexical entries for 'tall' consist of the features [Adj] [comparative] [+ pole] [_ person, head to toe]. All that is needed to defeat the Burgean is a case in which psychologists ascribe the same concept (i.e. same semantic features) to two children on the basis of the children's use of a word w, even though the children come from linguistic communities in which w has a different meaning.

At this point the Burgean may point out that psychologists interested in conceptual development attribute to children concepts which the children have not yet mapped onto words. For instance, Carey writes that

'although young children map a different concept onto

the word "animal" than do adults, there is no doubt that a concept animal with the same extension as the adult's plays an important role in their thought...Nonetheless animal functions in the thought of 10-year-olds and adults differently than it functions in the thought of young children' (1985:183).

These remarks about the child's concept animal obviously cannot be interpreted metalinguistically, as remarks about the concept the child has mapped onto the word 'animal' (as we interpreted remarks about the child's concept tall); Carey's point is precisely that the child's concept animal has not yet been mapped onto 'animal.' It seems that in this case the child is attributed the adult or communally established concept, even though the child's conception of animals is different from the adult's ('young children do not realize that all animals eat, breathe, and reproduce'; Carey 1985:183).

However, even if we grant that the Burgean may be able to generate a thought-experiment on the basis of cases such as these, it will not suffice to reinstate pan-anti-individualism; that view requires that children's concepts are always described in terms of those current in the child's community. Moreover, such cases hardly suffice to show that there is a 'necessary or deep individuating relation' between the contents of thinkers' mental states and their social environments, as Burge maintains (1986a:4). It is just as plausible that children are described as incompletely grasping the communal concept (as opposed to being credited with an idiosyncratic concept) when the features composing the child's concept are not yet known. If this is so, the non-individualistic mode of

description is adopted largely for convenience, and may be abandoned if the underlying features are discovered.

5 Conclusion.

According to the Burgean "pan-anti-individualist" view of the content of psychological states, the intentional states attributed in psychology are individuated with respect to subjects' linguistic environments. This means that a person's being in some intentional psychological state necessarily involves her occupying a particular sort of linguistic environment; a subject who was physically and functionally identical to her, but occupied a linguistic environment that was different in some relevant respect, would be in a different intentional state. Amy and Amy2 are physical and functional twins who belong to linguistic communities which are relevantly different; the fact that the word-form 'tall' has a different meaning in the two societies should, on the Burgean view, result in their being considered to attach different concepts to the word. Yet, as we have seen, this difference in linguistic environment does not require developmental psychologists to judge that Amy and Amy2 represent different meanings for the word-form 'tall.' In fact, our examination of current models of semantic development suggests that the two girls would be attributed the same lexical entry for the word 'tall.' The case of Amy and Amy2 thus provides a counterexample to the pan-anti-individualist thesis; it shows that there is at least one area of cognitive psychology which is not purely non-individualistic.

If Burge is correct in claiming that Marr's theory of vision is non-individualistic, what differences between this theory and the theory of semantic development might account for the difference in content individuation in the two fields? One difference is that according to Burge's analysis, Marr's work implicitly assumes a causal or covariance theory of content. This forms the basis of Burge's argument that the theory is committed to attributing different representational states to individualistically identical subjects occupying environments in which the normal causes of perceptual states are different. Regardless of whether this analysis of content individuation in Marr's theory is right, it cannot be extended to cases in which it is the role of linguistic or social environment in determining content which is at issue. A causal theory of content is no help here, for the communally established standards of linguistic usage are not the normal causes of the psychological states, the content of which they are supposed to partially determine. It is not linguistic norms which cause children to construct lexical entries, but the adult utterances to which they are exposed.

However, there is a parallel between the theory of vision and the theory of semantic development, a parallel to which Burge attaches great importance. As mentioned earlier, both theories are "success-oriented" (or at least not "success-neutral," in Burge's phrase). Burge bases a general argument for the non-individualistic nature of psychology on the claim

that psychological theories of our various cognitive activities presuppose that we are successful in our interactions with our environment (1986a:44). He argues that this motivates psychologists

'to frame explanations that account for these successes, and correlative failures, in such a way as to illumine as specifically as possible the mechanisms that underlie and make true our evaluations [sc. of the activities as successful]' (1986:25).

It is true that psychologists assume that children eventually succeed in discovering the meanings words have in their surrounding communities, and that the aim of research in semantic acquisition is to discover the mechanisms which enable them to do this. But what exactly does this assumption of success come to? Applied to the case of semantic development, Burge's interpretation ascribes to developmental psychologists the strong assumption that children eventually converge on the socially established meanings of words. But it is just as plausible that theorists of semantic development, like theorists of the acquisition of syntax, make the weaker assumption that each child eventually acquires a knowledge of language which permits him or her to communicate successfully with others in the community (cf. Chomsky 1986:16). Successful communication need only require that each child's lexical entry for a word is similar to the lexical entries of other members of the community.

Even if we suppose that developmental psychologists adopt the strong version of the assumption of success, it is hard to see why this would commit them to individuating representations

of word meanings non-individualistically. Specifying the early idiosyncratic lexical entries the child constructs in terms of features provides no obstacle to the project of explaining how a child eventually acquires the correct meaning of a word. Moreover, specifying the child's meanings simply in terms of how they deviate from the typical adult's actually might be an obstacle to discovering the principles guiding the child's construction of lexical entries. If, rather than knowing which elements of a word's meaning the child has acquired, we know only which elements she lacks, we will have less to guide us in determining the mechanisms responsible for child's acquiring those elements she has acquired.

I conclude that Burge's pan-anti-individualist view of content individuation in psychology is false. This does not mean that psychological states are never individuated non-individualistically. But even if lexical entries are sometimes attributed with reference to linguistic environment, the fact that they usually are not shows that there is not the deep or necessary relation between psychological states and linguistic environment that Burge believes there to be.

Notes.

[1] In fact this is not strong enough for the anti-individualist; information about a child's linguistic environment is not merely important but essential to specifying the child's lexical entries correctly. If linguistic environment is a partial determinant of the content of psychological states, a developmental psychologist who is ignorant of the linguistic affiliation of one of his or her subjects cannot reliably specify that child's representations of word meanings. If psychology is non-individualistic, a psychologist who attributed the same lexical entry to Amy and Amy2 would be making a mistake.

[2] The attribution of idiosyncratic meanings is also seen in this study. More advanced children interpreted 'alive' as meaning 'capable of activity or movement,' and therefore overextended the word, applying it to inanimate objects such as clocks 'because they go tick-tock' (Carey 1985:30).

[3] Here, as elsewhere, I assume for simplicity that the correct meaning and the adult meaning coincide for the word in question.

[4] This is not always remembered. According to Carey (1985:18), Piaget attempted to chart the development of the concept alive in children by probing their understanding of the word 'alive.' But, as she points out, 'the word "alive" may not actually be mapped onto the child's concept that most closely approximates the adult's concept living thing' (1985:19). I suspect that studies of children's comprehension of a word 'X' are only described by psychologists as revealing the child's concept X when children are found to attach a concept to 'X' which is similar to that which adults attach to it.

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