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January 2011

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Recommended Citation

Mackay, Nigel and Petocz, Agnes: Essays in realism: analysis and discussion 2011, 52-118.
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Keywords

discussion, essays, analysis, realism

Disciplines

Arts and Humanities | Life Sciences | Medicine and Health Sciences | Social and Behavioral Sciences

Publication Details

Mackay, N. & Petocz, A. (2011). Essays in realism: analysis and discussion. In N. Mackay & A. Petocz (Eds.), *Realism and psychology: Collected essays* (pp. 52-118). Leiden: Brill.

CHAPTER TWO

ESSAYS IN REALISM: ANALYSIS AND DISCUSSION

Nigel Mackay and Agnes Petocz

Part 1 of the book, THE NATURE AND CONTEXT OF REALISM, consists of three chapters. In Chapter 1 (the previous chapter), *Realism and the State of Theory in Psychology*, we discuss the problems and challenges of the state of theory in psychology, and then set out the elements of the realist approach that informs the essays in this collection, briefly considering the development of realism, and its general import for psychology. In Chapter 2 (the current chapter), *Essays in Realism: Analysis and Discussion*, we provide an introduction to each of the essays, commenting on their relevance and their role within the collection. In Chapter 3, *Anderson's Development of (Situational) Realism and its Bearing on Psychology Today*, Fiona Hibberd presents a more detailed discussion of the history and development of this realism.

Hibberd's essay starts briefly with the background to and influences on Anderson's work, his bringing together of various realist strands in earlier thought into a systematic and thoroughgoing realist philosophy, and some important contrasts with his contemporaries. She continues and elaborates the same realist themes that we have introduced in Chapter 1, though in a different way and in places in more detail: the primacy of ontology; the idea of the situation or propositional nature of reality; the logic of relations; determinism and the idea of a causal field. In addition, she blends into this discussion other related matters that we have mentioned only briefly or not at all: Anderson on the categories, or categorial fea-

tures of situations; the general nature of science and critical enquiry; Anderson's mode of argument; and some similarities to and differences from other major thinkers, e.g., Hume, Kant, Alexander, Wittgenstein, Russell, Dewey and pragmatism.

Hibberd's chapter is introductory in the sense that it covers the basics of situational realism, their philosophical provenance and development in Anderson's work, and it sets out the ideas that are preparatory to the psychological essays that make up most of the rest of this collection. Yet it is not introductory in the sense of being a "realism for beginners". It deals with a number of difficult philosophical notions, and it may be sufficiently detailed and novel to make even informed psychological readers feel dropped in the deep end of something that is not their specialisation. We suggest, however, that it is valuable to read early, if not to grasp all the detail, at least to glean a general picture of the bases and context of realism and to get a sense of the intellectual provenance of the later essays. Then, perhaps after reading the other essays, a return to Hibberd will add further substance and context to the other authors' attempts to come to grips with the various conceptual problems of psychology.

Part 2: FROM PHILOSOPHY TO PSYCHOLOGY (Chapters 4-7) illustrates a historical transition phase. It is concerned with the way the principles of *philosophical* realism, as articulated in Anderson's work, have developed into a *psychological* realism as articulated in the works of Maze and others, a realism that makes explicit its importance to psychology.

There are many places where Anderson discusses the realist principles that have been introduced and set out in Part 1 of this book. But his 1927 essay *The Knower and the Known* (Chapter 4) both represents a distillation of realism and, if it is not easily accessible to psychologists, it is directly relevant to their interests: It deals with cognition, a theme at the heart of modern psychology, and with the

underlying doctrines that lead to misunderstandings about the nature of cognition.

In effect Anderson sets out the “cognitive situation” that we introduced in Chapter 1—albeit in epistemological, philosophical terms, rather than psychological, making clear the ontological grounds on which the knower (the cognising subject) has to be distinguished from the known (the object of knowledge), and the relation between them. Although written primarily for philosophers, Anderson’s paper is characteristically clear and direct in style, albeit compact with meaning. It shows the link that realism makes between on the one hand ontological and logical matters, and on the other epistemological and psychological matters. Showing the failure of psychologists to see this link is central to realist critiques of psychology.

Anderson makes two main points: The first is the very general one that a relation, being between two or more terms, is distinct from and not part of those terms: Entities are not constituted by the relations into which they enter. This point is a logical or ontological¹ one. Its importance for psychology can hardly be overstated, because the second point is that cognition (like much of what psychologists are interested in) is a relation, specifically a relation between the knower and the situation known. Anderson also argues that knowledge is propositional in that it is of situations, and situations are necessarily complex, things being of such and such a character, or of matters being related in some way. One cannot know “a thing”, *simpliciter*, the very least that one may know is that a thing is under certain conditions, a situation.

While today’s psychologists would think the targets of Anderson’s critique, Berkeley or Descartes, are quaint and removed from contemporary psychology, as later essays show, modern psychologists

¹ For the realist these are identical in that logic is about the general forms of the world and not a calculus that sets out the rules of reasoning.

commit the same conceptual errors. There is something of a continuity of error from Descartes, through the 17th century empiricists' "way of ideas" to modern psychology. As we commented earlier, it is routine, for example, to treat cognitions as things rather than relations between things, and to follow the language and practice of folk psychology of treating beliefs as things we *have* and that are *in* our minds—the common internalist view of mind. Indeed the main activity of orthodox and cognitive psychology consists of populating the mind with cognitive entities: postulating them, measuring them, proposing hypotheses about their structure, sketching their supposed role in causing behaviour, and even searching for their location in the brain. But these cognitive entities—as opposed to the neural states that enable the person to enter into cognitive relations, or the neural states that obtain when the person is standing in those cognitive relations—will not be found. Consequently, the real relation, that of *being conscious of something* is abstracted to become the ineffable and inexplicable phenomenon of *consciousness*. Indeed, elsewhere Anderson has an essay called *The Non Existence of Consciousness* (1929). Modern psychology's ubiquitous mental representations, like Berkeley's ideas, are constituted by their very role as objects of knowledge, yet simultaneously treated as if they are (mental) entities with dimensions that would make them fit, in proper scientific fashion, as causal structures. More generally, in the functionalist philosophy of cognitive science, hypothesised mental processes and entities are individuated and defined by the functions that they perform, that is, the relations into which they enter, following the same pattern of conflating relations and entities that Anderson identifies in Berkeley and Descartes.

In this early paper, Anderson introduces two further themes that are of especial relevance to psychology, because they deal with the relation between cognition and motivation. The first theme is the idea that the mind is not a unity. The phenomena of mental conflict and self-knowledge require, according to Anderson, a *plurality* of

knowers². This raises the difficult question (which Anderson here notes but defers) of how we come to use the term “I” and of the illusion of a single, unified self or mind. The second, related, theme is the relevance of the Freudian concepts of the unconscious and the wish. The knower, the subject term of the cognitive relation, is a *desirer*, pursuing states of affairs in the environment that will *satisfy* processes in his or her mind. In other words, there is no such thing as purely *disinterested* knowing - all cognition is motivated. These two themes reappear in several of the later essays, which discuss their implications for psychological theory and research.

In Chapter 5, *The Concept of Attitude*, John Maze shows the failure of psychology (still behaviourist at least in its terminology in 1973 when the paper was first published) to account for motivational factors in action. He introduces a theme common in realist works, that a nonteleological account of motives is required to explain action, and he goes on to argue a point that is particularly prominent in his writings: that Freud’s drive theory has the best claim to a coherent analysis of motive. The pattern of this argument, which is given in different forms and with different emphases in several of the papers in this collection, takes up the same logical point that Anderson discusses in his essay, the distinction between things (terms) and the relations between them, and the fact that a thing cannot be *constituted* by its relations. Maze points out that the psychological notion of attitude as commonly construed involves an evaluative claim, namely, that whatever the attitude is directed towards possesses some good or value inherent to it. That is, there is something about the object such that one *ought* to feel this or that towards it. However, goods are always goods-for-someone, and indicate a real or supposed relation between the person and the object concerned. They are not properties of the object. Thus, the widely used concept of attitude involves *pseudocognitive* statements (“X is good/bad/right/wrong”, etc.), confuses relations with properties

² Boag discusses this topic in Chapter 20.

and, impossibly, attempts to define a mental state in terms of the relations into which it may enter.

This paper also provides a good example of the affinity between realist analyses of psychological concepts and psychoanalysis, albeit not the popular view of psychoanalysis, but a psychoanalysis purged of Freud's lapses into teleology. Maze asks, in effect, what is it that presses us to believe that some good (aesthetic or moral) is in the object, given that the logic of relations means it cannot be. His answer is disguised motives, and he offers a Freudian account of these: Not only is psychoanalysis the strongest candidate in the field to account for unconscious motives, but Freud (though by no means all of his followers) clearly distinguishes between drives as motives and those objects which are empirically, contingently, found to bring about states of drive-satisfaction. In short, Freud's theory, consistent with the principle of nonconstitutive relations, distinguishes between the biological source of drive and the objects with which it enters into relation. In contrast to this, social and personality psychology is replete with notions like attitude, set up on conceptually flawed and unexamined bases, and then treated as though in good scientific order they are fit to be treated as the causes of behaviour. The decades-long debate on the (causal) relation of attitudes to behaviour largely misses the point that the concept itself is logically incoherent, a reification of a relation.

The question of a deterministic theory of motivation is addressed in Chapter 6, the second of Maze's essays in this collection, *Drives and Consummatory Actions*, which is the penultimate section of Maze's 1983 book *The Meaning of Behaviour*. Although now more than twenty-five years old, this book is the most comprehensive realist attempt to deal with the ills and prospects of psychology. In the book Maze is concerned with "the basic psychological question 'Why did this person do that?' " (p. iii, quotation marks in original). And in the earlier sections of the book he has argued that psychologists that are teleological, that posit internal agents or the inherent

purposiveness of mind, cannot answer this question: They fail on grounds of incoherence and question-begging, depending as they do on the logically flawed view that the aspect of mind (purpose, choice, agent, self, etc.) claimed to bring about action has its aims as part of it, that is, is defined by its aims and so is constituted by the relations into which it enters. While the cognitive science that has, at least in theory, replaced such teleology has the virtue of attempting to treat cognition as part of an objective and determinist science, its dependence on representational epistemology is similarly self-defeating. Cognition, on pain of incoherence, cannot be understood as the internal representation of the world, but is rather a relation between the organism and external states of affairs. Nor on its own can cognition account for action. For in order to account for action, one must go beyond the instrumental aspects of behaviour, perception, skills, development, analysis of abilities and so on, with which psychology concerns itself almost exclusively,³ and answer the question "but what are these *for*?". And to answer that question, one must combine an account recognising that cognition is a relation, with a proper, causal account of motive, one stripped of teleology. This will tell us how the desire component in the formulation *desire-plus-belief leads to action* stands as a causal variable in the production of behaviour.

Maze then sketches a causal account of motivation modelled on Freud's theory of drives, albeit divested of some of Freud's later speculative ideas on instinct, and compatible with modern physiol-

³ Thus Johnson-Laird (1988 p. 27) says "The mind's main tasks are:

- to perceive the world
- to learn, to remember and control actions
- to cogitate and to create new ideas
- to control communication with others
- to create the experience of feelings, intentions and-self awareness."

But there is no mention of what these might be *for*, nor of the role of psychology in explaining *why* these instrumental processes occur.

ogy.⁴ Centres in the brain are hardwired and, switched on by preset stimuli, they drive specific, pre-programmed behaviours. Where those behaviours, through feedback, bring about the appropriate physiological consequences—also preset—the drive centre is switched off, and driven behaviour ceases. Learning is the accumulation of information by these centres about what behavioural variations on the specific actions, and what objects, bring about the cessation of the drive excitation and the circumstances in which they do it.

The exact physiology or the number of drive centres is an empirical matter and does not affect the general form of Maze's account. There are some deceptively important consequences of this formulation. (1) It removes teleological accounts of purpose⁵ and the representationism that is nowadays substituted for it, both incoherent. Drive is not defined by any aim: It seeks nothing, but in pure causal fashion sets in motion a specific action which, in the right circumstances, brings about the termination of the drive state through feedback, as say the action of sucking and swallowing may through changes in blood sugar levels. (2) It provides an account of the distinction between primary and derivative goals, terminating (in principle) the question "why did he do that?" with the statement of the primary drive action, in a way that agentive formulations cannot. (3) It is a central state materialism in which mental processes are relations into which brain processes enter. This undercuts the whole presumption of contemporary psychology, namely that mental processes are things that exist *in* the mind (or brain) and which can be specified and measured by psychologists. Cognitions or in-

⁴ In the 25 years since Maze's book, research on neuro-psychoanalysis has made the general form of Freud's physiology even more plausible. Though of course the particular physiology is not necessary to the conceptual viability of the drive concept.

⁵ Freud's notion of drive is similarly nonteleological—despite his sometimes loose way of talking about it, and despite the routine interpretation by psychoanalysts of drive.

formation are secondary to drive in the sense that “each instinctual-drive accumulates information and misinformation about the location and means of acquisition of the objects necessary for its specific actions to be performed” (p.162). (4) This is a pluralistic, strongly partitive view of mind, which provides a basis for understanding phenomena such as the distinction between being conscious of events and being self-conscious (conscious of one’s consciousness), and repression. Moreover, it follows the point mentioned in Anderson’s essay that cognitive processes are always motivated, never fully disinterested and rational, an echo perhaps of Hume’s (1739) Treatise, “Reason is ... the slave of the passions, and can never pretend to any other office than to serve and obey them” (Bk. II, Part III, Sect. 3).

First-time readers sometimes miss the depth and complexity of the analysis in *Maze*, perhaps because of his elegantly condensed style of writing. In fact we might note that the ability to summarise complex arguments and ideas in psychology, and pick the eyes out of it, or a body of literature, is something that *Maze* and at least some other realist authors possess. This may be in part a consequence of the mettle developed in having to articulate and defend the minority position of realism against established opinion, but it is also a consequence of practising the realist principle that logical testing of a theory must have priority over empirical or experimental test. The clarity and directness of this realism is mirrored in the ability to disinter the essential points and arguments (including the inconsistencies) of those positions under analysis. And *Maze*, like some of his fellow realists, is prepared to say that the emperor, however grand, is naked, and that his suit of new ideas covers nothing.

In Chapter 7, as his title, *Maze’s Direct Realism and the Character of Cognition*, suggests, Joel Michell reviews major themes in *Maze*’s work, concentrating on cognition and its central place in psychology, and in doing so he fleshes out some realist themes and their

implications for psychology. Michell also provides, as he generally does in his work, the historical threads that led to the positions he discusses. His review is a reminder, still true today, that much of realism's promise has yet to be realised. Not only have psychologists generally not tried to build theories consistent with realist requirements, the realist requirements themselves have not been fully set out. Indeed, Michell's paper reads like a programme for realist psychology, and several of his ideas have been taken up in later work, including in some of the later papers in this collection. Realist analysis, as discussed earlier, is not being touted as a "new paradigm"—the core at least of realism is immanent in rational discourse—but no one should underestimate just how deeply the critiques cut, nor how profound a reconfiguration of psychology's programme is entailed. Michell's review is then a reminder, as he says, of how radical realism is, for example in rejecting the ubiquitous information-processing view on grounds of incoherence. We have almost to begin again to develop accounts of cognition and motivation that are not shot through with the confusions of representationism.

Given that cognition is, as Anderson puts it, a relation between a knower and a known, the issue for the realist is what is the nature of the subjects, the objects (the terms of the relation), and the relation of cognition. These are the elements of the cognitive situation that we introduced in Chapter 1. Michell nicely comments that "Maze takes the subject matter of psychology to be the study of how cognition shapes behaviour in the service of the instinctual drives."⁶ He reviews Maze's critique of representationist theories of cognition, namely that cognitive representations are logically incoherent. The issue of representation can be articulated as that of how do cognitive representations get their meaning, because representationism requires that when a person knows something in the world this is a

⁶ Illustrations of this may be seen in the several essays by Maze in this collection.

two part process; the person knows/accesses the representation, and the representation refers to or represents the state of affairs. The representationist story is appealing because often persons think that they know something in the world and it turns out that they are mistaken. In such cases it is taken that the person-representation part of knowing has occurred normally, but the representation-world part has gone awry. It is the latter that makes the cognition nonveridical. The fatal flaw in this model is that the representation-world part of cognising, where it is held that a representation *refers* to the state of affairs, while it appears to draw on the ordinary sense of reference, in fact does not. When a symbol properly refers to something (the example given is “red” referring to the appropriately coloured things), one must know the symbol, the symbolised entity, and the fact that the symbol is used to refer to the symbolised thing.⁷ This is how “red” gets its meaning. In the arrangement that representationism depicts, the person does not, in fact *cannot*, know both the representation and the thing it refers to for that was exactly what the theory was designed to answer in the first place, yet that is also just what would be required for representation. The theory is circular.

A central point made by Michell is that the support which representationists get from the computer analogy also fails. Michell goes beyond Maze’s view that computers do not really cognise. He argues that if computers represent they do so in a way different from how cognitive representations are supposed to represent. The computer’s internal states may be attributed meaning, of course, because they can be interpreted by a *programmer or user* as words, moves in games and many other things. But this would in any case be a form of standard, *extrinsic* reference, for that is the only kind of reference possible: Something stands for or refers to something else by virtue of their being two independent things where one is *taken*

⁷ See Agnes Petocz’s essay on a realist account of symbolism (Chapter 16, this volume) for further discussion of symbolism as a three-term relation.

to indicate the other, by a third party. Thus, computer states do not have intrinsic meaning or content, as cognitive representations are supposed to have. That is, they do not have specific and unique content, refer only to one thing, what they are about, or mean. Indeed, the idea of *intrinsic* meaning or reference, where the content is within the referring term of the reference is another example of the fallacy of constitutive relations and is simply incoherent. It collapses a relation into a term. Therefore, the computer may well be used to model the neural processes underlying cognition, but it fails as an *existence proof* of cognitive representation; in fact, it is an existence proof that cognitive representations cannot possibly involve intrinsic reference.

Michell also deals with Maze's argument for the plurality of knowers in the one person; his thesis that the knowers are drives, physiologically characterised; that the objects of cognition are propositional, in the sense that they are objective situations, always a matter of something being a certain kind or something being related in a particular way to other things. He also treats Maze's thesis concerning the observability of cognition. Since to observe a relation one must observe both the terms that are related, realists must reject both the traditional view that only one's own mental states are observable and the internalist view that cognition consists of observing some inner state like a representation—a view that ultimately makes knowledge of other minds impossible. Instead the realist must develop an account of what it is to observe another's entering into a cognitive relation where both the terms of the relation, the subject and object of the cognition, are observed. Michell then expands on Maze's view that we can in many situations observe another's beliefs in the causal texture of his or her movements in relation to the environment. That is, other minds are not locked behind the screen of our representations of them, for the phenomena "in" other minds are in fact relations between the organism and objective states of affairs, relations which may in principle be observed.

Though Michell does not discuss it here (but cf. Michell, in press), the view that one can observe directly the cognitions of others is in opposition to another widespread thesis in recent psychology, one that arrives together with the constructivist epistemology of information processing. This is the idea that human social interaction depends on the possession of a *theory of mind*, that is, on the construction of a set of explanatory hypotheses about others' beliefs and desires and their role in the production of behaviour, and that such a theory is required because all knowledge of other minds is necessarily indirect.

Michell also goes on to make some suggestions about the thorny problem of error (see also Rantzen, Chapter 10 in this collection). Much of the justification of constructivist, indirect, accounts of knowledge, and so the representationism of contemporary psychology, comes from the fact of error. From Descartes to cognitive behavioural therapy it is held that mistaken perception or false belief can only be explained as a failure of the constructed, inner representation to match the external thing it represents. While this popular account is fatally flawed (the indirect theory of knowledge, by disallowing access to the object, allows no means to compare it to its representation) the realist still needs to say what error consists of: If we perceive directly, what is being perceived when we misperceive, given that a realist cannot hold that it is an actual situation? Michell proposes that error consists not of the cognition of some state of affairs but of the failure to cognise, an inhibition—perhaps motivated—of cognitions, or an ignoring of facts, and that this makes it appear as if a person is perceiving a non-existent state of affairs

Michell's review is valuable because it shows just how radical are realism's implications for psychology in the hands of a critic like Maze. One by one the assumptions of orthodox psychology are exposed and examined for their coherence, and found wanting: the internalist view of mind; the representationist, indirect view of

cognition; the view of other minds as necessarily non-observable; and the overly cognitive (motivationless) accounts of behaviour.

Part 3, CRITIQUES AND DEVELOPMENTS, consists of a series of papers that give realist analyses of a whole range of psychological concepts and theories. The first four papers, Chapters 8 to 11, deal with the nature of concepts, the argument for the computational model of mind, the problem of error, and the idea of class or category. These may appear, at first blush, to be specialist matters and the general psychologist may not be familiar with the complex arguments that produce the stance that orthodox psychology takes on them. Nevertheless, they have consequences throughout psychology. For example, any psychology that talks of mental concepts rests on a view of what concepts are and how they are attained. Any account of cognition must deal with illusion, truth and error. Any account or use of classification makes assumptions about the nature of class and category. Chapter 12 enters the methodological heart of psychology, addressing the question of the quantitative nature of psychological variables, and it too has profound implications for psychology's assumptions and measurement practices. The remaining chapters, 13 to 23, move into other areas: accounts of meaning and knowledge (Chapters 15, 17, 18, 19), the requirements for a theory of symbolism and symbolic activity (16), the problem of mental causation (22, 23), the unity of mind (20), drives and affects (21, 23), clinical and applied psychology (15, 19), method and the role of qualitative approaches (18, 19), and critiques of social constructionism and post structuralism (13, 14, 15, 17). Together, these essays reveal the systematic and general nature of the realist programme.

Terry McMullen's "*Out There*", *Not "In Here": A Realist Account of Concepts* (Chapter 8) examines the use of *concept* in experimental psychology—though it should be noted that a similar account of concepts is used in various applied areas such as educational and developmental psychology that are not always experimental. McMullen points out that psychologists, excepting behaviourists,

take an internalist and abstractionist view of concepts: This is that the mind abstracts general features from a set of elements, or concrete instances, and so “acquires” concepts—mental classes or categories representing the elements. In this internalist, abstractionist model, acquiring a concept means establishing a mental structure. This is the familiar talk of modern psychology: The newborn possesses no concepts—or, by analogy in an experiment, the experimental subject does not have the particular concept whose acquisition is under investigation—and the story to be told is how in the process of learning concepts the child or subject turns concrete sensory elements into meaningful and of course *internal* representations of classes and categories of thing. This is the view of concepts as representational structures *in here*.

McMullen argues that this received view suffers from several related conceptual problems: the self-contradictory nature of representationist theory (which other papers in this collection also deal with); a homuncular and viciously regressive account of the meaning (reference) of the symbols involved; an incoherent reification of concepts; and a dualism as conceptually problematic as Descartes'. But the foundation of the abstractionist view is a logical and ontological error: the view that there can be “simples”, “semantic particulars” that form the basis of mental schemata. And this error is an instance of the more general failure to grasp that everything knowable or speakable must be complex and propositional. The abstractionist theory, widespread in modern cognitivism, requires that there be irreducible atoms of experience supplied by the world, and it is the activity of the mind that associates these simples into complex concepts or ideas. In this respect contemporary cognitivism continues the idealist, associationist tradition of Hume and others. Of course, modern theories use different and apparently more scientific terminology: Hume’s “simple perceptions” become “semantic primitives” in Anderson and Bower (1979). Nonetheless, what is required for associations even to start are concepts (e.g., “sameness”) of the very sort whose genesis from non-concepts

(meaningless sensory inputs) the theory is designed to explain. The cognitivist or associationist mind needs to have been pre-primed with concepts in order to acquire concepts—a matter that some cognitivists like Fodor seem at least to see and attempt to overcome by using a nativist strategy (see the following chapter by Maze). The realist account of concepts avoids these problems and obviates the need to think up “solutions” to them: Situations, the objects of knowledge, are *already* complex and propositional. There are no indivisible “elements” to be linked by mind.

The traditional theory of the association of ideas is, of course, an attempt to answer the semantic question of how we acquire meaning. This is something that becomes acute in this era of the syntactic, computational model of mind, and in that context the issue is about how one synthesises, bootstraps, meaning from non-meaningful elements by the application of purely syntactic procedures. McMullen takes the sort of critique he has mounted against associationism and applies it in some detail against the possibility of such bootstrapping, in any of its variants, in information-processing theory, connectionism, or any other. The task that psychologists have set themselves (it is perhaps information-processing psychology’s *central* problem), that of showing how mind can abstract or construct meaning from meaningless “atoms”, concepts from non-concepts, is futile: It is only “required” because psychologists’ ontology, and relatedly epistemology, holds that what exist, the objects of knowledge, are pure particulars. The answer the abstractionist provides simply begs the question, assuming the possession of the very concepts whose genesis it is supposedly explaining. Once it is grasped that the least and most that may be known is *already* propositional, it will be realised that concepts, like all objects of knowledge, are, as McMullen concludes “so to speak ‘out there’”. They are not internal reifications, representative mental entities, built up according to an impossible story of abstraction.”

Maze, in the third of his essays in this collection *Representationism, Realism and the Redundancy of 'Mentalese'* (Chapter 9) goes to the heart of what has come to be the scientific pride of psychology in the last half century: the development of cognitive science and the thesis that the mind may be understood in computational terms. At the centre of the computational model is representationism. This is the view that cognition, in which we must include perception, awareness and any apprehension of the world, consists of representing external entities or processes by some sort of mental tokens or symbols; that thinking, remembering and higher mental functions then consist in operating on those internal symbols—hence the ubiquitous phrase *cognitive processing*; and that behaviour is guided by the processed cognitive representations (beliefs). This is an indirect theory of knowledge in that all knowledge of the external world is mediated, is of representations of things, and is not knowledge of things themselves, to which we have no direct access. The computer is taken to be both a model for constructing theories of cognition, and a justification of the representationist epistemology that underpins cognitive modelling: If computers can “process information” by operating on symbolic representations of external events according to rules, then in principle minds can do the same, and it is the business of psychology to find out how this happens.

In spite of conceptual quibbles at the margins and some in-house differences about the best way to articulate the position, and exactly how representations might be embodied, a broad computational/representational view of mind remains the establishment position in empirical psychology, even when it is claimed by some cognitive scientists that they have “moved beyond” (the old Fodor style of) computationalism into, for example, neural networks or dynamical systems theory. It would be almost impossible to overstate the importance of this computational/representational thesis to modern psychology. It is the very backbone of most empirical theory, experimentation and application. Even areas like personality, clinical, or social psychology, outside of the main cognitive sci-

ence domains of perception, language, memory, cognition and so on, have come to understand and articulate their ideas in these terms. It is an orthodoxy that the advent of the computational model of cognitive science is what enables psychology to be scientific without reducing it to mere behaviourism; that is, has delivered a genuine *science of mind*. This conception of cognitive science is so basic to modern psychology that challenges to it are seen by almost all psychologists who think of themselves as scientific not as disagreements over how best to be scientific about mind, but as opposition to the possibility of scientific psychology—perhaps a sentimental rejection of the supposed dehumanizing view of the person involved in comparing it to a computing mechanism—and relegated to the unscientific fringes of psychology. Given how entrenched is computational representationism in mainstream psychology, it is worth presenting a somewhat extended commentary on Maze's arguments in this classic paper.

Maze argues that, even in the modern versions employed in cognitive science, representationism falls into solipsism, and provides neither the basis for distinguishing true from false cognitions, nor a satisfactory account of inference. Therefore, it is not the proper basis for a science of mind. Further, he examines Fodor's (1975) influential "linguistic" version of representation in which an innate language, *mentalese*, with its stock of rudimentary concepts is held to be needed both to learn any natural language and to cross the otherwise insurmountable gap between "meaningless" stimuli and meaningful cognitions. Maze argues that only a *direct* account of knowledge can rectify the deficiencies of representationism.

Now Maze agrees with Fodor both on the centrality of cognition to psychology and in wanting to establish a causal, scientific determinist account of mind. He has no wish to attack the computational model on the grounds of any supposed failure to fully encompass human agency. Rather, Maze wants to show that representationism is incoherent and cannot be part of a sound science of mind. Mod-

ern cognitive-science representationists like Fodor generally believe, if they have considered the matter, that somehow their representationism overcomes the long-known and fatal objections to traditional representationism (the thesis that all knowledge is of representations of things, and not of things themselves), namely: It leads to solipsism because we are restricted to knowing only what is inside our own minds; it is incoherent because it makes reference to things themselves while denying the possibility of knowledge of their existence; and even if there were some a priori reason to believe in an external world we could not distinguish between true and false beliefs, between how we represent the world to ourselves and the actual case, because there is no means of checking the latter independently of the former.

The reason that cognitive scientists like Fodor consider that they escape from these objections is, as Maze points out, that they see mental processes as embedded in a causal world and they regard themselves as engaged in the scientific task of tracing the causal links between external events, the stimulation of sensory organs, the transformation of stimuli from external sources into representations of those sources, and the operations on those representations that produce the complex cognitive states that guide action. And of course it is taken for fact that each step in this causal progression is real and material—it is not that cognitive science embraces mind-body dualism. So, because, there is a causal connection from the external world to mind there is no solipsism.

Maze deals with this defence against solipsism by pointing out that the very talk of external objects from which the causal chain derives is simply not allowed by representationism. As long as indirect perception is a universal principle (and it clearly is in the formulations of Fodor and others) there is for the representationist no access to such objects except via representations. Only by denying representationism and accepting direct realism can they consistently talk of the elements in the causal chain that they investigate.

There is curious “mote and beam” aspect to another of the representationists’ reasons for rejecting direct realism, the realist’s handling of the problem of error (see Rantzen, Chapter 10 in this collection). The indirect account of knowledge upon which cognitive science depends, and other criticisms of direct realism, draw aid and comfort from the supposed problem that direct realism has with error. There is a common but incorrect assumption that direct realism is committed to the idea that perception or belief is *correct*, that it cannot be wrong. And, since it is clear that beliefs *can* be false, this is taken to show that direct realism is mistaken. Indeed, the puzzle of perceptual illusion was a spur to the development of perception theory, and the fact of mistaken belief is taken to lead naturally to and justify representationism by treating false beliefs as misrepresentations of external facts: Facts are stable and incorrigible, but if beliefs are representations then they are variable and corrigible and that is where error may enter. Maze points out, however, that far from resolving the problem of misrepresentation and error, representationism in any form is unable to cope either with true and false belief or with the distinction between them. Even if some a priori justification or divine revelation established the existence of a mind independent world, the problem of knowing whether a particular belief was true or not would still be there for the representationist. Unless at some point we have direct access to facts, there can be no confirmation or disconfirmation of representations of states of affairs in the world.

Maze then turns more specifically to Fodor’s version of representationism in which the latter argues that it is necessary to have a language within which to represent external objects and events, and indeed to learn a natural language: That is, there is a *language of thought*, mentalese. This mentalese constitutes the basic semantic units from which the acquisition of natural language and knowledge proceeds. More generally, cognitive psychologists take it that when in an act of cognition we go from sensory input to perhaps basic representations to a properly formed belief like “there is a robin on

the lawn”, two sources of information are involved: the sensory input and a stock of information in the mind prior to the act of apprehension. The two together and some sort of matching process are supposed to enable the belief. Psychological theory is replete with the postulated internal entities supposed to bear this pre-perceptual information, schemata, scripts, constructs, templates etc. The language-of-thought hypothesis sets up the stock of innately given, prior information needed by the Fodorian mind, not just for a particular act of perception, but to allow any perception or knowledge whatsoever. Although Fodor might be reluctant to think of these as innate *ideas*, it is clearly a nativist position and what is innate is semantic—or there would be no point to this hypothesis. Of course, the problem of how this background knowledge was established and biologically hardwired, given that for Fodor there never is or was or could be any direct access to fact in the evolving organism, is the same as that of the acquisition of current knowledge in any indirect epistemology. Again, Maze’s point is that all these arguments in support of representationism repeatedly beg the question: They smuggle in premises that require the very direct knowledge that representationism denies, or accept as given something that is inexplicable within the terms of representationism. They are incoherent.

Maze does not offer the kind of detailed argument and evidence for a cognitive science embodying direct realism that the representationist does for orthodoxy. He could not: Representationism has been so dominant that non-representationist psychology is necessarily at the beginning. What he aims to establish, then, are the basic requirements of an account of knowledge that any psychology must incorporate if it is not to fall foul of the logical problems of representationism. These are that humans are able to gain direct knowledge of (come to stand in a cognitive relation to) objects in the world, though we know little about how our complex neurology *enables* this, and of course we won’t investigate in these terms unless we abandon the search for representations. It follows that cognitive

processes such as reasoning do not consist of internal operations on mental/neural symbols according to rules (though they may fit rules) but in perceiving aspects of the world's structure, for example, that a particular situation (a conclusion) must obtain if certain other situations (the premises) obtain. Thus, for any intellectually honest cognitive scientist the undoing of representationism should mean a complete rethink of psychological science. But as Maze says "Psychological science is sufficiently advanced to enable one to predict that the arguments of this paper will not be greeted with glad assent by those whose academic careers have been invested in representationist cognitive science" (p. 179). And two decades after the paper's publication representationist cognitive science proceeds apace.

Andy Rantzen's chapter, *Constructivism, Direct Realism and the Nature of Error*, (Chapter 10) continues with the matter raised by Maze: the long-standing issue of error and how realism can deal with it. This issue has two aspects: The first is, given that realism holds that perception is direct, of things or situations and not of representations or ideas of things, how can it account for the fact of error, where a person believes something to be the case but that situation does not obtain. For example, in the case of perceptual illusions, what might be the "object" of the false belief? The second issue has to do with the confusion between *direct* and *certain* or *indisputable* knowledge. It is often assumed by critics that the realist account of cognition must mean that this yields certain or indubitable knowledge and, particularly infuriating for the critics, that realists consider themselves to be in possession of that certain knowledge. It is sometimes held in contrast that a nonrealist, constructivist epistemology not only allows for error but that it promotes the virtue of tolerance in domains like psychotherapy by conceding that everyone's perception is equally correct.

Rantzen is concerned with the first of these issues, but not the second (which is discussed by Mackay in Chapter 15, and in less de-

tail by Maze in Chapter 13). Rantzen sets out the orthodox position on perception and cognition in contemporary cognitive science, and shows that it is constructivist in that it holds that the brain constructs knowledge of external situations from the limited and often deficient data that are presented to the senses, building intermediary representations of the world. This process is taken to be through symbol manipulation and indeed *inference* from the data. The external world, if perceived at all, is perceived indirectly. The direct objects of perception—or, more generally, of knowledge—are representations. If the representation correctly depicts the world, it is veridical; if not, it is error. In most constructivist views this is touted as a solution to the problem of error. So much so that it is considered fact by most of psychology. The constructivist criticism of realism is then that it has no means for explaining error because direct perception of external situations does not allow for a domain containing the false or mismatched “perception”.

In one sense, though Rantzen doesn't emphasise this, the realist has no case to answer, for the supposed alternative position contains a fatal logical flaw, as has been pointed out repeatedly, and so could not possibly be correct: If all cognition is indirect, as constructivists hold, there is no means of establishing correspondence between any representation (including of course the representation of the putative fact that “all cognition is indirect”) and the represented fact (including that all cognition is indeed indirect). Thus the constructionist has to abandon the notion of truth—and hence error—or recast truth as inconsistency between representations or some such formula. As this slides into relativism, the realist might justifiably feel that the constructivist has no business accusing anyone of an inadequate account of error.

Yet there is more to it than this, and Rantzen attempts to criticise and improve on the sketchy accounts of error in the realist literature. Realism is still faced with the problem that, as cognition is a relation between a person and situation, in the case of error, where

there is no situation to cognise, then either we are left with the impossibility of a single-term relation or misperception needs to be understood as a process different from perception. It is not simply perception gone wrong. Following O'Neil (1958) Rantzen discusses the apparent classes of cognitive error, those of omission and those of commission. In omission the correct perception is unavailable to the person either because of the deficiency of the perceptual apparatus or the inadequacy of the information coming from the environment. In commission, in spite of the availability of appropriate apparatus and information, the perceiver neglects this and fails to correctly cognise the situation.

Rantzen takes up Michell's suggestion (Chapter 7 in this collection; and 1988) that some sort of inhibition may be at work here—once again pointing to the importance of motivation so neglected by contemporary cognitive science. Rantzen takes part of this suggestion and argues that error, instead of being perception or cognition gone wrong, is in fact some sort of non-perception or non-cognition, a mechanism different from perception, such that misperception or error and cognition are asymmetrical. He outlines a hierarchical theory which rejects and replaces the notion of errors of commission in favour of an account of multiple errors of omission. The theory is based on the realist requirements that cognition is of external situations rather than of representations, and that the criterion for veridical cognition is the truth of that cognition. Rantzen argues that the failure to cognise correctly comes about because of either environmental or organismic obstacles to cognition, not the failure of inference from deficient information.

Rantzen describes errors of commission as *second-order errors of omission*. There are in his account three sorts of situation where a person fails to cognise some fact: simple lack of opportunity to cognise; some inability to cognise because of the nature of the perceptual processes or set-up; and where cognition is inhibited. All these are errors of omission. In contrast, errors of commission, typically

described as false beliefs, Rantzen argues are second-order errors of omission. This is where the person not only has failed to cognise some fact, but in addition has failed to cognise that first failure. He notes that the two-stage process may be the result of inhibition. More generally, any error of omission, of any type, may be compounded by another, second error of omission to do with the first error, that is, an error of perception about the error of perception. And this is in effect an error of commission.

Here, something needs to be said about J. J. Gibson because Gibson's account of perception (Gibson, 1950) is mentioned in several of the papers, including Rantzen's. The reason for this is that Gibson's metatheory, his direct-realist account of perception, is compatible with the realist account of cognition: But Gibson's theory considered overall is both a realist metatheory and a theory of the means and mechanisms of perception, and of course many different theories might be compatible with the one metatheory. In using Gibson's direct realism to discuss the issue of perception and error, the realist should be taken to endorse the metatheory without necessarily endorsing the particulars of Gibson's theory, which must be judged empirically as well. That Gibson's direct realism is a challenge to the metatheory of contemporary cognitive psychology is indicated when Rantzen mentions "Fodor and Pylyshyn's 58-page polemic against Gibsonian realism".

Psychology, not just cognitive psychology, is necessarily concerned with the apprehension and organisation of knowledge, and must depend on accounts of category, concept and class. In Chapter 11, Phil Sutcliffe's (1993) paper, *Concept, Class, and Category in the Tradition of Aristotle* offers a defence of the Aristotelian account of class and category on the grounds of inescapable realism. He starts by pointing out that the logical evaluation of theory has priority over its empirical test. A theory has to satisfy logical requirements before it is empirically assessable, and if a theory fails logical test and is logically compromised, it is untenable a priori and so empirical test

and observations are irrelevant. Many psychologists mistakenly think that logical issues may be resolved through empirical observation. This error is perhaps one of the reasons why conceptual work in psychology is so often seen as valueless, and perhaps why empirical psychologists are unmoved by even the most incisive logical critiques of theory and method.⁸ Sutcliffe's paper is an extended example of this point, how logical requirements constrain the psychological account one may give of such things as class, categorization and concept. We have seen that realist critiques, such as those offered by Maze and others, often aim to show that the positions they attack require the very concepts that the positions claim to have dispensed with (e.g., indirect theories of cognition need the possibility of direct cognition to avoid solipsism and recognise error). Sutcliffe employs the same strategy: He argues that proposed modern replacements of the realist, Aristotelian view of class and category require exactly that classical view which they deny, and upon which the supposedly alternative, modern theories—the kind now fashionable in psychology—are based.

Sutcliffe's arguments are close, detailed and cover some of the history of these fundamental logical notions, as a basis for later discussion of psychology's use of them. He outlines the Aristotelian tradition, from Aristotle (384 BC – 322) through Porphyry (234 AD – c. 305) to the *Port-Royal* logicians, Arnauld (1612 -1694) and Nicole (1625 - 1695), that a concept has intension and extension. Logically, the intension of a concept is the necessary and sufficient conditions for something to be of its kind, and the extension is all and only those objects (the class) satisfying the necessary and sufficient conditions given in the intension of that concept. This conception flows

⁸ One of the authors was astonished to hear a very distinguished experimental psychologist, when a symposium participant pointed out that his explanation of a cognitive phenomenon was fundamentally homuncular, reply: "I don't have a problem with that" and continue his exposition! He clearly believed that this response was an adequate rebuttal of a conceptual charge.

from the realist view that every existent thing has properties that may be predicated of it, and each property is of some thing or class of things. Thus the objects to which a term refers constitute its extension/denotation, and the properties that define it are its intension/connotation. But, as we have seen from McMullen's essay (Chapter 8 in this collection), logical concepts are, in the hands of psychologists, given a new interpretation. The realist, ontological basis of logical words such as class, concept, intension and extension, namely that they refer to states of affairs, is undermined, and psychologists use them as though concepts, classes and categories are purely mental structures, constructed by minds to interpret the world.

The psychologists' use of these notions is compatible with the trend in recent years to undermine the Aristotelian view. Some logicians and psychologists argue that the traditional view of category is inadequate, and they attempt to construct accounts of category based on ideas of prototype, or Wittgenstein's⁹ "family resemblance" view. In part this is supposed to allow for the genuine fuzziness of human concept learning, but behind this is a pervasive constructivism of varying degrees whose position is that things, properties and kinds are not in the world but in the minds (in schemata, for example) of the beholder. It is of course often those in the hard nosed end of psychology, involved in the experimental investigation of perception and reasoning, who are interested in specifying the exact nature of notions like class and category. But many of these otherwise scientifically-minded workers slip into non-realist accounts.

Sutcliffe painstakingly applies a common realist strategy, to show not that those who claim to have overcome and replaced the classical accounts are "not realist" but that, in setting out their modern

⁹ Maze (in Chapter 13 of this collection) and Hibberd (Chapter 14) similarly point out problems in an associated Wittgensteinian idea of *meaning as use*, a principle adopted by social constructionists.

substitutes for the traditional view, they assume the very (realist) concepts that they are supposedly replacing. For example, a number of classificatory systems attempt to replace monotypic (monothetic) with polytypic (polythetic) definitions of class. That is, they attempt to do away with the idea that a class of things is defined by necessary and sufficient properties of those things, perhaps to make it suitable for computational procedures for clustering. They might, for example, use resemblance of some of a class's members to other members, and a different resemblance of *those* members to yet other members to define membership of a class, polythetically. The class is then made up of a family or members with various not necessarily wholly shared features.

While one can make up such classifications easily enough, Sutcliffe points out that each attempt to construct an alternative account of classification starts with a demarcation, say, between the objects of concern and those not of concern, that cannot but use the traditional monothetic classification, namely one in which a class of objects is distinguished from another by virtue of the possession, or not, of certain necessary and sufficient properties. A polythetic classificatory system cannot be universal because it depends on the traditional monothetic concept of class. Sutcliffe presents a variety of arguments on the idea of the polythetic class, each showing that polythetic formulations of concept, class and category and related concepts are *de facto* monothetic.

Putting the matter as neutrally as one can, when dealing with the psychology of concepts, say where a person learns, acquires, employs or, in casual psychological talk, "has" a concept, we are always dealing with the pairing of the person with a concept. And an adequate psychological account of this process of "pairing" must then include an adequate account of the concepts themselves; must specify clearly what are the subjects of inquiry, that is, say what is a concept, a class, and a category—something that many psychological theories fail to do. Sutcliffe then turns to psychological treat-

ments of concept and category that have conceptual similarities to the logical theories that he has criticised. He draws on Rosch's prototype theory (1988; Rosch & Mervis, 1998) as an example of an influential "modern" and cognitive approach to the psychology of concepts. This approach, following similar themes in mathematical and philosophical theories of class, concept and category, holds that traditional Aristotelian, intensional, means of specifying a class—by giving necessary and sufficient conditions for membership—is inadequate, and needs to be replaced or at least heavily revised, by the importation of a family resemblance or prototype and extensional means of class specification. In the latter a class is somehow based on or best exemplified by a prototypical member, but nonetheless contains a range of perhaps poorer fit members that do not share an identical set of necessary and sufficient properties to define the class. As we move away from the prototype, the members of the class become increasingly less typical and at its "borders" the class is fuzzy, without the clear cut boundaries that an intensional definition gives them. This account of class and category is taken to fit the psychological facts, and the person in the act of categorising is held to make a threshold judgment that an object is sufficiently close to the prototype to count as a member of its family. Though there is no suggestion that Rosch or other modern accounts intends this, it is suitable to models of mind that treat the mind as imposing categories on the world.

Sutcliffe's response is to show through a series of demonstrations that, notwithstanding the practical difficulties that may arise in specifying the necessary and sufficient conditions for membership of any particular class, the application of prototypic and polythetic judgments always depends upon exactly the sort of monothetic discriminations that they are supposed to replace. There is no workable polythetic means for defining class membership. He concludes with an argument that for a person to "have a concept" is for that person to know the state of affairs that is the intension-cum-

extension of the concept, a state of affairs that exists independently of mind and whose properties are apprehended in learning.

In Chapter 12, *Normal Science, Pathological Science and Psychometrics*, Joel Michell makes a powerful attack on the very heartland of psychology's scientific pretensions: measurement. The realist tradition is to stress normal science as *critical inquiry* rather than as any particular set of methods and procedures (as commonly conceived in the education of mainstream psychologists), or as a social activity characterised by working on problems from within a paradigm (Kuhn, 1962)—as commonly conceived since Kuhn's thesis by the opponents of the mainstream. Error and even breakdowns—where theories are wrongly accepted as true and there is no serious attempt to test them—are inevitable in science because science is difficult and scientists are fallible, cognitively limited, motivated, swayed by ideological and commercial factors and so on. However, these are not pathological; the self-critical spirit and associated methods of science are a corrective, and this error-correction is normal science. Indeed it is the self-critical procedures, logical and empirical test, and not merely the inquiring attitude, nor immunity from error and breakdown, that distinguishes science from other forms of enquiry. But Michell argues that there are situations when science becomes pathological. This is when to a breakdown or error is added a higher-order breakdown: when, added to the error of accepting a false hypothesis, there is the *refusal to test it*, shaped by a higher-order, uncritical attitude to the processes involved. This, he argues, has happened in psychometrics.

Psychologists simply assume they are able to measure virtually any mental abilities, traits, or attitudes without needing to establish that the attribute being measured is indeed quantitative. They assume that the relations between the attributes and the test scores generated are quantitative, and the latter measure the hypothesised and presumed quantitative attributes concerned. But, as Michell argues, quantitative structure involves additivity. There are at least

some structures that are not quantitative (e.g., kinship structures), and so the assumption that psychological attributes indeed have quantitative structure is an *untested* empirical hypothesis: The “scientific task of quantification” simply has not been done in psychology, and developments in the means for assessing additivity—conjoint measurement theory (Luce & Tukey, 1967)—have been ignored by psychology. This might just be an error of omission, a breakdown in scientific process, and not a pathology, but for the fact that there is built into psychology is a higher-level dogma that prevents any serious examination of the matter.

Michell turns to the history of science and psychology to trace the genesis of this pathology. The Pythagorean thesis that nature is fundamentally quantitative was part of the scientific revolution, whose success obscured the fact that there is no necessity about this. Later, psychology’s bid to join the revolution was aided by the predictive value of psychometrics in various applications—even though this is really nothing more than actuarial relationships between test and criterion, no proof that tests are measures of anything. The failure of psychologists to do the scientific task of quantification was further obscured, and the problem entrenched, when S. S. Stevens adopted an operationist, antirealist, principle that the rules for making the relevant numerical assignments define the variables, and that measurement is the assignment of numerals to objects or events according to rule (Michell, 1997). This stipulatively defines (rather than discovers) psychological attributes as quantitative. Thus apparent gains—acceptance as a quantitative science, perhaps commercial payoffs in applied psychology—displaced the goals and procedures of genuine, scientific, critical inquiry, and has led us into scientific pathology.

The publication of Michell’s thesis in various places (for example, 1990, 1994, 1997, 1999, 2001, 2008, 2009a, 2009b, 2010) has produced a great deal of heated debate. Not all of those involved understand the realism that underlies Michell’s argument. There are two as-

pects of this realism that are important here. One is that Michell rejects the relativism built into Kuhn's popular view of science. Kuhn's view treats normal science not as a critical enterprise but as a mere social activity driven by a paradigm, largely a matter of applying a set of accepted procedures, models, theories and beliefs; nor does it treat science as a cognitive enterprise, but holds that truth and knowledge are only so within the paradigm. Kuhn therefore would not identify any such thing as a pathology of science, where the pathology depends on error. The second is, against the operationism mentioned above, that the quantitative or otherwise nature of variables is a matter of how the world is, not a matter of *how persons operate on the world*. More generally, the realist view is that the formal (logical and mathematical) sciences deal with the real structures of states of affairs in the world, and are not mere expressions of the forms of thought, discourse or convention—a view that has become fashionable in the past century.

Perhaps the major challenge to the orthodox psychological metatheory of empirical psychology in recent years has come from social constructionism and the sceptical account of knowledge that it and related philosophies embody—a challenge increasingly encountered as one moves away from cognitive experimental to social, personality and applied psychologies. This challenge is the target of Chapter 13, John Maze's critique, *Social Constructionism, Deconstructionism and Some Requirements of Discourse*.

Social constructionism rejects the traditional ontologies of mind in which the mind is set over and against the world, either in their dualist version, where the psychological puzzle is how mental stuff relates to the external material world, or in their modern cognitive science version where the puzzle is how the mind/brain represents the external world. Instead, it is taken that social discourse is somehow (ontologically?) primary and that mind, its supposed contents, and the world are constructed in that discourse. Their research focuses on how the various social/psychological subjects are shaped

and influenced by discourse, particularly the discourse of those in power. And it is taken that the subjects, which might be such different things as justice, emotion, self, psychological disorder, or even mind itself, have their being by virtue of that discourse. It follows that knowledge (if one allows such a thing, and sometimes social constructionists do not) of the discursively constructed world cannot be objective or true. It is relative to the context and interests of the language community, subject only to arbitrary, socially constructed rules. Realism and mainstream cognitive science are seen by constructionists as similar and both attacked as positivist, and more generally as “foundationalist”, part of a long tradition going back at least to Plato in which thinkers attempted to establish certain foundations for all knowledge.

Deconstructionism, a movement more important in literary and cultural studies than in psychology, shares a style of argument and certain general theses with social constructionism, and influences it. In both, language is given priority over, and somehow constitutes, all those things that naively we might suppose pre-exist language and to which we think we refer from within it. For deconstructionists, the relation of meaning, of “signifiers” to that which is “signified” is not that of a word to a thing, reference, as we might ordinarily suppose it. But, because nothing has any independence of language or rather, as one might say, there is no ontology, everything that we speak or think “refers” only to another equally arbitrary signifier. There is no world that gives final meaning to signification and no objective knowledge.

Maze addresses himself to important theses in these two related positions, taking as representatives leading figures in each, Kenneth Gergen (e.g., 1985) on social construction and Jacques Derrida (e.g., 1990) on deconstruction. His broad strategy is to show that their positions do not meet the standards of coherent, intelligible discourse—something that he takes to be a foundation of realism. As in other essays, Maze makes the point that discourse requires that we

must reject any idea of constitutive relations: A relation can only be sensibly spoken of as holding between¹⁰ two or more independent terms, and cannot constitute those terms. To repeat a theme that appears in a number of the essays of this collection, it is nonsense to say that the relation of mind to some thing (for example, knowing something) constitutes that thing; that there are things whose existence is constituted by a relation as, say, an image might be supposed to exist by virtue of being the object of awareness. This is a charge used against a number of idealist accounts of mind and knowledge, because they are riddled with this sort of error.

Interestingly, Gergen picks up on aspects of this error, rejecting the representationist theory of knowledge, but in reaction falls into a general scepticism denying the possibility of knowledge, truth and objectivity. Dismissing as foundationalist any views that hold to these concepts, Gergen stresses the performative and political aspects of theoretical and scientific claims including, supposedly, of his own theory—as though this solves the problem of truth. Gergen’s theoretical focus has, as Maze acknowledges, produced the valuable unearthing of social and political commitments disguised as science, but Maze points out that the claims social constructionists make about language, persons, people, emotions, real-world practices, knowledge, historical contingency, construction, power relations, are just that, claims that *something is the case*. To say something is constituted in social discourse makes no sense without there being some entities doing the constructing. In short, constructionism depends on exactly the concepts the constructionist is ostensibly rejecting. In this constructionism is self-contradictory.

Maze suspects that social constructionists and deconstructionists are in part critical of the possibility of *objective knowledge* because

¹⁰ It may be noted that while social constructionists reject representationism, various of their philosophical kin such as psychological constructivists (discussed by Mackay in Chapter 15) take a representationist view of knowledge.

they confuse this with a claim to *indisputable knowledge*, and they think that the possibility of error means the impossibility of truth. But the realist thesis on the possibility of objective knowledge is merely that something believed may be true and, of course, it may be false. It allows for the possibility of truth (and error) and has nothing to do either with indisputability or with a foundationalist quest for certainty.

Maze also argues that the deconstructionist thesis on the indeterminacy of all meaning and the impossibility of reference is as similarly general and corrosive a scepticism as that of constructionism, and is subject to the same critique. In practice, deconstructionists, like the constructionists, must exempt their own utterances from their claim that no assertion is true or false and no matter is decidable, for otherwise in the very act of uttering it, they contradict that claim—or must be held to be outside discourse and simply making sounds. Deconstructionists take the anti-objectivist thesis further. Derrida argues that logical principles such as laws of identity and contradiction are arbitrary, relics of an outmoded and context-dependent logic, and that something *can* simultaneously be itself and not itself. Maze's reply takes us back to the requirements of discourse: Derrida's theory about the redundancy of logic is either self-contradictory or quite literally unspeakable, dropping out of discourse into unintelligibility.

In sum, therefore, to make the kinds of critique that both constructionists and deconstructionists wish to (often justifiably), and to uncover in theory and practice the hand of powerful interests, social forces, and dissembling, requires the very concepts of truth, objectivity and logic that their philosophies deny.

It might seem odd to include here only the final paper of a series, Fiona Hibberd's *Reply to Gergen* (Chapter 14), without including either the original papers which Gergen attacked (Hibberd, 2001a, 2001b) or Gergen's reply (2001). However, Hibberd's paper stands on its own. It confronts directly several common misconceptions about

realism that were expressed in Gergen's paper, and in doing so states and clarifies some realist fundamentals. It also exposes a certain style of argument, a sleight of hand, by which constructionists, amongst others, disguise self-contradiction as merely embracing an alternative, nontraditional form of discourse. In her earlier papers in this series (2001a, 2001b), Hibberd argued that social constructionism and positivism share some important similarities, a conventionalism, a meaning-as-use thesis, and an antirealism—in spite of the fact that social constructionism sets itself up against positivism. Moreover, critics misidentify realism as positivism.

In her reply Hibberd shows that the realism for which she argues, being *situational* realism, does not ignore context, as charged by Gergen. A basic principle of realism is that whatever there is, is an occurrence or situation in space and time (see Chapters 1 and 3). That is to say, whatever exists is a situation located in context, and this affects that situation. Hibberd illustrates the importance of context in the realist account of causation. In contrast to simple linear accounts of cause widely used in psychological research, realism takes causation to be a complex relation where an *event* acts upon *field* or context to produce an effect—a change in the field. Depending on the field (context), the same event or situation may bring about different effects, and different events or situations may bring about the identical effect, so the concept of a causal field is of central importance in causal analysis. In psychology persons, being unique both as individuals (regardless of shared properties) and by entering into many relations, produce a unique causal field. This fact has important methodological implications, but is typically ignored in the popular statistical procedures adopted within psychology.

Next Hibberd tackles Gergen's argument, a style of argument that is particularly slippery. It is carried out via a technique that, as Stove points out elsewhere (1981) "neutralises success words," that is, takes terms that ordinarily indicate reference, truth, falsity, fac-

tuality, contradiction, assertion and so on, and emasculates them by various means, putting them in scare quotes, perhaps, or rendering these all as moves in a “game” that might be played differently, claiming that they are irrelevant, or otherwise reframing them as not indicating reference, truth, falsity and so on. The constructionist trump card—if we may introduce a game reference of our own—is of course that the critic cannot show this to be an invalid argument or that the conclusion is false because to do *that* is to enter a different game, a traditional game, indulging in a “rage for the real” (Gergen, 2001, p. 423) that the constructionist is not obliged to enter¹¹. However, Hibberd shows that in spite of his protestation, the social constructionist thesis of truth within traditions or games both denies the possibility of external reference, and depends upon it in the very act of its utterance. It is what in terms that the constructionist might otherwise favour a performative self-contradiction. That is, when Hibberd and Gergen agree or disagree, say, on the influence of continental epistemology on social constructionism, they “agree on a certain state of affairs”. It is just this that realism says is a fact, a situation, and is not merely true under a particular set of conventions. Hibberd extends this argument to show that treating logical principles as conventions, mere rules, that could be changed at will, simply makes discourse impossible, including of course the discourse that Gergen is trying to sustain in attacking the views of his critics.

The relevance of the themes of Hibberd’s paper to psychological practice may be seen in Nigel Mackay’s *On Some Accounts of Meaning and their Problems* (Chapter 15). It deals with a compound position that includes both a prevalent account, or set of accounts, of meaning and a related group of arguments used to defend that account against any possible criticism. This is a position that is taken not just by specialists in theoretical and philosophical psychology, but also by practising psychologists reflecting on their own practice,

¹¹ The same argument is discussed by Mackay (Chapter 15)

who absorb and take comfort from the views of the former. The approach, which following some of its protagonists we will call *meaning-making*, is opposed to the cognitivist mainstream and has gathered strength to the degree that the theorists present themselves as offering a viable challenge to the psychological orthodoxy. It is a family of psychologies in domains such as psychotherapy, narrative psychology, constructivism, social constructionism, developmental constructivism, epistemology, social discourse analysis and beyond offered by those who wish to redirect the cognitive turn in psychology. They identify their common focus as meaning-making, arguing for a change in attitude towards persons and the authority of scientific psychology supposed by the establishment. They support this by appeal to an anti-objectivist, constructivist (or constructionist), postmodern philosophy. The bones of this partly moral argument, are that knowledge and reality are constructions by persons and not objective. Claims by psychological scientists and practitioners to know what leads to and ameliorates “disorder”, and so to know others’ reality and what changes to effect in them, are therefore hubris. Further, such claims to truth are likely to be expressions of power relations disguised as objectivity. Meaning is implicated in that what persons construct (and there are different accounts of how this is done), *their* knowledge or reality, yields *their meaning*. And it is the meaning of the constructed world that explains why they do what they do, what they feel, think, or what distresses them.

Like Maze in his paper on social construction (Chapter 13), Mackay is in the position of agreeing with many of the sentiments and some of the principles in the meaning-makers’ argument: He too holds that that psychology needs to be tolerant in theory and in practice, and that meaning is a vital yet neglected aspect of psychology. Moreover, Mackay argues that meaning is indeed not inherent in objects—not a *property* of apprehended objects—and is constituted in the relation between persons and objects. But at this point he parts company with the constructivists and constructionists. He argues that tolerance is demanded not by the impossibility

of knowledge but by the state of psychology's ignorance; that is, we require objective knowledge, about such things as the ignorance and limits of psychology and the facts about how damaging dogma is in psychotherapy and other applied psychologies, in order to uphold the principle of tolerance. Were there no objective knowledge, the constructivists and constructionists could not consistently appeal as they do to facts, about the power relations embedded in psychological practice, about the variability of meaning across cultures, etc. They could not even engage in meaningful discourse about anything.

Mackay argues further that *meaning* needs further explication. The term is used indiscriminately in psychology to refer to at least two different psychological processes; one might be termed *symbolic meaning* and the other *meaning as salience*. The first is that where words, signs, acts, marks and tokens of various kinds have meaning in that they stand for or refer to something else, in language, conventional or other myriad and varied representational systems. In propositions that make reference to situations, these tokens become part of truth claims. Meaning as *motivational salience*, is *meaningfulness*. It is where something has particular salience (is experientially meaningful) to a person, by virtue of its place in his or her system of interests. In this sense a harsh word from a lover may have particular meaning (be meaningful) to a person in a way that it would not from a stranger because of the importance of the lover in that person's system of interests. Meaning is a relation between a person (specifically motives) and objects. It is not constructed as part of a non-objective individual or social reality, though it does result from the interaction between persons and objects relevant to their motivational interests. In line with a realist account of the independence of things from the relations between them, and the objective nature of relations, Mackay argues that the relation of motivational salience is an investigable part of the real, determinate world, though as a relation it does come into being in the interaction between persons and objects.

As with Maze (Chapter 13) and Hibberd (Chapter 14) part of the paper is also given over to a critique of the kinds of replies that constructionists generally give to criticism of their arguments, particularly as exemplified in the response papers of McNamee and of Raskin and Neimeyer (McNamee, 2003; Raskin & Neimeyer, 2003) to the original version of the paper (Mackay, 2003). The constructionists' primary *defensive* move is to argue that constructionism is simply immune to objectivist assessment because they do not accept the objectivist premises. Specifically, among the defensive responses are that language is not about reference, but about making things happen; that constructivists make no assertions, but only offer an alternative discourse, in a world of many equally real constructed discourses, and in a spirit of openness and pluralist tolerance that derives from their epistemic pluralism; that truth (in as much as it is a useful notion) is contextual and is relative to discourse; that even debate, rationality and logic are contextual. To this Mackay replies, in similar vein to Maze (Chapter 13) and Hibberd (Chapter 14), that even the description of what constructivists do or do not do, their statements about how language functions and does not function, their references to what some realists said or did not say, require, if these are to make sense, acceptance of the very things that the protagonists deny: The constructionist is indeed making claims about states of affairs, wants others to believe that they are correct, and their discourse depends on logical principles that are not optional, such as maintaining a distinction between something being that thing and being not that thing.

The constructionist's *offensive* move is to accuse realism of a range of errors: It is essentialist, absolutist, foundationalist, realists claim to know an absolute, transcendent reality or truth. Realism is also epistemically arrogant, claiming to know with certainty. Mackay's response is to point out that this is a "realism" of the constructionist's own making, a target that is a mixture of positivism and much that realism rejects. However easily these attacks can be shown to be simply misdirected, they indicate how deeply ingrained

are the common misunderstandings about realism: It is positivism; it is just the orthodox view of science and knowledge. These mistakes about what realists claim have been pointed out in detail (Maze, 2001) and even the commonalities of constructionist and realist critiques of psychology explained in the literature with which constructionists are apparently engaged. The persistence of this mischaracterisation suggests that there runs through the constructionist approach a hostility to the canons of argument and evidence and this limits their capacity to take reasoned opposition seriously and allows them to dismiss as “uninteresting” evidenced positions that oppose theirs.

In Chapter 16, *Why Psychology has Neglected Symbolism and what a Realist Approach can Offer*, Agnes Petocz continues with the theme of meaning discussed by Mackay, but her focus is on symbolism and symbolic activity, phenomena whose ubiquity makes it relevant to almost every area of psychology. Her analysis begins with an historical and conceptual investigation of the reasons for the neglect of symbolism in mainstream psychology. She argues that this neglect has been the result of converging conclusions about the scientific intractability of symbolism, first from the vast extra-psychological literature, and second from the psychological mainstream, which has neglected meaning in its efforts to attain scientific respectability. Petocz acknowledges that there are signs of change, and notes that, with the recent movements within mainstream psychology towards expansion and integration, the climate is now favourable for the return of meaning and symbolism. However, smooth integration is being hampered by the inability of psychology to find a suitable metatheoretical framework. Here, she addresses the point we identified in our introduction: Most contemporary mainstream psychologists would consider themselves to be realist and would not think the issue worthy of debate. Yet, as is argued in many of the essays, the mainstream position is neither *consistently* realist nor *genuinely* scientific. Instead, it remains just as trapped in aspects of

Cartesianism as are the supposedly antirealist Cartesian proponents of the new movements.

In the second part of her chapter, Petocz sets herself to answer the question what a *thoroughgoing* realist approach can offer towards a general theory of symbolism. Rather than present a complete account of the principles of realism, she selects just five key points helpful for a discussion of symbolism and meaning. Here, we meet again the idea of the conditions of discourse, the direct-realist view of cognition as a *relation* between knower and known, the point that the terms in a relation cannot be constituted in whole or part by that relation, the ontological egalitarianism of realism, according to which there are no “levels” of reality or “degrees” of truth, and the broader conception of scientific method as critical inquiry. Together, these principles strike at the heart of mainstream psychology’s position, exposing its misconceptions of realism, and having radical implications: that mind is not in the brain, that the contents of consciousness are not private, that relations such as cognition and meaning are as real as anything else that exists and are thus legitimate objects of scientific investigation, and that the attempt to investigate nonquantitative phenomena via quantitative methods is scientifically inappropriate. Some of these themes have appeared in earlier essays, in the context of other topics, but Petocz applies them to the task of showing what a realist approach can offer towards a general, scientific theory of symbolism.

She adopts the *modus operandi* of all of the essays in this collection - which is to begin with conceptual analysis, following the realist view of science as critical inquiry and the principle that conceptual/logical testing must have priority over empirical testing. Locating symbolism within the broader domain of meaning, she offers an analysis that unites different types of symbolism. She argues that any theory of symbolism must respect certain logical constraints, the primary constraint being that symbolisation is a three-term relation. Because one of the terms in the three-term relation must be

a cognising organism, another logical constraint is that a theory of symbolism must be a psychological theory. This guarantees the central role of psychology in theories of symbolism. From that logical constraint there follow a number of psychological requirements which any psychological theory could reasonably be expected to meet (e.g., explaining how and why symbols occur, the bases for the symbol-symbolised connections, individual versus universal symbolism, and so on). She then shows briefly how these constraints and requirements are either violated or neglected in the many different existing approaches to symbolism to be found in the non-mainstream literature (e.g., in hermeneutics, semiotics, sociology, anthropology).

For an alternative, realist theory of symbolism, especially one which can extend its explanatory reach to difficult cases of symbolism, Petocz draws upon psychoanalytic theory, particularly the writings of Freud. Clarifying Freud's often confused theory, she offers a realist, scientific version of psychoanalysis in general and symbolism in particular. This is based on the same aspects of Freud's theory that other realists, including Anderson, have found to be valuable for a realist psychology: the Darwinian deterministic approach to motivation in terms of instinctual drives; the cognition-motivation connection with the drives understood to be the subject terms of the knowing relation; the role of unconscious mental processes (desires and beliefs) in the production of human behaviour; and the vision of scientific investigation as extending legitimately to the *combination* of hermeneutic inquiry and causal explanation.

Petocz's chapter is relatively long, dense in content, and tightly argued. This is because the essay is largely a condensed form of material that was presented in more detail and elaboration in her earlier book on Freud, psychoanalysis and symbolism (Petocz, 1999). The last sections on psychoanalysis and Freud's theory are particularly truncated, and the reader from mainstream psychology may feel that the theory of symbolism seems more *hers* than Freud's (e.g.,

she acknowledges that this is not the standard Freudian theory), that it requires more empirical substantiation, and that it is neither necessary nor appropriate to attribute so much to Freud. However, anyone who is familiar with the contents of Petocz's book and/or with Freud's published writings will appreciate her general stance that these ideas and themes are well entrenched and even developed in the Freudian corpus, albeit often so embedded in and compromised by other irrelevant or conceptually untenable material that a major task of textual extraction, exegesis, and synthesis is required.

Chapter 17, Philip Bell's *A New Psychology - The Metaphysical and the Mundane* deals with the discipline of cultural studies, an area that, like symbolism, is not of mainstream concern for psychologists working in the empirical traditions of Anglophone psychology departments, yet one where, as he demonstrates, theories require psychological assumptions to attempt explanation of what is a very human product, culture. Bell takes the work of Brian Massumi (2002) and, more briefly, of Lisa Blackman (2008), as typifying recent writing in cultural studies, an area well outside the interest of most psychologists. The language, major names, theories, journals, and studies (rarely empirical in any sense that psychologists would recognise) would be alien to most psychologists. But it is included here for several important reasons. If we treat psychology as a social or human science, then cultural studies becomes a sister discipline or even rival way of trying to understand human action, albeit one very different from mainstream empirical psychology. Indeed, all the human and social sciences depend on a psychology, whatever name it may go by. That is, they incorporate an account of the determinants of action and of the place of motives and values in this, a theory of mind, of knowledge and its objects. More generally, and as they must, they take positions on what constitutes truth (though they may be disinclined to use that term) in the human sciences. Indeed, as Bell argues, they take the mundane notions of psychology and give them a metaphysical interpretation. They may then be

subjected to the same analyses as the other topics of the papers here—and Bell explicitly sets out to apply to poststructuralist literary-philosophical theory the sort of criticisms that Hibberd (2001b) and Maze (2001 & Chapter 13) use against its less radical cousin, social constructionism. Moreover, while the intellectual style of cultural studies is very different from mainstream psychology, and its idiom would be unrecognisable to most psychologists—a factor that keeps the two disciplines apart—yet the *Weltanschauung* that maintains and informs it, a postmodern, intensely theoretical, and anti-empiricist philosophy, has in fact made inroads into psychology. Social constructionism and personal constructivism, hermeneutic readings of psychology and psychoanalysis, phenomenology, deconstructionism, and psychologies of meaning-making (part of the earlier mentioned heterogeneous group in psychological theory) ground their arguments in one or other variant of the same postmodernism;¹² and a surprising proportion of the articles in psychology's theoretical journals display the same sympathies.

Bell juggles two main tasks, one is to examine the opaque and slippery text of Massumi, interpreter of the major “Theorist”, Gilles Deleuze (1996), and of Blackman in her undergraduate text (2008). Massumi's writing is dizzyingly abstract, and semantically elastic to the point of incoherence. Yet Bell extracts a number of identifiable theses from Massumi's work (2002). These are various positions on philosophical, particularly ontological, and psychological matters: on the nature of the subject, on what is to count as real, on language, on determinism, on memory, thought, affect and desire. They are, however, not necessarily what Massumi claims them to be. The other of Bell's tasks is to show that, in spite of the typical poststructuralist claims to transcend dualisms, overcome the static conceptualisations of phenomena, traditional causality and the

¹² This may be controversial in that postmodernists are inclined at times to distance themselves from (say) phenomenologists or older style hermeneuticists. “Poststructuralism” is a favoured term for the approach of these writers in “literary-philosophical” theory.

categories of empirical science, or to avoid idealism, essentialism, foundationalism, and hypostatization, authors such as Massumi and Blackman commit standard antirealist errors.

Bell points out that large parts of Massumi's work are simply incoherent. His reasoning is often circular, and even in apparently endorsing a position he will undermine it. Massumi, for example, in saying that perception is not subjective—with which a realist must agree—says that the thing perceived is its being perceived. In this he violates the conditions of ordinary discourse, treats things as constituted by their relations, and ends up in a subjective idealism. Massumi also follows other poststructuralists in attempting an ontology without objects or beings. Instead, he focuses on "becomings", as if these might exist without the entities that change, or more generally on relations as if these might exist without the terms that subtend them. Thus relations become reified, and even treated as agents in a dynamic world. Bell also points out the vitalism that this is associated with: Becoming requires life to be invested with the power to become.

Bell shows that, though written in a language alien to empirical psychologists, cultural studies requires a psychology, and many of the same problems that the other essays in this collection claim exist in orthodox psychology persist in cultural studies. Bell's final comments indicate something that, in our view, may be the most troubling of all. The writing that Massumi exemplifies—though Bell treats it seriously and avoids the temptation to parody—is shown by Bell to be intellectually dishonest and divorced from critical inquiry. Like some of the other postmodern views discussed in these papers (Maze, Chapter 13, Mackay, Chapter, 15), it has built into it an immunity from criticism. This is the view that critics, should they point out contradictions or problems, do so because they are ensnared in the illusions of positivism and an antiquated idea of truth and the hubris of realist certainty. Critics fail to understand that texts make no claim to truth, but are written merely to illuminate

meanings, and to augment experience. Therefore, the immunity thesis goes, criticism may be ignored.

In the next chapter (18), Joel Michell takes up the matter of *The Place of Qualitative Research in Psychology*. In recent years there has been something of a revival of qualitative methods in psychology, crossing over from disciplines like education, social and cultural studies, and clinical disciplines. As Michell points out, this is often justified philosophically, by associating traditional “realist” research solely with quantitative methods and claiming that an alternative, that is a nonrealist, paradigm is required to accommodate qualitative research. Michell then turns “the qualitative question” on its head; in effect, he asks not “can psychology legitimately use qualitative methods?” but “are we in psychology using quantitative methods legitimately?”

Michell’s answer, as in his earlier chapter (12) is that we do not know—yet. But there is in science and philosophy of science a powerful *quantitative imperative*—an outgrowth of the Pythagorean-Platonic view that reality is fundamentally quantitative. And this, together with a desire to repeat the successes of the quantitative natural sciences, has led mainstream psychology to *assume* that psychological variables are quantitative, without first doing the scientific job of testing the hypothesis that they are. Consequently, traditional psychologists take it that the scientific method is and must be quantitative, and identify it with measurement and experimentation.

Thus it emerges that often advocates both of qualitative research and of quantitative research methods, though they hold to different views of truth, knowledge and method, in fact agree to what in a related context Sherwood (1969) called the thesis of the separate domain. This is that there are separate domains of knowledge achieved by different means. It is common for the apologists for qualitative research, for example Guba and Lincoln (1994), to argue that there are different discourses, different paradigms of research,

with different underlying philosophies, qualitative research needing to reject that of the positivist, scientific mainstream and its quantitative experimental traditions, and adopt a postpositivist philosophy. And of course, from the other side, the traditional empirical mainstream are deeply suspicious of qualitative research and its philosophy. One only needs to spend a short while in the company of either group to grasp this mutual suspicion.

Once again, realists find themselves, if not calling for a plague to descend on both houses, at least wanting to make clear that the con-
trast of these domains is misguided: Science is not positivism; quantification is neither necessary nor sufficient to define science; to reject the quantitative imperative is not to abandon objectivity; to allow that qualitative research leads to knowledge is neither to abandon science nor to embrace the postmodern relativisation of truth in which each discourse is a linguistic framework with its own logic and its own internal, paradigm-dependent standards of truth. The realist conception of science is, Michell insists, that of *critical inquiry*. This is the fundamental method of science. Discourse has the form it has because it makes contact with reality. Things can be as claimed, and truth is possible. Indeed, to assert otherwise is to deny our assertion in the very act of saying it. Particular methods, quantitative or qualitative, are supplementary and do not define science. And what makes a method scientific is that it uses a combination of careful and systematic observation and the best available error-detection mechanisms to bring the investigator into better contact with phenomena which might otherwise remain hidden. It does so by the means that we mentioned in connection with Michell's chapter on the pathology of psychometrics (Chapter 12): by being a method of systematic doubt and error-correction in the face of our epistemic fallibility.

Michell also discusses the realist account of situations and the idea of quantity. Any situation is propositionally structured; something is predicated of some subject term. Number and quantity are

features of all situations just because subjects are countable things located in time and space—hence the source of the temptation to Pythagoreanism. Yet the attributes of the predicate term may or may not be quantitative. It is a matter of discovery. For example, the main phenomena investigated by physics turned out to be quantitative. But much of the data of psychology appear not to be quantitative in that sense, but qualitative. They may perhaps be ordered (e.g., that taste is sweeter than this) but not properly quantitative (that wave is 2.4 times higher than this). Further, in spite of the fact that psychologists make repeated observations on these qualitative phenomena (e.g., intellectual ability) and generate frequencies from their aggregated data, this does not amount to an observation of their quantitative structure. They are only taken to be so because psychologists import the assumption that the underlying structures *must* be quantitative. There are some psychologically important phenomena, for example meaning (see the discussion in Petocz, Chapter 16) whose nature means that even these psychometric transformations cannot be applied. Rather than considering that a qualitative approach may be required, traditional, quantitatively-obsessed psychology, neglects them, thus relinquishing the opportunity to develop accounts of the possible qualitative structures of psychological phenomena.

The theme of the scientific legitimacy of qualitative methods is picked up in the next chapter (19) in Agnes Petocz's *Science, Meaning and the Scientist-Practitioner Model of Treatment*. At first glance it would seem to be a long way from the abstract world of psychological theory. But its force is to demonstrate just how closely theory and practice are intertwined, and just what are the costs to psychological practice when that practice is based on inadequate or flawed theory.

Petocz addresses a response by Robert Sternberg, the President of the American Psychological Association, to a media article which described scientists and practising psychologists as engaged in con-

tinuous warfare across a “scientist-therapist” gap. Sternberg rejects this, insisting that there is no such warfare, and setting out the various aspects of the scientist-practitioner model which underpins the academic training of psychological practitioners. Training via this model is designed to ensure that science informs practice and practice informs science in a continuing process of mutual support and refinement. Petocz’s objection is not to the model; she agrees that it is justified as the standard for all forms of psychological intervention. But, she argues, the *implementation* of the model is only as good as the conception of science on which it is based, and that is seriously defective.

The paper centres on one of the major themes of the present collection of essays - the nature of science as *critical inquiry*, and the extent to which that is neglected or violated within the supposedly scientific psychological mainstream. The core of Petocz’s argument is that it is not *science*, but a package of distortions driven by *scientism*, that prevails in psychology and that involves deep misconceptions about two things: the meaning of science and the science of meaning. Prominent among these distortions is psychology’s attitude towards measurement and psychometrics, as discussed extensively by Michell (including in chapters 12 and 18 of the present collection). But there are many other distortions, ranging from various methodological and data analytic practices to ideas about what *content* is appropriate for scientific psychological investigation. One major content area that has been excluded is that of meaning. This has reinforced the idea of a science-meaning gap, and has fuelled the hijacking of meaning by non-mainstream movements ideologically committed to antirealism. Consequently, it has left the community of academically trained psychologists with the unfortunate view that clinical and other areas of practice must depart from science to the extent that the practitioner wishes to deal with meanings.

However, as Petocz argues, this view has no scientific warrant. She sets out the realist aspects of science, including its necessary methodological attunement to the nature of the subject matter of inquiry, and its rejection of a simplistically linear approach to causality. She then draws out the implications for psychotherapeutic outcome or efficacy research, particularly with respect to the question of what counts as *evidence*, showing that these implications strike at the heart of the “evidence-based” practices sanctioned within the mainstream by appeal to the scientist-practitioner model. She then addresses the meaning of meaning and its place in scientific psychology. Her treatment of meaning overlaps with her treatment of symbolism (in Chapter 16), highlighting the nature of meaning as a three-term relation and the logical constraints and psychological requirements that must be met by any adequate scientific theory of meaning. She notes that the way ahead looks promising, for there are increasingly sophisticated qualitative tools and techniques suitable for the scientific investigation of meaning.

Petocz closes the paper by discussing the implications of her arguments for the scientist-practitioner model of treatment, and the changes in the implementation of this model that would be required for psychology to do justice to it. She thus offers some suggestions for progress in scientific psychological practice, and identifies three main positive consequences. The first, following from a more sophisticated conception of causality, would be a better understanding of the relationship between theory and practice, leading, in turn, to a more healthy appreciation of the minefield that is psychotherapeutic outcome research. The second, following from the rehabilitation of meaning within mainstream psychology, would be the beginnings of a breaking down of some of the pseudoboundaries between behavioural, cognitive-behavioural, psychodynamic and other theories and treatment models, albeit in a way that respects the limits of any such eclecticism. The third, following from appreciation of science as critical inquiry, would be an overhauling of understanding and teaching in the entire field of research methods,

thus dispelling the misguided tensions between quantitative and qualitative approaches, and producing a new generation of practitioners better equipped to attune their treatment to the nature of the particular person/problem/environment constellation that they are faced with.

In addressing itself to practical issues at the heart of psychology, Petocz's paper shows in a number of ways the relevance to psychology of the principles of realism discussed in these essays. It answers directly Stam's challenge, mentioned in our introduction, for realism to show how it can offer "a psychology of practical significance". Importantly, while many practising psychologists have called for greater flexibility and multidisciplinary on political, economic, or social grounds, a realist approach shows that such flexibility is warranted on purely *scientific* grounds. The realist message is that proper adherence to the scientist-practitioner model requires that we make sure that we get our science right.

The next chapter (20) picks up on a theme touched on in Petocz's paper, and discussed in some of the earlier essays (especially Chapters 5, 6, and 16), that of realism's combination with aspects of psychoanalytic theory. Simon Boag's *Addressing Mental Plurality: Justification, Objections and Logical Requirements of Strongly Partitive Accounts of Mind* deals specifically with the convergence of the realist relational view of mind and the psychoanalytic pluralistic theory of motivation. If knowing is a relation between a subject (the knower) and an object (the situation known), then it is of interest to ask whether there is within each person just a single, unified knower or "self", or whether there is a plurality of knowers, and, further, what exactly is the nature of this knower or knowers. Boag argues that there are convincing reasons for adopting a strongly partitive view of mind. Abnormal phenomena such as dissociative identity disorder and split-brain states, together with normal cases of self-deception, mental conflict in desires, and repression, all seem to suggest that the mind is not a unity but, as Anderson, following Freud, described

it, a “society or economy of impulses” (1934, p. 74) that is, a “set of drives or urges and not ... an abstract cognisor” (1953, p. 360) Boag traces to Plato’s Republic the view that psychological conflict entails a multiplicity of knowers or desirers, and he explicitly follows Maze (1983) and Petocz (1999) in adopting the view that the competing knowers are the motivational systems, the instinctual drives, as conceived by Freud.

Despite its obvious relevance to psychology, the topic of mental unity versus division is widely discussed in the philosophical literature, but rarely within psychology. Boag considers the various philosophical objections and competing approaches, according to which mental conflict and the phenomena of irrationality do not necessitate mental partitioning. However, these alternatives do not survive Boag’s critical scrutiny. For example, Heil’s (1989) account treats mere beliefs as somehow implying direction of action, when, as Boag points out, beliefs are policy-neutral and need to be combined with motivational states to produce action. This point is relevant to mainstream psychology’s widespread preference for *cognitive* theories of motivation (a theme examined by Newbery in the last chapter of this collection). Boag also considers the claim that, phenomenologically, we only ever have a single frame of reference, and replies that the illusion of unity may well be the result of multiple knowers all operating via the single body and the single set of perceptual apparatus. Next Boag addresses Gardner’s (1993) thesis that plurality in terms of motivational sources within a single person does not entail a plurality of parts which function like agents, but reduces to a Humean bundle of conflicting desires. Boag’s response is to point out that motivations must operate through the organism’s perceptual apparatus, that any (even a single) “agent” must have its own source of motivation, and that a “desire”, being a relation, requires a *desirer*. Desirers must have their own properties, and be specified independently, otherwise we risk falling into the conceptually flawed practice of defining them only in terms of their

objects, and postulating them in an ad hoc way to account for any possible behaviour.

Drawing together these points, Boag identifies three logical requirements for a coherent and acceptable strongly partitive theory: that a knower must possess its own intrinsic properties and cannot be defined in terms of its relations; that an account must be provided of how the knower is related to cognition and motivation; and that a plausible account of the causal origins of the knowers must also be provided. Against these criteria, Boag contrasts Pears's (1984, 1986) strongly partitive theory with that of Maze (1983, 1987), and finds that only Maze's account meets the criteria. Here, the candidates for the subject terms are the Freudian instinctual drives, which can be defined deterministically, in terms of their physiological sources, are *psychobiological* in the sense of being connected to the organism's perceptual apparatus, and have causal origins explicable via evolution.

The relevance of the issues discussed in Boag's paper lies in the fact that they challenge some widespread misconceptions in mainstream psychology: that we are comprised of a single, unified knower or "self"; that beliefs are sufficient to explain the direction of action; that conflict is resolved via decisions made by a non-motivated, rational agent; and that drive theory is inadequate as a basis for the explanation of human behaviour.

The perceived inadequacy of the concept of drives is taken up in the next chapter (21) by Doris McIlwain, in *Rezoning Pleasure: Drives and Affects in Personality Theory*. McIlwain's point of departure is the observation that, in personality theory (as in psychology generally), affects and emotions are becoming increasingly more accepted and considered "research respectable", whereas drives are being deleted, not only from mainstream psychology but also from psychoanalytic theory. McIlwain explores how and why this has happened, and why it is important for psychology to include drive theory as part of its motivational package. She then proposes, via Westen

(1997) a “lean mean motivational model” of personality development which combines a view of drives based on the early Freud with a differential affect theory indebted to Silvan Tomkins. She argues that mere rejection of Cartesian dualism in favour of embodiment is not enough; one must take embodiment seriously, and spell out exactly *how* it is relevant. Once that is done, it becomes clear that drives are indispensable. Re-including drives moves us towards a more fully embodied, determinist, scientific theory of mind and motivation. She then applies her model to a number of case studies in personality theory (narcissism, psychopathy, personality disorders, etc.), showing how these cases can be accounted for via the “cascading constraints” of genetic-environment interactions combined with different developmental paths based on co-assemblies of drive-affect and affect-affect relations.

The value to realist psychology of drawing upon psychoanalytic theory is a theme found in Anderson’s writings (1934, 1940, 1953), and reinforced by other authors in the present collection. McIlwain shares Anderson’s views on the central role of affect, although she does not follow him in nominating the affects or feelings as the subject terms of the cognitive relation (1934; see also McMullen, 1996) preferring instead to adopt the view (taken also by Maze, Petocz, Boag and some other authors in this collection) that the drives are the knowers. But, like Tomkins, McIlwain accords equal motivational status to affects in personality theory. Of particular relevance to the present book is the way in which McIlwain shows that, in the rejection of drive theory, the confusions and errors in mainstream psychology are exactly the same as those to be found in many post-Freudian developments in psychoanalysis.

For example, in attachment theory, which is something of a “flavour of the month” in contemporary psychological theory and research, much explanatory weight is given to internal schemas and working models, and lip service is paid to the importance of evolutionary mechanisms promoting attachment behaviours. Yet the

“messy pleasures of the body” and the roles of drive and affect within those schemas or working models are left out of the picture. Similarly, many contemporary accounts of affect, emotion and motivation are simply cognition in superficial disguise; the Cartesian rational mind retains its long privileged place in the explanatory landscape.

With respect to the erosion of drives from psychoanalytic theory, the move away from a classical Freudian paradigm to object relations, self psychology and intersubjectivism is based on the same errors, and motivated by the same ideological commitments, that are found in mainstream psychology. First, there is the pseudodichotomy of drives versus relationships. Drives are seen as blind, biological, noncognitive urges, seeking only the non-social pleasures of the body, unable to be modified, and forever disconnected from “higher” social and cultural activities. Conversely, intimacy and relatedness are seen as independent of drives and not underpinned by bodily needs. However, as McIlwain points out, Freud saw drives as malleable in terms of what elicits them and in their manner of expression, and as necessarily related to objects/people, thus precluding a simplistic biological reductionism. Second is the firmly entrenched view that cognitions, beliefs or self-structures can be motivational in themselves, and so can adequately *replace* drives as motivational sources. But, as McIlwain argues, “we cannot live by meaning, transference and intersubjectivity alone”; cognitive and self structures, scripts, schemas, narratives, scenes, etc. are all underpinned and shaped by drive-related interests and needs, and cannot stand as *alternatives*. Connecting these two errors is the desire to rescue motivational theory from the bonds of determinism and naturalism, which are perceived to diminish our humanity.

In general, then, the themes explored in McIlwain’s paper speak directly to the unrecognised sources of difficulty in much of contemporary mainstream psychology. Primary is the almost universal lack of an adequate theory of motivation, and failure to understand

what would be required within a deterministic, scientific psychology. Mainstream psychologists would insist that they support realism, naturalism and determinism; yet they do not carry this through in their treatment of motivation. In continuing to elevate cognition above motivation, to the extent of offering theories of motivation that are little more than cognition-plus-free-agency in disguise, mainstream psychology's crypto-Cartesianism and convergence with humanistic theory is evident. Ironically, as McIlwain shows, developments in psychoanalysis have followed the same course, and for the same humanistic moral reasons - to *replace* a determinism of drives with an autonomous, teleological "self" or "self-structure" disconnected from mere bodily needs and pleasures. Consistent with a thoroughgoing realist perspective, McIlwain shows the "explanatory muscle" of an "honest metapsychology" which includes a deterministic theory of motivation, based on argument and evidence, rather than on fashion and ideology.

The realist, determinist approach to motivation includes causal roles for both desires and beliefs/cognitions. In the next chapter (22), *A realist Account of Mental Causation*, Sharon Medlow takes up the question of how the realist *externalist* theory of cognition can account for the causal efficacy of cognition. Specifically, if cognition is *not* an internal brain state, but, instead, a *relation between* brain state and situation in the environment, how exactly can that relation as a whole play any causal role? How can relations be causes?

Medlow's paper fills a gap in the realist literature, because, as she correctly notes, a clear account of mental causation has not yet appeared. She addresses specifically Maze's (1983) attempt, in which he appeals to the brain's "relational properties" to account for mental causation. This appeal betrays an assumption, almost universally shared, that it is an object's possession of *intrinsic* states, properties and processes, and not its standing in relation to other things, that makes it causally efficacious. Therefore, even if cognition is a relation, it is the internal properties and processes of just one term of

that relation, the brain, which are the causally efficacious aspects of cognition.

But this would mean that, strictly speaking, the fact of the brain's standing in relation to environmental situations would then be redundant in the causal story. In mainstream psychology, this problem is mistakenly thought to be avoided by an *internalist* view of cognition. But internalism cannot escape the need to connect the internal brain state with the external world (i.e., the objects of cognition), and, as discussed in several of the earlier essays, all attempts to accommodate this result in accounts that are conceptually flawed and so cannot possibly be correct. Externalism must be the way to go. But, as Maze's appeal to "relational properties" shows, there is a strong temptation to smuggle internalism back in when it comes to explaining mental causation. Medlow argues that, apart from the fact that the concept of relational properties is unsound, Maze's appeal to them leaves his account open to the charge of epiphenomenalism, which he is explicitly at pains to avoid.

In evaluating critically the concept of relational properties, Medlow draws upon one of the major principles of realism, the distinction between relations and their terms. She argues that an object's relations cannot be its properties, for properties do not imply the existence of anything beyond themselves, whereas relations do. So, the error in claiming that an object has relational properties is a matter of confusing situations that extend beyond that object, and necessarily involve other objects, with internal states that belong exclusively to that object itself. Hence, Medlow emphasises the realist point that, cognition being a relation, the brain is necessary but not sufficient; the brain's properties and processes are "foundational" to its cognitive relations, in the sense that it could not enter cognitive relations without having those properties, but the relations are external to the brain's intrinsic properties. In *mental* causation, therefore, as opposed to *physiological* causation, it is cognitive relational situations that must be causal.

Medlow then proceeds to develop an alternative realist account of mental causation. She does this via three steps. In the first, she addresses the “locality assumption”, which underlies the belief that externalism and behaviour-causation are incompatible. This is the assumption that what happens at the causal nexus is local, proximate and intrinsic. She argues that, even if we accept that causes must be spatially and temporally contiguous with their effects, we need to recognise that properties and events are themselves extended in space and time, and that, in causal situations, only parts of those situations actually ever come into direct physical contact. She uses the example of a window’s being broken (effect) by a brick’s having been thrown through it (cause). Strictly speaking, only one surface of the brick comes into contact with one part of the window, yet we do not conclude that only the properties located on *that* surface of the brick were responsible for breaking the window; instead, the cause is a complex situation which includes the brick’s relevant properties, its rate of movement, etc.

Mainstream psychology has long followed the behaviourists’ insistence that if psychology is to become a respectable science it must restrict its causal explanations to observable behaviour. In the second step in her argument, Medlow revisits the definition of behaviour, exposing psychology’s failure to provide a clear and coherent account of this central variable. She demonstrates that even the behaviourists failed to acknowledge that behaviour involves not just bodily movements, and not just outcomes of those movements, but the guiding of the movements by the organism’s beliefs about consequences of movements. Only in that way can we distinguish behaviour from accidental consequences of movements. Hence, behaviour is itself a causal process in which cognitions play a causal role, and which is extended spatio-temporally such that it begins and ends in environmental events, at some stage involving processes internal to the organism.

In the third step in her argument, Medlow returns to the main question - the role played by cognitive relational situations in the causation of behaviour. Her account draws upon the realist thesis that causality involves a network or field, rather than a simple linear sequence; it involves a three-term relation (A's leading to B within situation C), rather than a two-term relation (A's leading to B). She then expands another theme discussed in earlier essays, the relation between cognition and motivation. Part of the causal field is the organism's motivational state, understood in terms of the operation of instinctual drives which motivate the organism to move. When combined with the organism's knowledge about situations in the environment and the likely consequences of certain movements, these lead to behaviour (movements guided by cognition about movement-outcomes). Medlow emphasises that this is a deterministic, nonteleological account; an organism does not *choose* to act in accordance with its beliefs in order to satisfy its drives; instead, an organism's knowledge of environmental situations, when combined with the organism being in a particular drive state, *causes* the organism to behave.

Medlow addresses the objection, often raised, that this account provides insufficient detail (e.g., of brain states, neurophysiological processes, etc.); that is, it does not address the neural mechanisms of mental causation. Her response is to reiterate the realist point that such processes pertain only to the subject term of the cognitive relation, and cannot provide information about mental causation itself, because cognitive relational situations cannot be reduced to their smaller components. Indeed, this point holds for any causal sequence; once we have identified the kinds of initiating event that reliably bring about certain kinds of effect within certain kinds of causal field, we have explained the causal process in question. And, since cognition is a spatio-temporally extended relation that obtains between organisms and environmental situations, realism locates human mentality in the natural world, and sees no ontological dif-

ference between mental causation and other types of causal events, which also involve relational situations.

Medlow's paper thus brings a realist account of mental causation into connection with other realist themes: mind as relation; the distinction between relations and the objects related; the characterisation of the subject term of the cognitive relation as a set of instinctual drives; causality as a network or field; and the locating of human mentality and its causal role within the natural world.

In the final essay of this collection (Chapter 23), *Drive Theory Reconsidered (Again!)*, Glenn Newbery continues with the themes of the previous two chapters: objections to drive theory and the question of mental causation. In some of the earlier papers, the importance of motivation in a realist psychology was emphasised, and Maze in particular argues (Chapter 6) that a properly conceptualised drive theory—of the sort that Freud set out, albeit tightened up conceptually—can satisfy scientific, realist requirements and say something about the primary term of a cognitive relation. However, as McIlwain discussed in Chapter 21, not only has psychoanalytic theory come to be widely rejected in academic psychology (see also Petocz in Chapter 16), but the kind of motivation theory that Freud offered, a nonteleological drive theory, has been rejected even within psychoanalytic circles. Within mainstream scientific psychology, there is something paradoxical about the rejection of psychoanalysis on the grounds that it is not scientific, and the replacement of deterministic drive theory with cognitive motivation theory, which offers teleological explanations involving independent psychological needs and an agent-like self which co-ordinates actions and chooses to behave. This popular and influential cognitive approach to motivation has dominated mainstream psychology for over half a century. Newbery examines it critically, via considering its two major objections to drive theory.

The first is the philosophical objection that drive theory, being mechanistic, cannot accommodate the causal role of higher mental

processes, that is, it cannot explain behaviour produced by such things as reason, forethought, intention and choice. Newbery's strategy is to show that, on the contrary, it is cognitive motivation theory that cannot offer a coherent account of mental causation, because it is implicitly committed to the antiscientific theses of dualism and free-will. Newbery shows that proponents of the cognitive approach want cognitions (and, significantly, only *conscious* ones) to play a role in the determination of behaviour, but they do not want to locate these within a properly deterministic system. He exposes the explanatory emptiness of the teleology and mysterious causal powers of the self-as-agent that cognitivist accounts posit, as well as the dualism that underlies them. Thus the theses that a properly determinist theory of drive does exclude, such things as the disembodied, rational, uncaused self, are deservedly rejected because they are incoherent and unscientific. However, drive theory *can* accommodate mental causation. Here Newbery deals with some of the difficulties in setting out a clear account of mental causation in a realist, determinist, and scientific account of behaviour. He concedes that Freud's later formulation of the ego as a set of control functions suffers from the same problems which are to be found in cognitive motivation theories. But Freud's earlier concept of the ego as a subset of the drives (the ego or self-preservative instincts) is sound; hence, the knowers, the subject terms of the cognitive relation, are the instinctual drives. Contrary to the mistaken view of the cognitive critics, drives *do not bypass* thought processes on their way to triggering action. Drive-structures are cognisors; they operate according to the "reality principle" (via reason, cognition, perception, exploration of the environment, etc.) in the service of the "pleasure principle" (satisfaction or gratification via consummatory activities and their experientially determined elaborations). As to the question how cognitions can be causally efficacious, Newbery follows Medlow in being critical of Maze's appeal to the "relational properties" of the organism. Instead, given that *all* causes are situations extended in space and time, *cognitive relational situations* are perfectly respectable candidates to be causes.

Newbery then considers the second objection to drive theory, which is based on experimental observations that organisms will engage in, or seek, activity even though each of their primary drives is (supposedly) satisfied. Here, Newbery argues that, since the perceptual-cognitive apparatus operates in the service of the primary drives, it is important to distinguish between a drive's being *satisfied* and a drive's being *inoperative*; drive satisfaction (as, e.g., in the case of a hungry person who has finished a meal) does not preclude continuing drive *operation* (perceiving and acting on relevant information, as when the hunger-satiated person is caused to stockpile food supplies on hearing that there will be a shortage). Hence, the critics' view that a person in a drive-satisfied state would simply cease to engage in *any* behaviour at all rests on a misunderstanding of drive theory.

Newbery completes his paper with a discussion of the difficulty of establishing empirically the nature and number of primary drives, even if they are defined deterministically in terms of their physiological sources. He argues that an alternative path - a path suggested by Freud himself - is to consider the question from the point of view of biology and evolution, according to which the environment has shaped the organism's motivational structures. Thus, the set of *phylogenetically primary* drives can reliably be identified in terms of those behaviours which are necessary either for the survival of the individual, or for the survival of the species to which the individual belongs. These drives have evolved in the service of the organism's basic needs. However, not all basic needs have led to the evolution of complex drive structures, because that depends on the environmental contingencies concerning the availability of basic supplies. This approach would also explain a number of evolved action patterns (e.g., the various forms of attachment behaviour) which are *innate* but phylogenetically *secondary* (in the sense of having evolved in the service of the primary drives).

Newbery concludes that, when revisited along these lines, drive theory *is* adequate to account for the observed complexities of behaviour, by providing a logically sound and empirically plausible case for motivation-cognition connection and derivation. Specifically, drive theory “provides an account of (i) how the primary biological drives are perceptually and cognitively based structures, which have evolved in the service of basic and indisputable biological needs, and (ii) how the purportedly independent psychological needs, whose primitive forms are evident even at birth (i.e., they are *innate*), would be expected to have evolved *in the service of* the primary biological drives, and so *not* be disconnected from them.”

It is fitting to end the collection with Newbery’s paper. First, it reinforces the case which has been presented throughout the essays that mainstream, realist, scientific psychology is neither consistently realist nor genuinely scientific. Secondly, it reinforces the value for scientific psychology of mining (and conceptually polishing) the rich material that is to be found in Freud’s psychoanalytic theory. Newbery exposes the crypto-Cartesianism and the scientific untenability of the dominant cognitive motivation theory in psychology, at the same time showing that its objections to drive theory are impotent and that the concept of drive contradicts only those aspects of contemporary models focusing on beliefs, needs, wishes and intentions which are in any case at odds with the principles of science. Finally, by elaborating on the distinctions between drive-satiation and drive-operation, and between primary drives and basic needs, the paper illustrates how realist work can clarify central psychological issues and extend them into new theoretical and empirical directions.

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