

FUNDAMENTAL PROBLEMS WITH FUNDAMENTAL RESEARCH:
A META-THEORY FOR SOCIAL PSYCHOLOGY

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The object of enquiry

The first requirement of any science is to have something to be *about*. In a realist conception of science, if not in an instrumentalist one, there has to be some entity, process or structure, or a whole domain of such things, that each discipline or specialist field sets out to document. If a science is to engage in more than just critical description (Harré and Secord, 1972), it must deal in more than just observable surface phenomena, and the patterns or relations that exist between them, including 'cause and effect'. This must seem rather obvious and on the whole unproblematic. After all particle physics is clearly about fundamental particles; molecular biology is about the molecular constituents and interactions of living structures, and so on. But turning to a more difficult case, what is social psychology about? If psychology in general is the study of the brain, or brain functions, or the mind, there would have to be a social part, a social brain or social mind if you will, for social psychology to devote itself to, and there does not seem to be. There are very good reasons to divide both the anatomical map of the brain, and the functional sub-divisions of brain activities (which might loosely be called the mind, without wishing to imply the existence of a second entity, distinct and independent of the brain) into areas designated visual perception, motor-control, and so on. However there do not seem to be similar reasons to designate one part of this system as social. However one conceives of the brain and/or mind, there does not appear to be one particular part or process that should have "social" written on it.

Turning to a different possibility, many people would say that psychology in general is about behaviour, in which case social psychology can quite straightforwardly be about social behaviour. Except that behaviour *per se*, whether social or otherwise, is only a surface phenomenon, a part of ordinary common experience, and the real questions it poses do not concern its nature

so much as its origins. On the whole we are not so much puzzled to know what human social behaviour is like, as to know *why* it is that way. And so it will not do for social psychology to concern itself with social behaviour as such. Likewise the natural sciences which appear to deal with the behaviour of particular materials or physical structures for instance, are seldom content just to catalogue their overt properties, and are much more concerned in the main, to discover from which underlying constituents or features these properties come. Observable behaviour is often the test of a theory, but what the theory attempts to describe is the origins of that behaviour, rather than the behaviour itself. Taken literally, the science of behaviour, or of social behaviour, is a very unscientific idea. As Chomsky (1963) put it: "As a general designation for psychology, behavioral science is about as apt as meter-reading science would be for physics". This brings us back to thinking of social psychology as the study not of social behaviour itself, but of its origins. Now, its origins could in some cases be predominantly social (or perhaps the word 'cultural' is less ambiguous) in which case the social sciences could claim the topic without reference to psychology; or else its origins could be largely to do with the psychological properties of individuals, but in that case the issue is a psychological one, which is in no strong sense *social* psychological. In essence the argument is this. The psychological explanation of a certain aspect of behaviour does not have to invoke something special called social psychology, just because the behaviour to be explained is *social* behaviour, anymore than a special meteorological psychology would have to be created to explain why someone puts up an umbrella, or an automotive psychology to explain how someone drives a car. Human psychology confers many capabilities upon us, and manifests itself in a wide range of activities. We do not have a separate apparatus for every activity we carry out, and so it would be pointless to have a separate division of psychology corresponding to each activity. It is much more likely that we use much the same psychological apparatus whether we are driving a car, or playing tennis with a friend, and the fact that one is considered a social activity and the other is not, is irrelevant to any kind of psychological explanation.

Product and process

However, this is not a paper about definitions, and the real issue is not just the specification of social psychology and its subject

matter. That is only a specific example of a broader argument about research in general and psychology in particular. The important point is that one cannot pick out a sensible category of processes for study, by working backwards from a category of phenomena and looking for its sources. Apparent kinds of explananda do not (necessarily) pick out natural kinds of explanantia. That may need some explanation. There is a view of science, called *instrumentalism*, which holds that directly observable objects and events are the only sure realities, and the other ideas and concepts by which they are linked in scientific theorizing are useful (hence 'instrumental') bridges by which to make connecting inferences, but no more than that (see e.g. Clarke, 1983a). These linking concepts are called intervening variables. The opposing view of science, which is called *realism*, holds that there are real objects and processes beyond our powers of direct observation, and that part of the job of science is to describe them. For scientific realists then, it is possible, although not necessarily the case, that the linking concepts in a theory, the hypothetical constructs by means of which one observation can be predicted from another, correspond to real but as yet unseen entities and processes.

This is what might be called the 'two-layer view' of the world. On the one hand there are the directly observable things that make up the body of ordinary experience. These are the 'surface phenomena' of the world, and they are what stand to be explained. Usually they are assumed to be the result or product of some unseen or unfamiliar process, whose discovery would provide the explanation. This is the second layer of the realist's view of things, and it is in this second layer that realist sciences attempt to operate, not in the first, which serves only as a cross-check to ensure that the mapping of the process layer has not gone astray. These two layers correspond to the conceptual and perceptual planes of scientific reasoning picked out by McQuorquodale and Meehl's (1948) definition of hypothetical constructs and intervening variables.

In realist sciences there are a number of known phenomena - things that require explanation (explananda), and a number of known causes, processes or templates which make up the domain of explanations (explanantia). Furthermore, it will be known in *some* cases but not all, which phenomena originate from which sources.

As such sciences develop new phenomena will be discovered, together with new source processes, and new connections of source processes with the phenomena they produce. On the face of it, a major mode of scientific development would be the

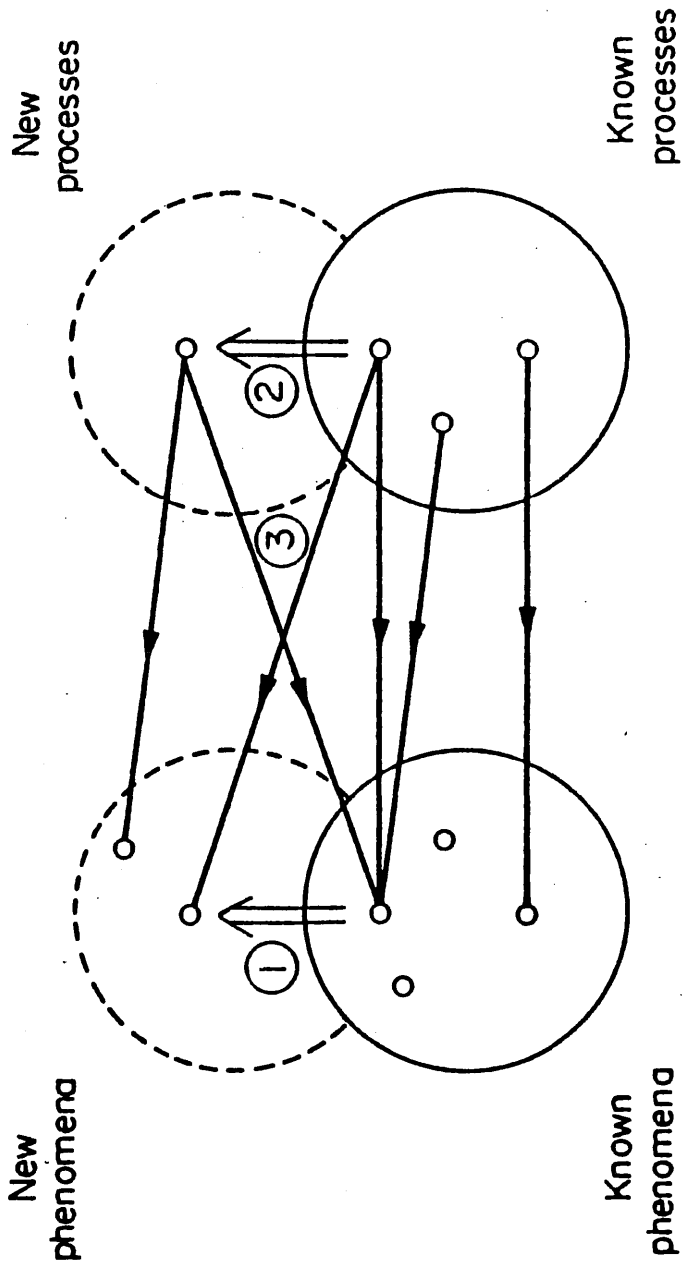


Figure 1: Sets of known and unknown phenomena and processes (from Clarke, 1983b).

selection of new (that is as yet unresearched) phenomena, which would then be studied to find their causes, or more generally their origins. In this way social behaviour, or particular types of social behaviour could be selected as topics, and traced back to the psychological processes responsible. In many cases, this is just what research projects in social psychology set out to do. This is how their purpose is conceived. But it is just this apparently sensible point of departure that leads to many of the problems. Firstly, sensible and familiar categories of phenomena do not correspond one-to-one with sensible divisions of the underlying process. There is a token - token correspondence between product and process, but no guarantee of a type - type correspondence. For instance there is no "physics of carpets" at least as a recognized and sensible branch of physics.¹ Of course every *particular* feature of every *particular* carpet is grounded in *particular* physical and chemical processes, which could serve as explanations for colour or toughness (that is the token - token correspondence), but no coherent branch of physics is picked out by the idea that carpets, and all the processes that go to make them what they are, taken as a set, should be a branch of the physical sciences in their own right. (There is no correspondence between the *type* of product, carpets, and the *type* of physical process which gives rise to their properties.) In the same way the seeking of psychological explanations for social explananda may not form a coherent and profitable topic for a branch of scientific psychology.

The second problem is that new phenomena do not have to originate from new processes. Very often it will be the case that a new phenomenon, once explained, will be found to originate from well known source processes. It would usually be a waste to expend one's effort accounting for type *D* social behaviour, only to find that its explanation was as for types *A*, *B* and *C*, which had been adequately studied already. One of the main pitfalls of starting with a surface phenomenon and looking for its source process, is that the process when found may be uninteresting. Data-driven or inductive research is quite likely to come up with unremarkable explanations. However theory-driven (or roughly speaking what used to be called hypothetico-deductive) research starts by choosing those theories which are interesting enough to matter whether or not they are true, and if that can be established by gathering unremarkable or common-place data, then so much the better. It is a bad thing if data-driven research leads to mundane theorizing, it is not a bad thing if theory-driven research leads to mundane data-collection.

This, incidentally, is also one of the main limitations of applied research, or at least of the version of it which is currently so fashionable. Real applied research, as its name suggests, consists of research findings, produced by appropriate scientific procedures, and drawn from the second or source-process layer of things, which have turned out to be applicable to the solution of practical problems. Fundamental research in medical science, and its application in medical practice is a good example of this. Its main thrust, and the source of most of the practical benefits, has been fundamental research with practical applications. However, what passes now for applied research is not the same thing at all, in many cases. Researching a practical problem directly, as if each cluster of events in the world which we find a nuisance must correspond with a distinct and coherent set of causal processes which can be discovered, is only to commit one of the more insidious inductive fallacies. Although not doomed to failure, this conception of 'applied research' is severely limited by all the disadvantages of doing science 'the wrong way round', starting from the *product* and going on to look for the *process*.

The third problem is that products do not generate, and therefore do not entail, the underlying processes - quite the reverse in fact. Selecting a product to study does not provide any path to the source process, as has been pointed out many times in critiques of inductive methods, and in the maxim that theory is under-determined by data. From a chosen process, the products can be inferred; but starting from a chosen product, all that may be expected is a critical description, and that is not even necessary for, let alone sufficient as, an explanation. The paradox that the scientific method addresses and overcomes is that we could easily discover what we already know, given the things we wish to find out, but there is no straightforward way to discover what we want to find out from the things we already know. This is why the scientific method has its apparent twist or 'back-to-front' quality. It has to proceed by imagining the answers that are wanted, from which one can infer the surface realities they would entail. If they do not match with observations the theory was wrong; if they do match, the theory might (but only might) be correct. The data (meaning literally the 'givens') are not the starting point. They are the end point, where the cycle of reasoning is closed successfully or unsuccessfully.

For all these reasons the predominant mode of advance in the established sciences has not been the selection of new phenomena for which explanations might be sought (arrow 1 in figure 1) but the conjecture that further processes might be at

work, whose products could be sought by way of evidence, or better still in Popper's (1972) view, 'refutation' (arrows 2 and 3). Theory-led advances were strongly preferred by Einstein, who held that experiments can sometimes test a theory, but no path can lead in principle from observation to the development of a fundamentally new theory. For such people the 'onion layers' of scientific understanding have to be peeled away from the inside outwards; not from the outside inwards. This odd seeming metaphor captures the apparent reversal inherent in scientific logic quite nicely. The most scientific way to peel an onion really is to start in the middle and remove one layer at a time until you reach the outside. In the case of scientific research the center of the onion is reached at the outset by a leap of the imagination. The step by step removal of layers is the stepwise progression of inference and calculation which follows, leading to the outside of the onion - the directly observable empirical consequences, which should come to the surface in the expected place, unless the original imaginative jump had missed its mark.

This might all seem like a quite unnecessary rehearsal of familiar scientific ideas, but unfortunately there is a great deal of social psychology which, for all its trappings of quantification and experimental procedure, does not fit the logical structure of the other sciences (Harré and Secord, 1972). What is more worrying is that this basic scientific logic is not merely unknown or unheeded in social psychology; it is largely inapplicable because of fundamental misconceptions in the field of social psychology itself, which put it out of reach of the scientific status it aspires to so keenly.

Consider, then, the effect of asking a rather different question from that which typifies much of social psychology: not "what is the origin of such and such a type of social behaviour?" but rather "given the psychological processes we believe to be at work, what additional ones might there be, which have yet to be accounted for, and by what accessible manifestation might we check their existence and properties?" Particularly we might ask after the *fundamental* processes or principles which do not merely add to the list of explanantia but provide the basis of it, as for example an understanding of DNA has done in biology, as tectonic plates have in geology, and the structure of the atomic nucleus has in physics. Such things are not just arbitrary items from the explanatory schemes of their respective disciplines, but core concepts around which large areas of knowledge are organised. Here again there is an interesting discrepancy between the progress of the advanced sciences and that of social psychology. In physics and chemistry for example,

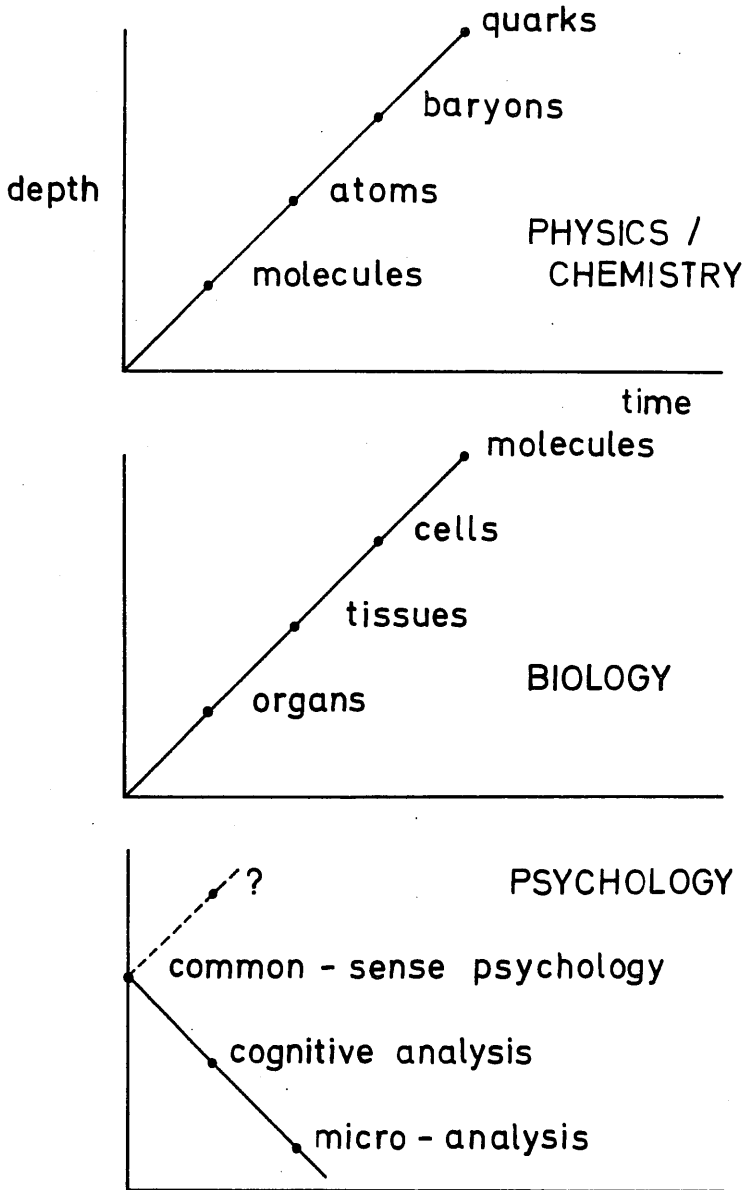


Figure 2: Progression towards or away from deeper entities in different disciplines.

or in biology, there is a clear sense that the further researchers get from ordinary common-sense concepts, the more fundamental their understanding becomes. This is shown diagrammatically in Figure 2.

As the physical sciences move from the study of molecules, to atoms, to baryons, to quarks; or the life sciences work from organisms, to organs, to tissues, cells, organelles and molecules, there is a clear sense that increasingly basic mechanisms are being uncovered. However, the apparently similar progression in social psychology from common-sense conceptions of action down to increasingly microanalytic studies of social behaviour seems to take us further from ordinary concepts, but also further from any real explanatory fundamentals. It is as if we are moving away from the original conceptual schemes but *in the wrong direction*. In a companion paper,² I suggest that there are theoretical grounds to expect this, and ways to remedy it. The reason briefly, is that we perceive the *natural world from the outside* and our investigations tend to lead us progressively further into the heart of the matter. Our ordinary introspective self-knowledge, on the other hand, is knowledge *from within*, but is nevertheless not central. Our natural knowledge of ourselves is, as it were, moderately profound. There are unfamiliar things which are both more superficial and more profound than common-sense psychology. Reductive, micro-analytic social psychology has moved away from the common-sense middle ground, but has unfortunately taken the route which leads to more superficial, rather than more profound, aspects of human nature.

Stable explanations

It could be argued that social sciences do not always respond well to the natural scientific notion of explanation. Whereas the surface phenomena of the natural world seem to rest on stable, tractable explanatory mechanisms and principles, into which enquiries can penetrate for quite a way before hitting the conceptual quicksands below - paradoxes of time and causation for instance, the social sciences seem more ephemeral. Surface phenomena may be described individually or collectively, but as soon as any attempt is made to go further towards an explanatory domain, the conceptual quicksands are found to lie *immediately* below the surface. Problems of mind and free choice, consciousness and cultural relativism - these imponderables are encountered at a much earlier stage of the search for explanatory processes, than are the corresponding paradoxes of the

natural world. If it is the case that social and behavioural explanation seems like quicksand, when compared with the 'solid rock' underpinning the natural scientific landscape, then it will not lend itself to once-and-for-all, timeless explanations. The inroads made into it, the tunnels we dig, will soon deform and collapse, and a different strategy must be used for making progress, which does not depend on the establishment of structures which are invariant and permanent. Rather the way of getting about in such a medium as this is to look for points of *relative* stability, relatively higher 'viscosity', against which leverage can be exerted, if only for the time being. The explanatory task of social and behavioural sciences in other words, may not be to relate ephemeral phenomena to timeless fundamentals and unchanging quasi-physical laws, but to relate what is fast moving to what is slow moving, to locate the rapid fluctuations of the social world in the larger and slower progressions within which they occur. This to introduce a theme which will be argued later on different (and better) grounds, that the central explanatory process we should be seeking derives the fast-running fine-detail of behaviour and other events, from the slower and grosser patterns which, far from merely resulting from the accumulation of micro-events, are the sign of major organising principles at work, *by virtue of which* the smaller and faster events occur as they do.

The role of experiments

This is not in the first instance an empirical matter, or not at least an inductive empirical matter. As we have seen before, the process entails the product, but not the reverse. If it is really the case that gross patterns marshal and organise the fine details into the configurations they adopt, then we should not expect observable events to imply their own organisational principles, but rather that the organising principles that are likely and interesting on theoretical grounds could be evaluated by the existence (or not) of the patterns of events they imply. Experiments and empirical studies generally may provide one of the forms of evidence and argument by which structural theories may be evaluated, but in themselves they will only test, not generate our understanding of the underlying sources of organisation. Often it will be the case, as Einstein held, that "the theoretical possibilities in a given case are relatively few, and among them the choice can often be made by quite general arguments".³ Arbitrary hypotheses will not stand in place of

coherent theories in this respect (Newell, 1973). If induction is unsatisfactory, then it is hardly any better to guess at the outcome of a data gathering exercise, and then see if the guess is right, than it is to collect the data with no such guesses and see what emerges. In just this way, much of what passes for theory testing experimental research, is no more than induction in disguise. Furthermore, it is not the case that every proposition must be subjected to direct empirical test. In the advanced sciences propositions are held together by elaborate constructions of inference and calculation, and the whole structure is grounded in observation at certain critical points, but by no means at every point. Again an analogy may help. Science is sometimes likened to a tower built from the ground up, each brick resting on the ones below, and the bottom layer resting on the ground (which represents empirical evaluation). A better analogy might be the rather futuristic tent-like constructions which are now used as display stands at outdoor trade festivals. Their striking feature is the elegant intersection of light fabric paraboloids above the ground, but they are held in place by massive concrete piles to which the lowest corners are securely bolted. The art is in balancing the great spans of apparently weightless structure against the secure anchor points that prevent the whole thing from blowing away. There is no virtue in the extremes. Too much elaboration of the tent and it would blow away in the wind. Too many anchor points positioned closely together, and the entire construction would be no more than a sheet pegged to the ground. Likewise in social psychology, and any other science for that matter, the virtue is not in pegging our ideas to the empirical ground at every possible point, that is only to prevent the theoretical architecture from taking shape. It is too often thought that the more a psychological theory can be grounded in observation the better it is, without limit. On the contrary there is an optimal balance between theoretical spanning and empirical anchorage. Too much of the former and the whole thing may blow away. It may disconnect from its empirical reference points altogether. On the whole though, the field of social psychology is so afraid of this mistake that it rushes to the opposite extreme of only allowing theoretical constructions that are so excessively fastened to their empirical supports as to lose any semblance of a coherent form in their own right.

A network of theoretical ideas linked together by inference and calculation, and tested at key points is a coherent theory, and is powerful precisely because so much can be inferred about new cases on theoretical grounds, without having to establish

every item by a separate empirical test. The commoner situation in social psychology is for a theoretical question to have an empirical answer, for which there is no theoretical reason, and for the answer to lead to the next question or study. That only adds up to an incoherent theory, and a body of knowledge which supports no new chains of reasoning, and hence no real understanding, as the individual elements of theory are only connected by (arbitrary) tracts of empiricism. This is why merely knowing the empirical answer to a question counts for so little in science. Without a theoretical reason to expect a particular answer, the actual answer has no consistent view to support or refute.

Nor is the rigor of empirical evaluation an unqualified virtue. In any exercise or experiment involving equivocal outcomes, there exists the possibility of two kinds of error - to miss what is there and to see what is not. Yet again our discipline has advocated one extreme, whereas a sensible balance would have been much better. Our experimental procedures, and our more general conceptions of evidence and proof, have been so weighted in favor of rigor, that is of never seeming to detect processes which are not really at work, that the opposite error has been allowed to run wild. We have created a discipline which is so extreme in its scepticism, that it could never possibly detect or document most of what is important about human social conduct. For this reason too, we have come up with a version of human mentality which is less, rather than more, profound and useful than common-sense mentalism. Far from being a discipline which cultivates professional and personal sophistication about people, we actually produce a carefully contrived naivety. Because of the extremes of misplaced scepticism by which psychological ideas are judged, psychologists end up believing in (or confessing to) a *less* substantial body of knowledge about human nature than everyone else.

Hypotheses are only worth testing empirically if they are entailed by, and therefore stand to evaluate, more general theories of sufficient stature to *matter* whether or not they are true. This is one reason why the course of science is not as directable as one might wish. It is not really a matter of choosing the questions and finding the answers. It is more a matter of the chain of ideas and suppositions leading where it will, and the experiments and applications fitting in with that as best they can.

An experiment is supposed to elicit the underlying process, or its implications, not to reconstruct the original phenomenon. In this respect the plea for more 'natural' and 'realistic' experi-

ments has been misguided, and has only served to increase the attention given to surface phenomena at the expense of underlying processes. The point of an experiment is *not* to recreate the natural course of events, but to force a process at work behind natural events to show its hand in an unusual and criterial way. If the critical test of a theory lies in the natural course of events then selective observation is all that is called for. If the criterial observation can only be made under unnatural conditions then these conditions are usefully created in an experiment. In that respect laboratory experiments work and are helpful only insofar as they are *unnatural*. Their applicability to other settings does not rest on the apparent similarity of circumstances, but on the consistency of the underlying processes involved. A 'surface experiment' in which a familiar chain of events A - B is investigated to see whether, under controlled conditions, B occurs if and only if A has occurred, is in itself hardly an experiment at all. One of the requirements of an experiment in the strong sense, is that the predictions which it tests are otherwise of low prior probability, not well known contingencies from the everyday world.

Studies which are not initially part of an overall framework will not usually add up to one after the event. Much of the reason for bio-medical and physical research producing a steadily richer and more potent picture of how things work, while social and behavioural disciplines get larger and more fragmented, is that natural scientific studies are part of an organised conceptual framework from the outset, and when completed they contribute to that part of the scheme from which they derived. Their conceptual significance is guaranteed by their theoretical origins, not contingent on the content of the outcome. With social psychological research, on the other hand, studies are often done in isolation from any significant body of theory, and are therefore without a framework to which they could be relevant from the outset, however they turn out. The so-called 'pieces of the jig-saw' accumulate in journals, despite the fact that a *real* jig-saw puzzle can only be made by taking a picture and cutting it up into pieces, not by making pieces and hoping they will form a picture.

It is not possible or desirable to lay down a detailed methodological prescription, since scientific methodology at its best is largely *ad hoc*. It is the specifics of each problem that will determine the specifics of the methods that would solve it. To hold to general *a priori* methodological policies and preferences is unhelpful, as is the practice of research funding agencies to require methodological specifications for a line of research long

before the stage in research is reached when the particulars of the problem become known, and the particulars of the method could be chosen sensibly. Likewise there is something odd about purely methodological projects which attempt to perfect the methods first so as to tackle the real problems later. That is like playing a curious game in which you are asked to solve a puzzle, although you will not be shown the puzzle until *after* you have set out in detail the step-by-step procedure by which you propose to solve it.

Topic domains

The underlying domain of processes in which a science may properly operate is what I will call a *topic domain*. The candidates for such a domain must have certain essential properties, if a systematic account of them is to constitute a satisfactory scientific topic.

Concealment

They must in some sense be concealed, or else they will not stand to be discovered in any strong sense. This turns out incidentally to argue in favour of methodological individualism, since it is easier to see how explanatory processes and mechanisms might be at work, but undetected, within the head of the individual, than it is to see just where in the social or cultural realm they might be hidden. Insofar as something becomes a social object or process in the first place by virtue of its featuring in human action, discourse, morality or conceptualisation, it is hard to see how at the same time it might be as yet undiscovered. It seems that to be social, something must be by definition in the surface realm, and therefore excluded from this research programme. The exception might be emergent and systemic properties of the social order, of the kind found in macro-economics, where public and collective patterns of preference and behaviour escape attention, by virtue of the scale, pace and complexity with which they operate, but for most micro-social processes this is not the case.

Coherence

The topic domain must also be coherent or else knowledge of one part will imply nothing about another. No inferences or chains of reasoning will be possible beyond the unconnected list of primary observations, and no map will be produced on which journeys of imagination and invention can be plotted.

Causal powers

Structures and processes in the topic domain must have causal powers over surface events, or else nothing in the familiar world would be explained by new discoveries, and no empirical test of conjectures would be possible. The area would slide into scholasticism. However, as we have seen it is equally important to avoid the opposite failing, of driving a subject into scientism by over-valuing empirical tests in the surface domain.

Stability

The constituents of the topic domain must be stable, or at least relatively so, when compared with the events to be explained, and with the procedures of discovery. If this were not the case, the findings would be out-of-date before they could be useful, and the very picture itself would be blurred like a moving object photographed with a long exposure, because the pace at which information and evidence for a certain state of affairs had built up would have been overtaken by concurrent changes in the state of affairs itself.

In this connection it is worth pointing out that the concept of a process is being used in two rather distinct senses. One, which I shall call an 'occurrent process' is like a causal chain in which later events arise because of earlier ones. The other, which might be called an 'existent process' means something more like a mechanism - some device or arrangement (possibly in the abstract like a generative grammar) which is responsible by virtue of its constitution for the chains of efficient causation which arise. The latter sense of process is to be preferred as its referents are generally less ephemeral, and better matched to all the criteria listed here, than are the fleeting wisps of history which typify occurrent processes.

Realism

The objects in the topic domain need to be real if the science of their description is not to be relegated to the disappointing status of mere instrumentalism. The power and impact of realist thought in the natural sciences comes across clearly in Medawar (1979), and its implications for psychology in general and social psychology in particular are discussed in Manicas and Secord (1983).

Natural kinds

The topic domain needs to be a *natural* division of object or process types, and further to be sub-divided into other 'natural kind' units for the purpose of investigation. Failure to comply

with this requirement will result in the formulation of pseudo-generalisations, since summary properties will be attributed to sets of cases or mechanisms, whose members do not make up all or only the instances which would have important features in common.

Fundamentals

Finally, the ideal topic domains for research consist of entities and processes which are fundamental to the system being studied. What is of greatest interest are the root causes or processes of more evident events, not merely a few arbitrary selections from the possible explanatory network. This is a property that many natural scientific topics seem to have, such as fundamental physical particles and forces, tectonic plates, the molecular basis of biology, and so on. In social psychology, by contrast it is hard to think of topics for which such fundamental status is even a possibility.

It may be that in social psychology, as in the natural sciences, the stable, fundamental explanantia will only be found by looking for constitutive rather than efficient causes. That is to say we may have to move further from the Humean conception of causal explanations in which events are explained by prior events or conditions, and adopt instead the tendency-opportunity schemata of the advanced sciences (Harré, Clarke and De Carlo, 1985). In this view of explanation what stands to be discovered is the constitution and structure of a system whereby it displays the range of powers and properties it does in the range of circumstances observed. Of course this would imply that social psychology will after all, deal largely in intra-physic processes and explanations, but not of the naive and non-contingent kind that some trait theories would suggest. This kind of intra-psyche explanation would allow people's behaviour, and their psychological nature itself to be as radically affected by changing social circumstances as, say, a computer's output and program are by its input. (This does not stop an account of the program from being a better explanatory model of its behaviour, than a record of its input history would be. The program subsumes both forms of explanation, because it also accounts for the input-output relations of the machine for all distinguishable classes of input.)

All of this is by contrast with many of the standard topics in social psychology (and possibly the concept of social psychology itself) which seem on closer inspection to deal characteristically in surface objects and phenomena, unreal entities, unnatural kinds, observation statements with little if any theoretical foun-

dation or implication, inductive inference from observation to generalisation, and to have no claim to being fundamental.

Suitable topics

On the face of it, the candidates for a satisfactory topic domain might seem to be the following: the structural and causal properties of the outside world, of which a special case would be the social environment or 'society'; 'content behaviourism', which means the description of particular networks and relations linking thoughts, feelings, actions and circumstances, a category which subsumes much of present day social psychology; the more abstract forms of behaviourism - instrumentalist laws of learning and reinforcement, generalised over a wide variety of specific behaviour and situation types; the nature and organisation of conscious experience; the 'software' of the brain, which might include cybernetic, artificial intelligence and functionalist topics and modes of enquiry - a conceptual strategy very like that of studying the parts of a circuit in terms of their functions, except that this is rather more abstract, and deals with the overall function of a system reduced to *its* parts or sub-functions; and lastly the 'hardware' or physiological description of the brain itself.

The functionalist point of view is a particularly interesting and important one for psychology, and it represents the mode of explanation used in most of modern cognitive psychology. Its clearest parallel is seen in the ways that engineers study computers and other complex electronic systems. Clearly if enough is known of the actual machinery, its individual parts or sub-systems can be identified and each described according to its function. Interestingly, much of what passes for the nomenclature of components in electronics, *resistor*, *inductance*, *amplifier*, *capacitor*, and so on, does not pick out what each component is, much less its specific nature and construction. These are labels for components according to what they *do*. Even the apparent description of what the pieces *are* is offered in categories of *function*. The really interesting twist is this. Even without knowing the physical construction of the device, a similar sort of analysis, a kind of abstract wiring diagram, can be drawn up, showing how functions are carried out by particular configurations of inter-related sub-functions, and so on. It is as if the function of the parts, and the parts of the function are equivalent and interchangeable explanatory vocabularies, and one can work with the latter without the need for additional

and supporting investigation of the former.

In real computers, and to an extent in real brains, there is a continual reassignment of part to different functional units, so in principle there may never be a simple and invariant relation by which structural (in the sense of neuroanatomical) and functional studies can support one another. This suggests that structural and functional studies will create self-contained explanatory domains, but with surprisingly little to say about each other because of the complexity and variability of the mapping rules between them. This is just what has happened in the case of computer technology, where the hardware and software disciplines have each advanced in their own right, with remarkably little to say to each other. It is possible that psychology will go the same way. Studies of brain, and studies of mind (in the functionalist sense) although metaphysically equivalent but differently formulated descriptions of the same thing, may for most practical purposes operate best as independent and largely unrelatable enterprises.

The conception of psychological enquiry as being the study of what goes on in people's heads from a broadly functionalist, cybernetic or 'software' perspective has been gaining ground in (especially cognitive) psychology, with much additional impetus provided by developments in computing and artificial intelligence (e.g. Turing, 1950; Ashby, 1952; Gregory, 1961; McFarland, 1971 and 1974; Minsky, 1972; Newell and Simon, 1972; MacKay, 1972; Segal and Stacy, 1975; Anderson, 1976; Alexander, 1977; and Fodor, 1981). The writing of behaviour grammars (Westman, 1978; Clarke, 1983b) and story grammars (Mandler and Johnson, 1977; Schank and Abelson, 1977; and Thorndyke, 1977) allows this approach to be applied directly to the analysis of social action and the (hypothetical) procedures of its production and interpretation. It is now widely agreed that this conception of the psychologist's task is the best available, but even so it has its limitations. Dreyfus (1972), Weizenbaum (1976) and Wilkes (1981) argue that it cannot stand alone as a complete and adequate psychological picture without additions from the more humanistic and more biological fields which straddle it.

Of all the alternative topic domains enumerated above, only the last two satisfy the criteria set out previously. The nature of the social world and the stream of consciousness, for example, are surface phenomena; while content behaviourism and abstract behaviourism are instrumentalist versions of the one-level world view, in which no recourse is made to real underlying explanatory mechanisms. These latter fields deal in occurrent rather than existent processes, and explain behaviour much as one might

explain the presence of daylight by saying that day follows night, and it was recently night, not as one would explain it by saying that the earth orbits the sun while rotating on its axis - that is to say by means of a 'constitutive' explanation in which the behaviour of the system is accounted for by its constitution.

Unfortunately most of social psychology at present is accounted for by a style of investigation which I call 'surface networking', in which pairs or groups of surface phenomena are selected and their inter-relations ascertained. Typical questions are 'Does A correlate with B?'; 'Does C cause D?'; 'Do people with more of E do more of F?'; 'Do people with characteristic G tend to think H?'; 'Is behaviour I seen under circumstance J?'; and so on. This is not only the commonest but also the worst version of the subject one could choose. Not only does it fail *all* the criteria for a satisfactory topic domain by dealing in surface phenomena, in incoherent assemblies, with no causal powers over further domains, and with ephemeral, instrumental, arbitrarily divided and non-fundamental events and relations, but it is also a never ending task. There are *infinitely* many (arbitrary) ways of carving up the domain of ordinary experience. There are therefore infinitely many correlations, contingencies and other relations which could be found between the resulting categories, and if they were all studied carefully for infinite amounts of time they would add up finally to nothing in particular. They are a mass of incommensurable, irreconcilable conceptual vignettes, which are not drawn from a more general scheme and could never be assembled into one. This is far worse than saying that all science is infinite and discovery could probably go on for ever. In well-formed disciplines progress goes on indefinitely from shallower to deeper forms of understanding (by and large). Empirical surface networks, on the other hand, only go round in circles for ever. They do not produce a steady cumulative progression of anything. They merely sprawl across the academic landscape.

If we adopt this line of argument, and resolve to pursue a form of explanation for human conduct dealing in the legitimate topic domains of brain 'software' and 'hardware' (and further if we should prefer software, for reasons which will be explained shortly), we are led to the question of what parts and interrelations exist within that system of brain software which we are coming to identify with 'the mind'.

The structure of psychology

It might seem that the sub-structure of the mental system could be inferred, at least to the limits of present knowledge, from the substructure of psychology as a discipline, much as one could infer from the organisation of anatomy books and courses, essentially how the functional organisation of the body is divided up. The list of topics that figure in most general psychology textbooks or courses, however, turns out not to be the array of parts from one conceptual scheme - one way of 'carving up the pie' - but the interwoven items from four different and largely unrelatable schemata.

Parts of the mind

The first schema suggested by the typical curriculum, is what might loosely be termed the 'parts of the mind schema', and this contains such topics as perception, cognition, memory, decision making, motor skill, and so on. These units could well be boxes in an engineer's blue-print for a natural or artificial mental architecture, and they are to some extent neuro-anatomically localised.

Global properties

The second schema consists of the epiphenomena, or properties and capabilities of whole individuals - learning, individual differences, psycho-pathology, and the like.

Types of description

The third schema differentiates not the various objects of description, but the possible types and levels of description, such as physiological and mathematical psychology.

Types of subjects

Finally the fourth schema distinguishes populations to which psychology may be applied, such as children, animals or old people.

Interestingly these four conceptual schemata for the infrastructure of psychology, suggest that some of the things we tend to bracket together are really quite different, such as developmental psychology on one hand and the psychology of children on the other, or personality as an individual attribute as opposed to individual differences considered as distributional phenomena.

In which of these schemata would one locate social psy-

chology, say, or the study of long-term relationships? It seems that no distinct part of the mind/brain system is dedicated to social functioning, so the first or 'parts of the mind' schema is a poor fit.

Social behaviour may be part of the second list dealing with epiphenomena, but as we have seen already there are problems if social psychology is regarded as the study of social behaviour. At present, most 'discoveries' about social behaviour are only discoveries in the very weakest sense, precisely because these problems are ignored and occurrences in the surface domain are unwisely taken to be the objects of study. They merely convey to one group of people what is already common knowledge to another group, which can be a useful thing to do, but it is hardly comparable with the real scientific meaning of discovery, which is to find out for the first time what was previously unknown to everyone. A good deal of attitude survey and other questionnaire work is open to this criticism.

The third schema does not fit, as 'social' is not a type of explanation for phenomena as yet unspecified, in the way that 'mathematical' and 'physiological' are.

The fourth schema is inappropriate as 'social' is not an entity on which to practice psychology although, groups, crowds or families might be. That, however, does not save the day since groups, crowds and families are surface entities, and their *processes* are only social insofar as *they* are surface too. Concealed processes in collectives are concealed within the individuals (for want of anywhere else they could hide) and as such are not social by nature, although they may be social by virtue of having exclusively social products. However, that line of argument does not work either. As we have seen, social products do not pick out a natural category of individual psychological process. To pursue the metaphor of psychology as the study of brain software, we really need to pick a sub-function that could belong in schema number one, the parts of the mind list, and social psychology it seems, does not. Ideally there should be a potential unit from schema number one, which exclusively (or at any rate very largely) gives rise to social behavioural phenomena, and which furthermore is *fundamental* to the organisation of social behaviour, rather than being just some arbitrary part of the source process domain but with no *special* importance.

The social part of psychology

If this cannot be found, the original question arises again with a vengeance. What could social psychology be about? To approach the question in a different way let us pose it in a different form. In what sense could part of psychology be social? Clearly this could not be in the sense that cognitive psychology is cognitive (there being distinct cognitive but not social processes in the mental system to act as the topic); nor the same sense in which mathematical psychology is mathematical (in that there are distinctly mathematical techniques for setting up and testing psychological theories, but there are not in the same sense social techniques, whose application marks out their own area of the subject).

There are certain ways in which part of psychology might be social, and they fall into two groups. The first group is best described by the term 'social psychology', meaning that particular *part of psychology* which is social, while the second group of formulations falls better under the heading of 'socio-psychology', meaning topics or areas of study that combine, or lie between, psychology and the social sciences, but do not fall entirely within psychology as specified by a general definition of the field.

Social psychology

Psychological explanations for social behaviour

The first formulation of 'social psychology' is the study of (individual) psychological processes having social products - that is psychological explanantia for social explananda. This is to strike a definite note of methodological individualism for the subject, but is nonetheless the most basic and satisfactory of all the possibilities. Its limitation, as we have seen is the difficulty of finding any natural types of psychological process whose explananda are exclusively social. Person perception, for instance, may be in no stronger sense an example of distinctly social psychology, than the perception of trees would be a justification for inventing arboreal psychology as a sub-discipline.

Social explanations of psychological phenomena

The second formulation would be the reverse, namely the study of social explanantia for psychological explananda, such as the effects of the social world on individual thought, action and

feeling. At best this creates a paradox and at worst a fallacy, as it suggests individual units (people) take their properties from the whole of which they are part (society). On the whole it is easier, although not absolutely necessary, to see influences exerted by the parts on the whole, rather than the reverse. After all, soup is chicken soup if the pieces of meat in it are pieces of chicken; however it does not follow that whatever one puts in chicken soup will become a piece of chicken. To take an example which is kinder to the view that the whole can influence the parts, the trees in a wood will depend for their kind and shape upon the kind of wood they are growing up, as well as contributing to the kind of wood it is by the kind of trees they are. In the same way it has to be admitted that a *mutual* influence exists between individuals and society, and each is influenced by the properties of the other. But that is not enough to overthrow methodological individualism. The dialogue between the individual and society (in itself a surface phenomenon) could be explained entirely by the organising processes in the individual, determining both the things in society to which the individual contributes, and the things in society to which the individual is susceptible. This is a basic property of control systems (of which the brain is an example), and of the way they interact with their environment. A thermostatic heating system changes the temperature of a room, and the temperature of the room changes the state and behaviour of the heating system, but all of the interesting control machinery is in the heating system, not in the room and its temperature. The effect of the heating system upon room temperature, and its sensitivity to room temperature, are both grounded in the design and construction of the thermostat, not in anything to be discovered by investigating assorted rooms, and the attribute of temperature which they display so unremarkably. Society is passive in the way that the room in this analogy is passive - a pool of institutions, buildings, conventions, social practices, and so on, to which individuals both contribute and react. The *processes* of contribution and reaction, however, insofar as they are anything other than surface chains of occurrences, are concealed, if anywhere, in the mental apparatus of the individual. To restate an earlier argument, social processes are surface processes otherwise they could not be social. Non-surface processes are individual.

Individual psychological processes acquired in socialisation

The third formulation would require social psychology to study those psychological processes which are social (than is cultural) in origin. The idea is gaining currency that individual 'selves'

are socially constructed (Harré, 1979; Llewellyn and Kelly, 1980) and that the proper task of social psychology is to analyse the ways in which apparently psychological entities, such as minds or personalities, are produced from the social order. However, this raises the problem of natural kinds again. What would all socially acquired psychological processes have in common? Would they be any more coherent than the set of all beliefs acquired on a Tuesday? Probably not. For something to be a good research topic, it must be not only important but also *importantly different* from other related branches of study. Many pseudo-topics, of the form the social psychology of x , where x is a common category of situation or action, run into just this problem. It may be that x , whatever it is, looks fairly different from y and z on the surface, but that is no reason to suppose that the psychological processes responsible for x belong to a wholly distinct class from those creating y and z , or that they deserve to be treated as a research topic in their own right. This objection can be applied not only to the sub-topics within social psychology, but to the idea of social psychology itself as a distinct and well-formed field to study.

Distributed psychological processing

In the fourth formulation, social psychological processes would be those which are social (that is distributed) in character and location. The ability to build a complex machine may be an attribute of a group but of no one individual. In that sense the psychological source process for the activity of building is going on in the group but not in the mind of each or any individual. This is rather like the view that a family must be studied as a system, and not merely a collection of individuals. The argument is usually overstated. Systems theory deals in systems many orders of magnitude more complex than their components units, whose emergent properties are radically different from those of the units and not to be inferred easily from the study of the individual units, even if *all* of them could be accounted for. Macro-economies may be real social *systems*, but families and groups are not. Put another way, it is easy to believe that people exist who can understand individual acts of buying, pricing and preference, but are bewildered by inflation and recession. It is harder to conceive that there are people who are adept at understanding other people, but who require a special conceptual scheme before they can make sense of a family. They might need to deal with the whole family rather than an incomplete sub-set of family members, but not usually in the light of a special set of ideas and understandings for

families, far removed from anything that works for people *per se*.

Socio-psychology

Hybrid processes

Turning now to the formulations which are *socio-psychological* rather than *social psychological*, we come to a fifth possibility, namely the study of those processes that are part psychological and part social, such as the processes governing the train of events in cases of unemployment or divorce. These cases are intrinsically hybrid and are understood better by the conceptual re-assembly of pure sub-processes than by the identification and decomposition of mixed domains as *ad hoc* primary topics. The reason again has to do with natural kinds, and the restricted levels of generalisation that are possible when working across natural kind boundaries.

Networks of social and psychological factors

The sixth formulation would be the study of that web of causally interrelated social and psychological factors that includes behaviour, profession, class, beliefs, personality, physical environment, and so on. This is another instance of 'surface networking'. It is a commonly employed conception of social psychology, but easily degenerates into a ragged and limitless linking of arbitrary variables, and is usually too inductive, instrumentalist and superficial to meet the criteria suggested earlier, as we have already seen. Again there are indefinitely many measurable physical and social variables that could be documented and interrelated. The job would be enmeshed in its own complexity long before it was near to completion. Happily there is no point in finding the relation (if any) between most pairs of variables, and the advanced sciences do not bother to try. Under formulation six of socio-psychology, however, the data mountain of unhelpful correlations is building up rapidly, making the learned journal collectively more and more expensive to produce, while less and less fruitful to read. Research of this kind is often incestuously bred from the literature itself. Instead of being a transaction with the world, with the literature in support, as research should be, this kind of work often seems to revolve around the transactions between the researcher and the literature, with the real world serving only as a means to that end.

The role of narrative

A curiosity that the chosen conception of social psychology needs to explain is the oddly self-explanatory quality of life stories, and other kinds of case histories (de Waele and Harré, 1979). They seem to provide explananda and explanantia together in the same package - to raise and solve the problems at one and the same time. Under formulation one of the nature of social psychology, this phenomenon would be seen as a psychological process controlling a psycho-social interchange, and the life course of the individual (the public, surface, social object) would represent both the sources and consequences of the individual's nature. As such it would stand as a record of the input-output history of the person, from which the mediating process may be omitted with little loss of intelligibility. However, that is only to raise a further problem, namely that of the uniqueness of individual biographies. If the mental system we wish to document is functioning as a kind of generative software, of which the individual's life story is an input-output record, each level in the program structure will be moulded to the requirements of its life situation by a higher order program responding to environmental contingencies, and the higher order program will be subject to revision by still higher order programs, and so on. In this case it is likely that the organising principles of any individual personality, at most if not all levels of abstraction and control, will be as unique as the life course with which they interact seems to be. This reduces the prospects for non-trivial general findings, since any description of organisation which is put forward is likely to arise only under very specific and rare circumstances, or else to be generally true only at the expense of being vague and banal (Meehl, 1978). In much the same way, the explanation of the behaviour of computers by references to their programs, will usually involve rather empty generalisations, or specific details which are confined to a particular instance, whose unique interaction history has given it a unique set of programs and behavioural tendencies.

The study of syndromes

Does this leave us without a suitable level of generalisation to address with our research? Maybe not. To take a different analogy, the physiological parameters of individuals vary widely, as do their consequences for health and behaviour. Few biologi-

cal generalisations would be true of all individuals, but happily the variations do not occur independently, but in more or less discrete clusters, of which an example might be pathological syndromes. Each syndrome has a number of interconnected features, so that if enough features are detected to establish the presence of the syndrome, the others may be inferred, prognoses made, and remedies chosen. However, it remains true that no syndrome is possessed by all individuals, and no individual possesses all syndromes. The generalisations that can be made are all conditional: "If someone has hypertension, then it follows ..." but of course not everyone does, and for the others it does not follow. The findings that might emerge at best from the line of research I am advocating here, would not be universals of human nature, but mental 'syndromes' - clusters of organisational features that occur and recur together. When they occur, they can be identified and strong inferences made, but for any one syndrome these inferences will be unjustified for most individuals. The mental syndromes would be a hybrid of individual, social, endogenous, environmental, and behavioural features and may well change quite rapidly with time, in which respect they would differ from personality traits and types. They would be more similar to the diagnostic categories of psychiatry and clinical psychology, but without the implications of pathology, diagnosis or cure, and of course they would be much commoner and more varied in type. An example of such schemata might be the system of psychological types suggested by Jung.

This is not the place to argue the merits of dynamic forms of psychology, but they do have one particular virtue which experimental social psychology does not, and that is a more sensible and more harmonious relation with the everyday psychology of commonsense mentalism.

Common-sense psychology

Most schools of scientific psychology start off by rejecting commonsense experience as unreliable, subjective, inaccessible and ephemeral. That is all as may be, but at the same time it is to miss the point. We are too ready to reject commonsense ideas on the basis of a one-sided cost/benefit analysis. We assess the disadvantages and costs of believing in commonsense mentalism, and so decide to discard it, but we do not consider the costs of setting it aside and attempting to start again from scratch. A case can be made for the retention of commonsense psychology

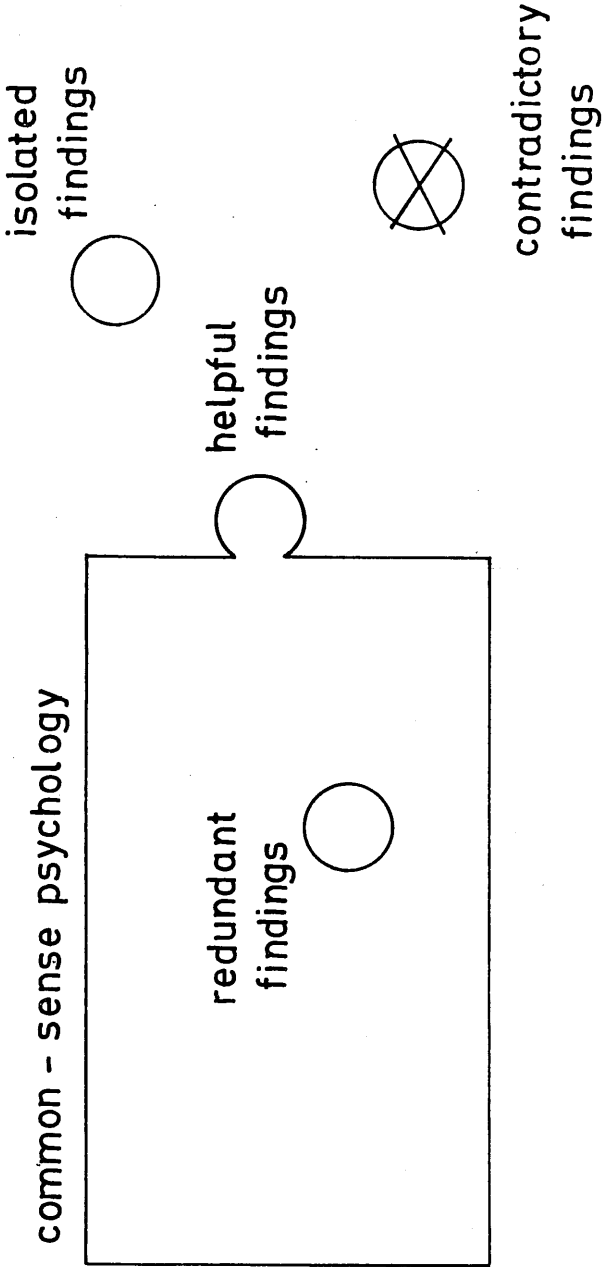


Figure 3: Four relations of scientific psychology to common-sense psychology.

as a starting point for scientific analysis, not on the grounds that it does not have all of the disadvantages that are claimed for it, but because for most people and most purposes most of the time, it is all there is. In everyday life, psychology is no more or less than the understanding of the pattern of thoughts, ideas, feelings, intentions, and so on, that are familiar to all of us, and on which we base our ability to understand, predict and control our behaviour, and that of others. That is, as it were, the state of the art, and the state of knowledge to which scientific psychology may add, or may fail to add, as the case may be. It is also the yardstick against which our efforts as psychologists and our findings will be evaluated by most people.

Relevance

What are lay people to make of scientific psychology? There seem to be four relations in which it could stand to the kind of psychology which is already so widely used in everyday life.

Redundant findings

The first is that it could fall within the area of knowledge that people already have, and therefore seem to them to be largely superfluous and silly. A mixture of trivial talk and scientific jargon being used for obvious things. This is precisely the view which the public seems to hold of certain areas of psychology at the present.

Isolated findings

The second possibility is that our research might be completely disconnected from the body of commonsense knowledge that people normally use. It would be none the less rigorous and valid for that, but for their purposes largely unusable. Since it would not start out by accepting and predicating its ideas upon ordinary mental experience, it would be in no systematic way complimentary to it, or an addition to the practical capacities that commonsense psychology bestows. This is also a very common judgment for large areas of modern scientific psychology.

Unacceptable findings

The third possibility is that research might be conducted from such a strange and alien conceptual starting point that it seemed entirely contradictory to the ordinary view, and as such was both unusable for most purposes and threatening. Because of the way it was formulated, it would not only make no addition

to ordinary commonsense beliefs, but would require many of those vital and functional commonsense beliefs to be set aside, before it could be seriously and sincerely believed.

Helpful findings

The last possibility, which comes about rarely by chance, and is even more rarely attempted deliberately, is systematic, careful, sceptical, rigorous scientific research being used to extend the domain of commonsense psychology, accepting its existence and its current content provisionally, and looking at psychological entities and processes which are outside it, but coherently connectable with it. That would avoid two extremes. On the one hand it would avoid making commonsense knowledge the topic of study: it does not need to be studied because by definition it is what people know already; and at the other extreme it would be to avoid ignoring it completely, and embarking on research in directions which have nothing whatever to do with the world as we normally experience it. However, much of social psychology at present does seem resolved either to study common-sense or else to ignore it, as for example in research on attitudes and attribution theories on the one hand and studies of social interaction and non-verbal communication on the other. (For explications and discussions of common-sense psychology see Heider, 1958; Smedslund, 1978; Furnham, 1983; and Fletcher, 1984). Since these topics between them cover all the major areas of the subject it is hardly surprising the saying was coined that "two thirds of social psychology is common-sense and the other third is nonsense".

In short the answer would seem to be to treat commonsense knowledge as part of the literature. That would produce the same mixture of acceptance and scepticism which we would apply to any other part of the literature. What we believe in everyday life as our commonsense psychology would be subject to review and revision if *particular* parts of it gave *particular* grounds for doubt. It would not have to be accepted for all time as an article of faith. On the other hand it would not have to be rejected lock, stock and barrel simply because some parts of it give grounds for doubt and concern, anymore than one rejects the entire scientific literature in a subject, just because some experiments are later open to question.

This is not a radical or an unscientific idea. It is common practice in the advanced sciences. Most of them grew out of the everyday knowledge and practices that were most closely related to their field. Biology grew from agriculture and animal husbandry, physics from navigation and simple machine making,

chemistry from cookery and attempts at alchemy. With the passage of time, the original folk sciences became swamped with technical findings, and in many places the original common-sense core was re-worked and redefined. But in the early stages the common-sense template was vital to get the conceptual structure of each subject started, and the pragmatic relevance of its content correctly oriented. No successful science has begun by disregarding the beliefs of people at the time, concerning that particular phenomenon, and then attempting to start again in a conceptual vacuum. The problem with much of social psychology is not that its content has turned out to be wrong, but rather that from the way its questions were originally conceived, it was doomed to be irrelevant from the outset, regardless of the answers it came up with. What we need, in addition to orthodox social psychology, is an 'augmentational' psychology, which sets out to enhance the understanding that people normally show for one another, rather than to set it aside as unimportant. This is not necessarily a matter of embedding its content in the content of our professional psychology, so much as embedding the capacities of folk psychology in the capacities of our enhanced version. We do not have to believe what laypeople believe, and more besides, so long as we can do what they can do, and more besides. A very nice example of common-sense concepts being used and extended in psychological enquiry can be found in Axline's (1964) account of the growth of a lost, frightened little boy into a complete person with articulate opinions, warmth and the capacity for relationships. If we could establish a branch of psychology which could take that kind of understanding as its point of departure, and go further still, setting up and testing ideas which extend our familiar framework, we should have achieved something remarkable.

Making matters worse

Let me go further. I think it may not be just that a narrowly conceived version of social psychology is less useful than it could be, but that it is actually harmful. There is likely to be a *negative* transfer of training between the ordinary, everyday psychology that is so vital to the understanding of one person for another, and the kind of impersonal, amental conceptual scheme which social psychologists are required to use for professional purposes. It may well be the case that, in certain respects, the better you become at being a social psychologist, the *worse* you become at understanding people. This negative

transfer has not been formally demonstrated but its plausibility and seriousness mean that it should not be dismissed lightly. Certainly it is not ruled out by the fact that experimental social psychology has similar goals to common-sense psychology, and it acts in good faith using what it believes to be sound and efficient methods. If anything these are just the conditions under which a negative transfer *would* occur, where there are two ways of addressing the same general goals, using methods which are similar enough to interact, but dissimilar enough that the use of one would preclude the uninterrupted employment of the other and would pose choices between mutually exclusive rather than supplementary modes of reasoning.

There have been many critiques and reformulations of social psychology in recent years (e.g. Langer, 1967; Foa and Turner, 1970; Hudson, 1972; Harré and Secord, 1972; Elms, 1975; and Harré, 1979) and some retorts (e.g. Totman, 1980). By and large the debate has centered around the alleged sterility and artificiality of much social psychological work, while the suggested remedies have tended to involve obscure philosophical and sociological positions which still fall short of the familiarity and utility of ordinary common-sense reasoning and terminology as a point of departure.

There seem to be several distinct disadvantages to any formulation of psychology that excludes our ordinary experiences of the character and regularity of mental life. Firstly there is *poverty*: much of the interesting material is necessarily missed out of the subject. Secondly there is *redundancy*. Because what is already known is not acknowledged, there can be no systematic safeguard against our appearing to discover it afresh, and that is what happens much of the time. Thirdly there is *low credibility* in the eyes of ordinary people. Not only are many 'discoveries' presented to them which are already familiar in the system of knowledge whose existence they rely upon and we deny, but these facts are presented as if we were ignorant of their already being known. Nothing could be less plausible than experts who insist on telling you what you already know, with no apparent awareness of the fact that everyone else knew it before they did. Fourthly the narrower kind of psychology can be unnecessarily *threatening* to our sense of social understanding and competence. For many of its beliefs to be accepted, the most crucial and valuable of our ordinary beliefs would have to be abandoned, because they are set up on incompatible bases. This is not always because empirical demonstration has shown one version of psychology to be better than the other, but simply as a matter of ideology that one is assumed to be wrong

in principle, and the ground is left free for the other to predominate. Counterintuitive ideas should not be forbidden, of course, but they should be produced with some regard for the fact that they cost us far more in conceptual disarray and retreat, than do merely *non-intuitive* ideas, which *add to* the prior state of knowledge, without robbing it of something in return. Scientific psychology should be available to us *as well as* our ordinary understanding, not instead of it, or at its expense. Fifthly there is the *inapplicability* of the non-augmentational psychologies. Many of the problems which people regard as being in the domain of real psychology, such as conflict, family breakdown, social unrest, and various forms of character analysis, including expert testimony in courts, are often left to psychiatrists rather than psychologists. This is not because the topics are necessarily psychiatric. The psychiatrists are not called in because there is a medical problem, or because the people in question are mad. They are called in because they have become the surrogate psychologists in a society whose psychologists have abdicated part of their proper role. Psychiatrists *will* often attempt to explain how a person's actions relate to their thoughts, feelings, ideas, hopes, resentments, and so on, and so they are useful to the rest of society who make sense of behaviour in the same sort of terms. Psychology on the other hand, by rejecting that form of explanation and any attempts to compliment it systematically, has directed its usefulness towards the technical fields of brain engineering in the hardware of software sense.

Sixthly there is the problem of *alienation*. It could be argued that the study of scientific psychology as it is presently conceived can in some respects be deskilling to its practitioners. It requires them to adopt and to practice the belief that their familiarity with the organisation of mental life, the basis of their real understanding and empathy with other people, is valueless. The effect of this, has been to alienate us as a profession, and as individuals, from the primary basis of human understanding - knowing *at first-hand* what it is to think, feel, know, and remember - knowing personally what those things are like and how they are organised. Since as psychologists we do not predicate our theories upon ordinary experience, our ideas do not complement the domain of knowledge which is practical psychology for most practical purposes, and hence they are often of limited relevance. We allow the ordinary faculties of sensible mature sympathetic understanding to go unused for professional purposes, at the risk they will atrophy or become unavailable altogether.

Our scientific psychology exaggerates its own discoveries by undervaluing the competition. It claims there is nothing to compare with, and compared with nothing it does well. Had the comparison been made more fairly, it would be found on many occasions to do less well than the forms of psychology it claims to displace.

Knowledge should be useful rather as a street map is useful. It should enable us to navigate through the parts of the world we are not familiar with, without getting lost. But what is the use of a street map on which you cannot find the familiar landmarks? How would you start to use such a map if you could not find on it, and therefore relate to it, the places and guidelines you use already? In just this way the map of mental functioning provided by scientific psychology is unnecessarily limited in its usefulness by going to such lengths to omit the familiar common-sense mentalistic landmarks to which it should be complementary, but in fact is irrelevant.

Like any good and useful map, the scientific account of a complex system should have a number of other properties too. It should, of course, be accurate, but that is far from the only consideration, and our evaluation procedures for psychological models are often much too concerned with accuracy at the expense of other features. Generality, consistency, completeness and coherence play as great a part in making a map useful as its accuracy, and yet they are criteria which are not applied to, and could not be met by, most forms of psychological theorising. In this respect scientific and common-sense psychology are similar. They both do well on accuracy, and then less well through the list, with coherence being worst of all. All the more reason then, for the scientific branches of psychology to re-think their strategy. At present they are giving us most of what we have already, and least of the qualities which our pre-scientific models lack.

No research strategy can work miracles. To achieve breadth and accuracy simultaneously is asking the impossible. We must start with one and work towards the other. To begin with accuracy and work towards breadth is like mapping a terrain on foot, step by step and detail by detail, covering more ground as time goes by but in the same detail. To begin with breadth and work towards detail and accuracy is like starting with a satellite photograph to get the general outlines, then aerial photographs, and so on. In this case the same ground is covered at each stage but in increasing detail as time goes by. Social psychology seems

committed to the former, even thought the latter has much to commend it. At each stage there is a global, complete, consistent, coherent picture. Above all when the details do arrive, it is known where in the big picture they belong. For the mappers on the ground the greatest danger and likelihood is that they will be swamped by the excess of details, and never retrieve the general picture at all. In this analogy the role of common-sense psychology would be to provide some of the early satellite passes, around which the search for greater detail and precision could be organised. At present we do the opposite. We emphasise the quantifiable minutiae, and reject the use of common-sense generalisations as guidelines to specific research.

To take a further analogy, teaching somebody a mentalistic psychology, is like teaching them to paint with their eyes shut. After a sustained period of training they become better at this curious knack than they were before. And in comparison, the trained do much better than the untrained. Provided no sceptic came along to point out that they still do not paint as vividly or spontaneously as those who do it with their eyes open, all might seem well. Much the same happens in social psychology: we are all taught to start off with our eyes closed, by abandoning the sources of information we would normally trust. We then acquire the skills of operating without them, and as time goes by, we can be shown to do better and better at it. What we do not ask, and should, is whether we are doing better than we would if those other kinds of information were also allowed into the picture.

In general, I am suggesting that social psychology as a discipline is misguided in a number of respects. This is not meant to be discouraging and certainly not disloyal. We should all have a greater loyalty to seeing the discipline improved upon than seeing it approved of, and the really crucial question is in what respect and by what means can it be advanced. The great difficulty, though, is that most disciplines do not lose their way, and the institutions and procedures of the academic world assume that they do not. The re-direction of a discipline is something the system makes no provision for. The criteria of publishing, appointment and promotion all presuppose that the present ways of doing thing are generally the right ways, and that radical departures must be mistaken. Vicious circles abound. If a discipline has gone in a certain direction then that direction determines the kind of material which exists to be taught to students. Therefore teaching jobs are created in those areas and people selected to fill them who are suitable to teach according to the existing traditions, and in general agreement with them. Researchers have more licence to innovate, but re-

search posts are short-term, and long-term change requires long-term employment, which is only provided by teaching posts, where on the whole dramatic change is contra-indicated. No clear cut priorities or leadership structure emerges because seniority has to be determined by politics and power struggles in a discipline that is too disarrayed to share a consensus about the structure of its subject-matter, or about the issues and accomplishments which are most crucial to it. The system is essentially conservative. Unwittingly it operates to keep things the way they are. A discipline which is on the right lines will stay on the right lines. A discipline which is going the wrong way will tend to go on doing so, and there is no simple remedy.

The moral of all this is not to abandon social psychology but to reconstitute it around the well-formed topics with which it overlaps. Experimental social psychology may continue to serve as a category of convenience, but the real allegiances of its constituents topics will be to their closely related areas of biological and cognitive psychology. They cannot cling to each other indefinitely in search of a spurious common element of uniquely and coherently social content.

In conclusion

A reconceptualisation of the nature of social psychology is called for. The main characteristics of this reconstituted field should be (a) the search for new classes of source process, rather than new classes of resultant phenomena and whatever produces them, (b) the selection of strong topic domains to investigate having the properties of concealment, coherence, causal power, stability, reality, natural-kind divisions, and fundamentality, (c) the strategy of moving from broad general formulations towards greater detail and specificity, rather than from the detailed study of one small item to the detailed study of the next, and (d) the attempt to supplement common-sense psychology systematically rather than ignoring it or else studying it.

Several lines of research would fit all of these characteristics, but one in particular seems to typify the general features I have been suggesting here. That is the study of the high-level psychological control processes which are responsible for global and long-term behavioural configurations. As I argue in the companion paper to this² these largely affective processes are generally outside of, but complementary to, the present domains of scientific and common-sense psychology combined. They are crucial for both theoretical and practical reasons, and are as

close to being the basic psychological factors in human social life as anything is likely to be, given the fundamental problems with fundamental research in this field, that I have tried to describe here.

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Reference notes

1. I am indebted to Dr. Kathy Wilkes for this example.
2. The companion paper is *The heart of the mind: a speculation on the fundamentals of human psychology*, which appears in L. van Langenhove, J.M. de Waele and R. Harré (eds.) *Individual persons and their actions*. Brussels: Free University of Brussels Press.
3. Cited in Bernstein, J. (1973) *Einstein*. Glasgow: Fontana/Collins.

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