

# A critical review of the biopsychosocial model

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**Objective:** The aim of this review is to provide an analysis of the epistemic status of the biopsychosocial model.

**Method:** A critical comparison of the biopsychosocial model with the general concept of models.

**Results:** In its present form, the biopsychosocial model is so seriously flawed that its continued use in psychiatry is not justified.

**Conclusion:** Further development of theory-based models in psychiatry is urgently needed.

**Key words:** biopsychosocial model, models in psychiatry.

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In a series of papers starting in 1960, Engel [1-5] outlined what he saw as deficiencies in the dominant 'biomedical model' as it applied to medicine in general and to psychiatry in particular. He characterised the biomedical model as reductionist ('...the language of chemistry and physics will ultimately suffice to explain biological phenomena' [2, p.130]) and dualist (i.e. it separated the intangible mind from the physical body). Despite the undoubted success of the biomedical model, Engel saw this tendency to discount mental life as a major weakness. Humans are not just biological preparations, he argued, but exist as sentient beings in a causally significant psychological and sociological milieu for which the biomedical model can give no account. A true science of human affairs, he averred, would be able to incorporate this milieu.

Engel acknowledged that his complaints against the dominant ethos were not new, that 'psychosomatic medicine' had been offered as the bridge between the 'two parallel but independent ideologies of medicine, the biological and the psychosocial'. That particular bridge had failed, he believed, because it was forced to conform to 'scientific methodologies basi-

cally mechanistic and reductionist in conception and inappropriate for many of the problems under study' [2, p.134]. This, he suggested, was the fault of the dominant model of science: a rigid, reductionist positivism that could not take account of human factors and, therefore, ignored them. Engel argued that, intangible or not, human factors are not irrelevant but apply just as strongly in orthodox illnesses, such as diabetes, as in classic mental disorders, such as schizophrenia.

In his view, the 'psychosomatic' model had failed in that it had been based on Freudian and Meyerian concepts, and he implicitly acknowledged that there was no way these could be formalised into a system compatible with the biomedical model. In the first place, constructs based in these theories were not amenable to analysis using the methodology of modern science and second, empirical research failed to validate crucial elements such as the disease-specificity model. Therefore, in order to advance where psychosomatic medicine had become mired in irrelevancies, he suggested a new approach: the biopsychosocial model. This, he insisted, was a scientific model where the psychosomatic had not been. In place of the unproductive psychoanalytic model, he suggested that a new approach, Von Bertalanffy's General Systems Theory (GST) [6], provided a suitable orientation. Its particular strength lay in its purported capacity to permit scientific investigation

across the different levels of a hierarchy without attempting to reduce higher levels to lower, or even dispensing with them, as the reductionist biomedical model did.

After a final tilt at the prevailing 'impersonal and mechanical' teaching in US medical schools in the mid-1970s, Engel concluded: 'The proposed biopsychosocial model provides a blueprint for research, a framework for teaching, and a design for action in the real world of health care. Whether it will be useful or not remains to be seen' [2, p.135]. This paper will argue that any value which may have derived from Engel's biopsychosocial model has been entirely fortuitous, because it is not a model at all.

### Models in science

Models are absolutely fundamental to the progress of science: all the more surprising then, when we realise just how little of the philosophical work on defining what a model is or does [7] has percolated through to the scientific literature. My dictionary gives nine definitions of 'model' as a noun, mostly concerning toys or young women in expensive clothes. The ninth definition states: 'A simplified representation or description of a system or complex entity, esp. one designed to facilitate calculations and predictions.' This is the technical sense in which we use the word in psychiatry, but as with so many of our technical terms, the word has lost much of its specificity over the years. While we may believe we have models, the following brief review of the literature will show that, in fact, we don't.

In ordinary usage in the non-medical literature, two meanings of the word 'model' are readily apparent. These need to be considered in some detail in order to show how far we have strayed from the theoretical mainstream in our use of the word. Lacey [8] states: 'A scientific model is normally a theory intended to explain a given realm of phenomena, or a sort of picture intended to explain a theory by replacing its terms with more perspicuous ones'. This is not entirely perspicuous itself: is a model a theory or the realisation of a theory? The question is not inconsequential, as the alternatives represent different branches of epistemology.

To Beer, the matter is less opaque: a model is 'a representation of something else, designed for a special purpose... All models have one characteristic in common, whatever their purpose. This characteristic is the *mapping* of elements in the system

modelled onto the model' [9, pp.394-395]. He distinguished between isomorphic models, in which every element of the larger system is mapped, and homomorphic, in which complexity is deliberately sacrificed. He also saw a difference between physical models (as in the model aircraft used in windtunnel experiments) and systems of mathematical equations which 'model the behaviour' of the particular theoretical system. Somewhat confusingly, he referred to the latter as 'theoretical models' rather than as mathematical models, which is what they are.

In his monograph 'The Nature of Explanation', Achinstein [10] did not explain the difference between a theory of explanation and a model of explanation, although it is clear from his usage that theories and models are conceptually quite different. A theory is a broad, general statement, while the model of the theory is the actualisation of the theory, the (truncated) theory at work, as it were. In this case (of explanation), the theory rests within the framework of a larger theory of language, while the model is the theory exemplified in a form of logical calculus. In the field of semantics, Leech emphasised this important difference between theories and their models: 'Whereas theories claim to tell us what reality is like, models claim to tell us what reality can and could be like—given certain speculative assumptions' [11, p.87].

The work of the philosopher Karl Popper was entirely devoted to theories: how to distinguish scientific theories from non-scientific; why we should prefer one theory to another; how science progresses via its successive theories, etc. To Popper, theories were ideas of the highest and most abstract kind. He used the term 'model' sparingly and almost always in a real and mechanical sense (e.g. [12, pp.172, 358-359]). This is perhaps a little unexpected, as the concept of testing theories was central to Popper's philosophy of science, and theories, he owned, were tested through their direct applications as models. However, since many of the theories he used in his work existed largely in mathematical form, they were perhaps not easily differentiated from the mathematical models which derived from them. In Popper's evolutionary view, the value of theories is that we can let them die in our places, but we will also construct a long series of models before suspecting the theory itself. The theory of heavier-than-air flight survived many crashed models.

One of the most influential modern philosophers of mind, Daniel Dennett, has at times tended to use the terms 'theory' and 'model' interchangeably, while at

others he has distinguished sharply between the theory and the model that simulates the theory. Theories are ideas; as such, they can have only logical consequences. By contrast, the whole point of models is that they are 'experience generators', acting rapidly to generate (an approximation of) the material consequences which flow from the application of the theory. A model is the practical means of matching a theory to reality. He emphasises that models must be distinguishable from the real things they model [13]. For example, one cannot get wet or windblown from a computer model of a hurricane (p.191).

At this stage, the two meanings attached to the noun 'model' are quite clear. A minority of authors use 'theory' and 'model' more or less interchangeably to represent an idea or notion. The majority, however, use the terms quite separately, reserving theory to nominate unembodied concepts or abstract notions and model as the name for a class of real things, usually simplified diminutives of the unseen objects and processes outlined in the theories.

In the somewhat less disciplined 'social sciences', things are never quite so clear. Ryan [14] identified the distinctive features of theories and models which have emerged thus far. In the first place, he accepted the Popperian notion of submitting theories to severe or critical tests: 'Given a (theoretical) generalisation, we want to see if it holds under unusual conditions...successful causal laws are those which apply under the most improbable conditions' [14, p.64]. We cannot just walk out and test a theory, however. Theories are ideas, and ideas don't fly; they have to be brought into the real world for testing. He saw it as important to 'unravel the distinction between theories and such close relations as models, maps, metaphors and analogies' [14, p.76]. Theories, he suggested, make existential claims about the world (i.e. they say that this is how things are). Models, on the other hand, do not. They simply say it is as if this is how things are: 'Those who produce models do not make existential claims about the world; but those who produce theories generally do...' [14, p.95]. The close parallel between his position and that of Leech [11] is clear.

The common element in these accounts of models, including physical, diagrammatic and mathematical, lies in their function: models model. What do they model? They model theories or theoretical constructs, meaning they embody, actualise or realise an idea, notion or concept. The idea is itself based in and derived from a series of propositions (a belief system) regarding the nature of reality. Such propositions are almost always metaphysical and, therefore,

lie outside the purview of science as we now define it. Nonetheless, there is no escaping them; our science is suspended in a web of metaphysical assumptions. To be science, however, the theory itself must be more than a mere metaphysical claim; its propositions must be cast in a form which permits empirical testing, which is where the model comes into its own.

Simply stated, the purpose behind a model is to see if, at a first approximation, the theory works, to actualise its logical consequences and thereby subject it to the kinds of 'severe tests' which Popper saw as essential to scientific progress. In this sense, models are real and their material consequences can be measured, whereas theories are ideas and can no more be measured than daydreams. Model-building separates the theories with a future from those which will always remain dreams. Anybody contemplating buying shares in the Perpetual Motion Machine Company should ask to see a working model of the product. If the prospective purchaser were shown a model Perpetual Motion Machine, however, how could it be distinguished from, say, a chocolate frog? In other words, what is the relationship between a theory and its models?

Beer listed five steps in building a theoretical (mathematical) model: (i) the variables to be used in characterising and understanding the process must be specified; (ii) the forms of the relationships connecting these variables must be specified; (iii) ignorance and the need for simplicity will ensure that all relationships other than identities are subject to error and so, for purposes of efficient statistical estimation, these error terms must be specified; (iv) the parameters of the model must be estimated and the extent of its identification ascertained after which, if this is inadequate, the model must be reformulated; (v) finally, the model must be kept up to date and used so that an impression can be formed of its robustness and reliability [9, p.394].

Clearly, these steps are idealised, but they indicate the rigour which must accompany model-building in its broader sense. A model must be a formal and recognisable embodiment of its theory. Regardless of the validity of the theory, if the model is wrong, investigating it is non-science.

### The biopsychosocial model

It is wrong to criticise an earlier theory because it was not formulated according to modern standards. However, knowing that standards change, it is legiti-

mate (in fact, necessary) to reassess older theories to see if they still meet acceptable standards. So where does Engel's biopsychosocial model stand?

The first thing we have to decide is whether it is a theory or a model. Engel was quite explicit: it is a model. However, since his construct does not satisfy any sense of a model as a formal, working representation of an idea, this cannot be accepted. At most, it could be a very general theory; even as theory it is seriously flawed. Engel relied on von Bertalanffy's GST to validate his assertion that the (positivist) scientific method could be used to investigate different levels of human activity. However, there is nothing in any of that author's writings which justifies that opinion. Engel tacitly acknowledged this: 'For medicine, systems theory provides a conceptual approach...(for studying the biopsychosocial concept)' [2, p.134]. At no stage did he indicate it was any more than conceptual.

Von Bertalanffy's writings provided the conceptual approach Engel required; to turn that approach into a methodology required another step. For that step, Engel turned to other writers, primarily Karl Menninger [14]. All other authors citing GST do so too, but there is no methodology in that book, either. All Menninger said was that he thought GST was an interesting idea; one which may be able to render psychosocial issues amenable to scientific analysis by the extant methodology. That has never happened and, in this author's opinion, never will because extended outside its physical and mathematical basis, GST became utterly banal. It could only line up rough analogies and try to extract broad, inductive generalisations about them. It could never extend to a general methodology of human affairs, not the least because nobody has ever shown that the critical matter-energy transfer functions are applicable to the mind-body problem. It is not possible to build a model of the mind based on a theory of systems; it is the wrong sort of theory. Models of mind have to be based in theories of mind, although one may organise the theory of mind according to the general principles of a theory of systems. In essence, a vital element of Engel's program failed to fulfil the promises he saw in it. That objection is, however, rendered otiose when we realise that Engel never actually wrote his biopsychosocial model.

A strange thing happened in Engel's seminal paper [2]. He set the scene by arguing at some length that the dominant model (he meant theory) in medicine, the biomedical model, was producing generations of doctors as technicians, was costing a fortune and was

not very nice to patients. He showed where this model had arisen, subsequently sketching an outline of what he thought a new model would need to be able to do in order to overcome the biomedical model's failings [2, p.131]. He then argued generally that 'a biopsychosocial model' (and note the indefinite article) would satisfy those requirements if it could incorporate scientifically the 'impact of nonbiological circumstances upon biologic processes' [2, p.134]. He continued to use this general term without specifying what form such a model would take: 'The development of a biopsychosocial medical model is posed as a challenge for both medicine and psychiatry'.

Next, Engel argued that GST could provide the basis for a biopsychosocial model, followed by another critique of 'dogmatic biomedicine', after which he announced the birth of his model: 'The proposed biopsychosocial model provides a blueprint for research, a framework for teaching, and a design for action in the real world of health care'. Note the change from the future conditional tense to the present, and from the indefinite article to the definite: of the model itself, there is nothing to be seen. Engel simply demonstrated a need for a particular approach, talked about it for a while then announced that he had found it. He had not. All he offered was an emotive case for more humanity and less technology in medicine: little more than a heartfelt plea based in a particular ontological stance. It was not a theory, and it was certainly not a model.

In a subsequent paper [5], Engel described the clinical application of his putative model. '(The) biopsychosocial model', he argued, 'enables the physician to extend application of the scientific method to aspects of everyday practice and patient care heretofore not deemed accessible to a scientific approach...The biomedical model can make provision neither for the person as a whole nor for data of a psychological or social nature...' He then outlined his 'model' but once again it was not a model in any practical sense of the term. It was an earnest plea for a model, a description of where such a model would fit in medical thinking, but not the model itself. He then devoted the rest of that paper to a detailed discussion of a case where psychological factors complicated a man's myocardial infarction. His discussion was interesting but certainly not revolutionary.

## Discussion

It is worth recapitulating some basic elements in the epistemic status of psychiatry. Our ontological

position is materialist; that is, we operate within a system that asserts there is nothing in the universe beyond matter and energy interacting in a time/space matrix. Being curious creatures, we like to investigate the universe, and there are lots of ways of doing this: armchair philosophy, listening to prophets, reading chickens' entrails, or by a rule-governed, empirical project aimed at elucidating the nature of the universe independently of all but our most basic prejudices. This last method we call science, and because the fundamental elements of our materialist universe are restricted, so too is the scope of our science.

The Universe itself, however, is so large: what is the intellectual starting point for one's investigations? For a starting point, we have theories, which often aren't much more than rather well-articulated guesses of how things must be behind the scenes ('...theories claim to tell us what reality is like...' [11, p.857]). Theories must proceed logically from our ontology, with no sudden discontinuities. Since we don't have a rational, empirical way of investigating non-observables, we rule them out of court (i.e. a scientific theory cannot invoke non-observables). To this extent, the rules governing the proper subject matter of science and how it is to be studied help to determine the form of those theories. At the end of this logical chain sits the model as the realisation or reification of the theory, a working demonstration of '...what reality can and could be like...' [11] built to test the theory in action.

It is all very restrictive. Ontology determines the content of theories and the rules for investigating them; the rules themselves help restrict the form of the theory; a model must exemplify the theory in such a way as to permit its investigation within the common ontological stance. There is a clear line from the most general statements of what we believe the universe to be, right down to, say, a working model of a better mousetrap. Mousetraps cannot defy any of the more general rules or principles on which their function depends, and there is only one test of a good mousetrap.

It is immediately clear that this restrictive view (of science) excludes from scientific consideration a great deal of what we regard as quintessentially human. If, as humans, what we think and feel matters, then we must either find a way of handling these very slippery notions within the field of science, or else change our rules of science. Unfortunately, changing the rules of science is not so easy, not the least because they derive directly from

our ontological stance. Since we have not yet been able to enlarge the arena of science to include things which are in principle unobservable (because they include the supernatural), we are caught, as Eisenberg noted [16], between the Scylla of mindlessness and the Charybdis of brainlessness. Traditionally, psychiatry has fudged this critically important issue, formerly by tolerating a number of incompatible theoretical schools, but more recently by espousing a vapid 'eclecticism' [17].

Engel's 'biopsychosocial model' was an attempt to steer a course through this unhappy impasse. He wanted to retain all that was best in the western scientific ethos, yet find a way of rendering our unobservable mental life amenable to an orthodox scientific analysis. His attempt foundered on a number of points, meaning that, theoretically, we are back to where we were 30 years ago when all the main theoretical schools in modern psychiatry were approaching their centenaries.

Even if we allow that his model had its imperfections, might his case not be saved by showing the model at work? Regrettably, this is not the case, for two reasons. First, a description of a model at work is not the same as a definition of the model itself. The act of nominating a model by demonstrating its output cannot simultaneously serve to define it separately from all other models which may have a similar output ('What is this?' is necessarily different from 'What does this do?' as other things might do it, too). Engel did not define his biopsychosocial model; instead, he hoped its definition would emerge ostensibly through a description of how it might function, with the emphasis on 'might'. This does not permit it to reach scientific status: a description of what something does can never be an explanation of why or how it does it.

In the second place, it might be argued that an approach which considers biological, psychological and social factors necessarily amounts to a biopsychosocial model; however, for several reasons, this is not the case. To begin with, we must clearly distinguish theories with real predictive value (i.e. they can predict something we did not know or which was counter-intuitive) from those which can only 'explain' or rationalise what we already know. The former are science, but the latter are just self-reinforcing prejudices. Only highly improbable predictions can test a theory's basic assumptions. Furthermore, researchers who gather data from a variety of theoretically unrelated fields will not be able to test the basic assumptions which led them to

collect just those data and not others. They may be able to detect associations but, critically, not errors in their own basic assumptions. Only a model with true predictive value can do that (and then not always).

Finally, unless there is an integrating theory already in place, gathering biological, psychological and sociological data about people will only yield scattered lumps of information that do not relate to each other in any coherent sense. Without an overarching theory to integrate the fields from which the data derive, associations between differing classes of information are meaningless. For these reasons, Engel's description of his model at work must fail as an attempt to define the model itself.

A critical reading of Engel's paper [5] does not reveal anything that would not be known (implicitly, if not explicitly) to any practitioner of reasonable sensitivity. What he argued powerfully, however, is that too many modern physicians are not of 'reasonable sensitivity', for which he blamed modern medical training: 'The reductionist scientific culture of the day is largely responsible for the public view of science and humanism as antithetical.... The triumphs of the biomedical model all have been in the areas for which the model has provided a suitable framework for scientific study'. That is to say, biomedical science was very successful so long as it did not stray too far from the same theoretical position as veterinary science.

In practice, if we want to know whether Engel's biopsychosocial model is truly a model or just a case of wishful thinking, then a simple test will decide the issue. Try making, say, a prediction about a man's psychological state from his biological data or vice versa, or perhaps try to predict wholly from sociological data which girls will develop postpartum mental disorders as young women or psychoses in old age. Since nothing like this can be done, Engel's 'model' is not a model in any interesting sense of the term.

Can his approach be saved? As a model, it cannot. To be a model, it must be based in a well-formulated theory. Regrettably, one thing psychiatry does not have is well-formulated theories. Biological psychiatry has been shown to be restricted in its scope [18]. Psychoanalysis is in retreat [19,20]; behaviourism is reeling under philosophical attack [21,22] and cognitive psychology as theory falls into the trap of the infinite regress [23], meaning it can never be scientific.

The biopsychosocial approach is widely accepted, especially for teaching purposes. What everybody

wants it to do is to drag mentalist psychology and sociology into the scientific arena so these ephemera can be lassoed by the ropes of the current (positivist) scientific methodology. Throughout the history of modern Western science, this has proven impossible. Eventually, and in despair, people have tried the alternative, which was to declare human mental life non-scientific thereby expelling it from consideration. As early as 1913, the psychologist J.B. Watson declared that: 'The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation' [24, p.163]. He announced that he wanted his students to know as much about the mind-body problem as students of physics and chemistry: namely, nothing at all. MacKenzie characterised Watson's program as 'mechanistic, elementaristic, associationistic, peripheralistic, environmentalistic—and correspondingly anti-teleological, anti-purposive, anti-nativist, and anti-emergent' [21, p.17], and it failed. By excluding mental life, researchers modified the data to fit their concept of science, rather than vice versa.

Engel's attempt at a new model should be seen in its historical context, as a reaction against the woolly excesses of psychoanalysis and the sterile nihilism of behaviourism. That his theory did not work the first time is perhaps unsurprising, but he has done a very great service to orthodox psychiatry in that he legitimised the concept of talking to people as people. His 'model' promised to fill a strongly felt need: that of uniting the disparate elements of human life in such a way as to legitimise an holistic approach. Arguably, that need remains as strong today as it was 40 years ago.

Nobody would argue that reductionist science has not served us well but, as Engel noted, only in the areas in which it could work. The inescapable conclusion is that in human psychology (which, for this purpose, includes psychiatry), our concept of science has failed its subject matter, not vice versa. What we need is a new methodology, new ropes for catching these wraith-like constructs, as the old ropes are too inflexible [25]. A new scientific methodology, however, will only work in a new scientific arena: if we wish to move beyond crude biologism garnished with well-meaning humanism, we need a new concept of science [26]. This is not to suggest that we should abandon materialism just as we start to get to the really interesting bits. Materialism involves more than just matter and energy. Today, we accept that

information, its transfer and manipulation, rest firmly within a materialist ontology. Information is very much a material matter. Thus, if mind consists of the manipulation of brain-based information, then we have a materialist theory of mind (i.e. interactive substance dualism which does not breach any natural laws). This type of approach would be entirely consonant with the emerging field of cognitive sciences [27].

## Conclusion

Psychiatry is the only branch of medicine lacking a well-formulated, theoretical basis and logically derived models with true predictive power.

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