

The more resources that are locked up in institutions, the less will be available in the community. Is there any hard evidence that would predict the best service to a given community? I hope this debate will continue. May I suggest that we need more facts, more careful comparisons and more research. We can all express opinions and provide horror stories of the failures of other psychiatrists, geriatricians, social workers, etc. We shall make more progress from modest letters, such as that in the *BMJ* (16 April 1977, p 1030) than from Dr Bergman's emotional protests.

Perhaps the most important question is 'Are we trying to provide a medical (and medical institutional) solution to a problem which is primarily social and domestic?'

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*Modern Geriatrics* (August 1975) p 20, 'Granny battering'.  
*Nursing Mirror* (24 February 1977) p 65, 'Granny battering', II.  
*British Medical Journal* (4 September 1976) p 571, 'Slow euthanasia—or "She will be better off in hospital".'

#### OPERATIONS AND DEFINITIONS: A BRIEF ANALYSIS

DEAR SIR,

Hasenfus and Magaro (1976) explore the possibility that what is operationally defined as the schizophrenic 'deficit' is in some instances equal to what is operationally defined as creativity. I want to explore their use of the concept of operational definition—especially in light of the philosophical tradition which gave it currency.

The original purpose of an operational definition was to give quantitative, measurable meaning to concepts which were otherwise totally intangible. In the face of a philosophy of science which demanded experienced or at least experiencable objects, it was an obvious necessity to tie such terms as gravity, magnetic field and gene to the very objective via measurement. Since my purpose is no more historical than what I have already said, I shall not pursue operationism to its roots (very likely Berkeley).

One of the advantages of an operational definition was its neutrality regarding what is real, what exists. So long as measurements could be made, so long as they 'hung together' coherently, operationally minded scientists were happy.

In general, such scientists shied away from the question: why do your measurements cohere? An answer would either force them to postulate yet another intangible and theoretical construct or else force them to see that without postulating something underlying, none of the operationally defined concepts were related. Without some theoretical concepts how can one be sure, for example, that weight (mass) measured by a spring and measured by a trip balance are really the same?

Given this background, I want to suggest the following about Hasenfus and Magaro. The expression 'what is operationally defined' is ambiguous. It can mean the concept, in this case, schizophrenic deficit or creativity. But it can also mean the actual operation, the test performed. Strictly speaking, only if the tests are identical can the concepts be related. Only if one already has a theory with some non-operationally defined terms can one claim that tests are similar; otherwise each test is necessarily different. The two concepts then cannot be equated—even in some instances—unless one has a theory (a non-operational one) relating the two concepts. It is by equating the test with the concept (when the previously mentioned ambiguity is overlooked) that one is seduced into seeing equivalents or similarities.

Consider now Hasenfus and Magaro's statement that 'both the creative, normal person and the overinclusive schizophrenic respond by giving more responses than the normal subject'. A convenient way to symbolize these statements is:

- (A)  $x$  is more creative than  $y = \text{def. } x \text{ lists more round things than } y$   
 $w \text{ lists more round things than } z$   
 (B)  $w$  is more schizophrenic than  $z = \text{def. round things than } z$   
 (1) where  $y$  is a normal person and  $z$  may be normal  
 (2)  $x$  is also normal; or  $x$ 's responses are not inappropriate.

Riders (1) and (2) are clearly necessary. There is nothing methodologically incorrect here. What they do show is that, taken strictly, (A) and (B) cannot be related without imposing a theory that speaks to what counts as normal and appropriate. If these terms can be operationally defined, then some value judgements will have been washed out of psychology. But the general problem of operational definitions—no relations without at least postulating some underlying characteristics—will still remain.

Now, I do not think that this is all just so much philosophical verbiage. What Hasenfus and Magaro rather clearly mean by operational definition is not really definition, but rather testability, the ability

to identify through quantifiable characteristics. One could, for example, identify acids by using a pH meter. But this test would not define acidity. Indeed, it measures acidity only in virtue of a theory.

By conflating the distinction between a definition and a test used to identify (a criterion), there is a tendency to draw attention away from many variables, in this case variables such as appropriate and inappropriate responses. How these terms function is surely crucial. Moreover, by dropping the entire concept of operational definition and focusing on the variables, Hasenfus and Magaro could conclude not just that 'there are some similarities between operational definitions . . . [that] as measured in the schizophrenic "deficit" does in some instances equal creativity', but rather that there are some similarities between certain schizophrenics and certain creative people. *Their* conclusion is about definitions, but surely there is more involved.

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#### LITHIUM

DEAR SIR,

Marini (1977) as well as Brunswick *et al* (1977) criticize our study of RBC/plasma ratios (Lee *et al*, 1975). While Marini's paper contains some mistakes, it does highlight to us a defect in the way our data were presented. We do not claim a relation between the ratio and plasma levels in all patients, but only in some (7 of 12). Where the ratio remains fairly constant despite changing plasma levels, the axis was rotated for statistical purposes which have not always been understood. Our paper makes this point but fails to emphasize it.

Psychiatrists will find the debate difficult to follow, but Marini criticizes our demonstration of the value of using a quadratic equation to explain the

relation because he feels it fails at the one certain point when RBC and plasma lithium are nil. But he argues assuming that '0 ÷ 0 = 0!! By L'Hôpital's rule in such situations, 0/0 can be evaluated and is the ratio of the differential of the numerator over the differential of the denominator.

Marini (1977) quotes old papers implying a passive distribution of lithium. He suggests that the low RBC values reported are due to a lag in achieving equilibrium, but ignore the work of Greil *et al* (1974) who found that equilibrium is established within 8 h. Recently, lithium extrusion from RBCs against an electrochemical gradient has been shown to occur by means of a counter-current exchange with sodium (Haas *et al*, 1975; Duhm *et al*, 1976). This effect was not seen by Maizels (1968) who used sodium-free media.

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