The Labelling Theory of Mental Illness

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The first part of this paper is a response to several recent critiques of labelling theory. The second part assesses the state of the evidence on the labelling theory of mental illness. The majority of the studies reviewed support the theory.

This paper will present an evaluation of the labelling theory of mental illness. To this date, there have been three critiques of labelling theory, those by Gove (1970a), Gibbs (1972), and Davis (1972). Gibbs and Davis, for the most part, evaluate formal aspects of the theory; Gove evaluates its substance. Gibbs suggests that the labelling approach is not really a scientific theory, in that it is not sufficiently explicit and unambiguous. Davis proposes that there are ideological biases in the labelling approach, and points to other approaches as alternatives.1

Although the papers by Gibbs and by Davis raise important questions, neither considers at length the most fundamental question that can be asked about a theory: how well is it supported by empirical studies? Gove considers this question in his critique, and the present paper is devoted to it. In the first section of this paper, I will respond to Gove’s evaluation, and in the second, present my own.

First, however, I wish to comment on Gibbs’ paper, since it raises a methodological question relevant to assessing evidence to be presented here. In his analysis of labelling theory, Gibbs demonstrates that the concepts used in the theory are ambiguous, since they are not defined denotatively, i.e., in a way which allows for only a single meaning for each concept. He argues that this ambiguity leaves open many alternative meanings and implications. For this reason, he concludes that the theory in its present state is of little value.

I will make two observations about Gibbs’ argument. First, virtually every other sociological theory lacks denotive definition. Indeed, Gibbs observes that the concept of social norm, an important element in labelling theory, has never been denotatively defined. Since this concept is perhaps the most basic sociological idea, Gibbs’ critique is less an evaluation of labelling theory per se than the state of social science.

Note that Gibbs’ critique is equally applicable to psychiatric theories. At this writing, I know of no psychiatric theory of functional mental illness which is based on denotatively defined concepts. The four basic components of the medical model, cause, lesion, symptoms, and outcome, as applied to mental illness, are not denotatively defined (Scheff, 1966:180). Nor are such specific concepts as depression, schizophrenia, phobia, and neurosis. Gibbs’ critique of labelling theory, therefore, applies equally well to all of its competitors in the field of mental illness.

My second observation is that Gibbs’ critique implies that there is only one kind of science, a positivistic one modeled on natural science. He appears to be saying that a theory has no value unless it can be unambiguously stated. It has been argued, however, that concepts and theories can have a sensitizing function quite distinct from their literal truth value (Blumer, 1954). Theories based on nominal (connotative) definitions can direct

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1 For a considered response to the question of bias in labelling theory, see Becker (1973).
attention toward new data, or to new ways of perceiving old data, which challenge taken-for-granted assumptions, and shatter "the attitude of everyday life" (Bruyn, 1966; Schutz, 1962). In such a view, the very ambigiosity of nominal concepts is of value, since they have a rich evocativeness which denotative concepts lack (Bronowski, 1965).

Science may be viewed as a problem solving activity, with two distinct phases (Bronowski, 1956). In the first phase, the problem is to somehow transcend the traditional classifications and models which imprison thought. In the second, the problem is to test a new idea meticulously. Sensitizing theories are relevant to the first phase of scientific problem solving. They are attempts to jostle the imagination, to create a crisis of consciousness which will lead to new visions of reality. Sensitizing theories are as valuable as denotative theories; they simply attempt to solve a different problem.

The need for new research directions in the study of mental illness has long been apparent. Although thousands of studies have been based on the medical model, real progress toward scientific understanding, or even a fruitful formulation of the problem, is lacking (Scheff, 1966:7-9). The sensitizing function of the labelling theory of mental illness derives precisely from its attempt to contradict the major tenets of the medical model; it is less an attempt to displace that model than to clear the air, as I indicated in Being Mentally Ill:

It should be clear at this point that the purpose of this theory is not to reject psychiatric and psychological formulations in their totality. It is obvious that such formulations have served, and will continue to serve, useful functions in theory and practice concerning mental illness. The ... purpose, rather, is to develop a model which will complement the individual system models by providing a complete and explicit contrast ... By allowing for explicit consideration of these antithetical models, the way may be cleared for a synthesis ... (Scheff, 1966, 25-27).

It seems to me that none of the three critiques discussed here appreciate the point that a sensitizing theory may be ambiguous, ideologically biased, not literally true, and still be useful and even necessary for scientific progress.

While the labelling theory of mental illness is a sensitizing theory, it can still be used to evaluate evidence, in a provisional way. The proper question to ask is not, as Gove asks, whether labelling theory is literally true, but whether the relevant studies are more consistent with labelling theory than with its competitor, the medical model. I will now turn to this question.

In his critique, Gove reaches the following conclusion: "The available evidence ... indicates that the societal reaction formulation of how a person becomes mentally ill is substantially incorrect" (1970a: 881). My own reading of the evidence is contrary to that of Gove. First, Gove's interpretation of most studies he cites seems at least questionable and, in some cases, inaccurate. I wish first then to state my objections to several of Gove's interpretations. Secondly, since Gove's articles were published, several new studies have appeared which have bearing on the controversy. Also, several relevant articles which Gove failed to mention were published earlier than his article. Later in the paper, I will review all of these articles.

Gove concluded that the majority of the evidence failed to support labelling theory through two kinds of distortion: first, by overstating the implications of those studies he thought refuted labelling theory and, second, by misrepresenting those studies he thought supported labelling theory. I will not try to refute all of Gove's interpretations, since to do so would be to restate labelling theory. I will simply indicate some representative errors that he makes.

Apropos of Gove's overstatement, let us examine how he interprets the study by Yarrow et al. (1955). To study the processes through which the next-of-kin come to define a person as mentally ill, Yarrow et al. interviewed wives of men who had been hospitalized for mental illness. Gove summarizes that study as follows: "Only when the husband's behavior became impossible to deal with would the wife take action to have the husband hospitalized." Gove's interpretation is questionable for two reasons. First, Yarrow et al. studied only those cases of deviance which resulted in hospitalization. They did not study all cases of the same type of deviant behavior which led to hospitalization, in the entire
population. The Yarrow study thus covers only a clinical population and is entirely ex post facto. Gove's interpretation repeats the classic fallacy of the medical model, which is to assume that hospitalization was inevitable, even though no observations have been made on the incidence and outcome of similar cases in the unhospitalized population. The history of physical medicine has many analogous cases. For example, it has been found that until the late 1940's, histoplasmosis was thought to be a rare tropical disease with a uniformly fatal outcome (Schwartz and Baum, 1957). Field investigations discovered, however, that the syndrome is widely prevalent and that death or impairment is highly unusual. Analogically, it is possible that the symptoms reported by the wives in the Yarrow et al. study, even if accurately reported, might terminate without medical intervention.

The question of the accuracy of the wives' report raises the second problem in Gove's interpretation. Yarrow et al.'s descriptions of the husbands' behavior are based entirely on the wives' uncorroborated account. Yarrow et al. recognize this difficulty, warn the reader about it, and are unassuming about the implications of their findings:

Ideally to study this problem, one might like to interview the wives as they struggle with the developing illness. This is precluded, however, by the fact that the problem "is not visible" until psychiatric help is sought. The data, therefore, are the wives' reconstructions of their earlier experiences . . . . It is recognized that recollections of the prehospital period may well include systematic biases such as distortions, omissions, and increased organization and clarity (p. 60).

Although Yarrow et al. clearly recognize the limitations of their study, Gove does not. He reports the wives' account of the husbands' behavior as if it were the thing itself. Judging from Gove, Laing and Esterson's (1964) detailed study of the way in which the next-of-kin sometime falsifies his account and colludes against the pre-patient may as well have never been written. Laing and Esterson spent an average of twenty-four hours interviewing members of each of the eleven families in their study, with a range of sixteen to fifty hours per family. They found consider-

able evidence which supported the patient's story rather than the next-of-kin's. For example, in one of their cases the psychiatrist indicated that the patient Maya had "ideas of reference," which supported one of the complaints against her. By interviewing the patient, the mother and the father together, however, Laing and Esterson put this "delusion" in quite a different light:

An idea of reference that she had was that something she could not fathom was going on between her parents, seemingly about her. Indeed there was. When they were interviewed together, her mother and father kept exchanging with each other a constant series of nods, winks, gestures, and knowing smiles so obvious to the observer that he commented on them after 20 minutes of the first such interview. They continued, however, unabated and denied (Laing and Esterson, p. 24).

Laing and Esterson found many such items of misrepresentation by the next-of-kin in all their cases. Their study suggests that the uncorroborated account of the next-of-kin is riddled with error.

This is not to say that Laing and Esterson's interpretation is correct and that Gove's is not. I am saying that Yarrow et al.'s study and the other studies that Gove cites in this context were not only not organized to test labelling theory, but were innocent of any of the possible interpretations (such as that of Laing and Esterson) which labelling theory suggests. Until such time as systematic studies are conducted which investigate both clinical and non-clinical populations, and which do not rest entirely on the uncorroborated testimony of one or the other interested parties, interpretations of the kind that Gove makes are dubious.

Another example of how Gove distorts the evidence, seeking to discredit studies which support labelling theory, is his analysis of my article, "The Societal Reaction to Deviance: Ascriptive Elements in the Psychiatric Screening of Mental Patients in a Midwestern State" (Scheff, 1964). The study reported in this article consists of two phases. In the first, preliminary phase, I had hospital psychiatrists rate a sample of incoming patients according to the legal criteria for commitment, dangerousness, and degree of mental impairment. In the second phase, we observed, in a sample of
cases, the procedures actually used in committing patients, particularly the psychiatric examination and the formal commitment hearing. The purpose of the psychiatric ratings was to provide a foundation for our observations in the second phase; they were used to determine the extent to which there was any legal uncertainty about the patients' commitability. The second phase of the study described how the judges and psychiatrists reacted to uncertainty. The article stated clearly that the study was divided into two parts:

The purpose of the description that follows is to determine the extent of uncertainty that exists concerning new patients' qualifications for involuntary confinement in a mental hospital, and the reactions of the courts to this type of uncertainty (p. 402).

In the first phase of the study, the psychiatrists' ratings of the sample of incoming patients were as follows:

<table>
<thead>
<tr>
<th>How Likely Patient Would Harm Self or Others</th>
<th>Degree of Mental Impairment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Very likely</td>
<td>Severe 17%</td>
</tr>
<tr>
<td>Likely</td>
<td>Moderate 42%</td>
</tr>
<tr>
<td>Somewhat likely</td>
<td>Mild 25%</td>
</tr>
<tr>
<td>Somewhat unlikely</td>
<td>Minimal 12%</td>
</tr>
<tr>
<td>Unlikely</td>
<td>None 2%</td>
</tr>
<tr>
<td>Very Unlikely</td>
<td></td>
</tr>
</tbody>
</table>

These findings, it is argued, are relevant to the question of the legal uncertainty concerning the patients' commitability. The legal rulings on the presumption of health are stringent. The courts "have repeatedly held that there should be a presumption of sanity. The burden of proof should be on the petitioners (i.e., the next-of-kin). There must be a preponderance of evidence and the evidence should be of a clear and unexceptional nature" (Schect, 1964: 403). Given these rulings, it seems reasonable to argue, as the article did, that the commitability of all patients except those rated at the extremes of dangerousness or impairment was uncertain. The ratings, it was argued, suggested uncertainty about the commitability of 63% of the patients in the sample, i.e., those patients rated as neither dangerous nor severely impaired.

In the second phase of the study, when we observed the actual commitment procedures, we sought to find out how the psychiatric examiners and judges reacted to uncertainty. To summarize our observations, we found that all of the psychiatric examinations and judicial hearings that we witnessed were perfunctory. Furthermore, virtually every hearing resulted in a recommendation for commitment or continued hospitalization. The conclusion of the article is based not on the first phase only, but on both phases of the study. Since the first phase suggests uncertainty with respect to the commitability of some of the patients, and the second phase suggests that the commitment procedures were perfunctory for the entire sample, and yet resulted in continued hospitalization rather than release, in virtually every case, the study appears to demonstrate the presumption of illness.

Gove's treatment of this article is somewhat irresponsible. By ignoring the second phase of the study, he takes the first phase out of context. Ignoring my argument concerning uncertainty, Gove suggests that had I placed the cutting point on the psychiatrists' ratings differently, by including as committable patients rated as moderately impaired and/or somewhat likely to harm themselves, my data "would have shown instead that the vast majority of committed mental patients were mentally ill" (Gove, 1970b). He implies, therefore, that the results of the study rest entirely on my arbitrary choice of a cutting point.2 In light of all the evidence presented in the article, where the cutting point in the psychiatrists' ratings is placed has little significance. Gove disregarded the problem that the study posed, which was whether or not patients were being committed illegally. He misrepresents my conclusion by imputing to me the conclusion that most of the patients are not mentally ill. The study did not make this point, since I regard the criteria for mental illness as even more ambiguous than the legal standards for commitment.

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2Gove's criticism of the cutting point applies more to an early report of some of the initial results of the study, a brief note in the American Journal of Psychiatry (Schect, 1963). That report acknowledged that setting the cutting point on the psychiatrists' ratings was problematic (p. 268).
Gove’s other criticism of the study concerns the questionnaire given the psychiatrists to obtain ratings of dangerousness and mental impairment. He suggests that I should have provided the psychiatrists with descriptions of the behavior that the scales refer to. This criticism begs the question, however, since it seems to assume that there are precise psychiatric or legal criteria of committable behavior. In fact, the legal statutes, though they vary in language from state to state, are all vague, general, and ambiguous. They state simply that persons who are dangerous or unable to care for themselves may be committed if a strong case can be made. No statutes or psychiatric statements set forth behavioral criteria. My study sought not to help psychiatrists and judges interpret these vague laws, but to describe how they reacted to the law’s ambiguity.

Some of Gove’s criticism seems based on a misunderstanding of labelling theory. He seems to think that showing that the commitment rates reported in various studies are considerably less than 100%, somehow refutes labelling theory (Gove, 1970a: 877-9). The argument made by labelling theorists that official agents of the societal reaction usually presume illness does not imply that commitment will always occur, any more than presuming innocence in criminal courts implies that acquittal will always occur. The master question which labelling theory raises with respect to commitment rates is more complex than Gove implies. At what point and under what conditions does the process of denial stop and labelling begin? Gove apparently acknowledges that labelling occurs, but only in the last stages of the commitment funnel, i.e., in the formal commitment procedure itself. I suspect that his formulation is much too simple, and that labelling occurs under some conditions much earlier in the process, even in the family or neighborhood; and, conversely, under some conditions, denial may occur late in the process, as some of my studies showed (Scheff, 1966: 135).

The crucial question we have raised vis-a-vis the medical model concerns contingencies which lead to labelling that lie outside the patient and his behavior. Greenley, for example, established that, independent of a patient’s psychiatric condition, the family’s desire to bring him home seems to be the most powerful determinant of his length of hospitalization (Greenley, 1972). Labelling theory proposes that the patient’s condition is only one of a number of contingencies affecting the societal reaction and, therefore, the patient’s fate. Further contingencies are suggested in Being Mentally Ill (pp. 96-7). Gove’s interpretation of labelling theory is simplistic and incorrect.

SUMMARIZING THE EVIDENCE

Since most studies of “mental illness” were not designed to test labelling theory, seemingly plausible interpretations of most of them can be constructed either for or against labelling theory. Furthermore, since the conflict between labelling theory and the medical model engenders such furious partisanship, we should also exclude studies based on casual or unsystematic observations, in which the observers’ bias are more likely to influence the results he reports. I have surveyed the research literature, therefore, for studies that meet two criteria. First, they must relate to labelling theory explicitly; and, second, the research methods must be systematic. At this writing I have located eighteen studies of this type. Of these eighteen only five, those by Gove (1973, 1974), Karmel (1969, 1970) and Robins (1966), are inconsistent with labelling theory; the remainder, those of Denzin (1968), Denzin and Spitzer (1966), Greenley (1972), Haney and Michielutte (1968), Haney, Miller and Michielutte (1969), Linsky (1970a, b), Rosenhan (1973), Rushing (1971), Scheff (1964), Temerlin (1968), Wilde (1968), and Wenger and Fletcher (1969) are consistent with labelling theory.

These eighteen studies vary widely in the reliability of the inferences that we can make from them. Four studies among those consistent with labelling theory use zero-order correlations—those of Denzin and Spitzer; Denzin; Haney and Michielutte, and Haney, Miller and Michielutte. For example, Haney reports the correlation between the decision to commit and social characteristics of the patients and petitioners. He finds positive correlations between commitment rates and these social characteristics. For example, he reports a higher rate of commitment for non-whites than whites. Although his findings are consistent with labelling theory, they provide only very weak support since he has not controlled for the patient’s condition. We are left with the
question that occurs so often in social epidemiology: Are non-whites committed more often because of the societal reaction to their social status, or because this particular social status is itself correlated with mental illness? That is to say, are non-whites committed more often than whites because of their powerlessness, or because there is more mental illness among them? Haney's studies do not answer such questions, nor do those of Denzin and Spitzer, Denzin.

Similar criticism can be made of the two studies by Karmel which fail to support labelling theory. Based on interviews with patients after their hospitalization, her data fail to show any evidence of the acceptance of a deviant role predicted by labelling theory. These are simple correlation studies with no controls (Bohr, 1970). Gove (1973) studied the amount and effects of stigma on a sample of ex-mental patients. His data indicate that the amount and effects of stigma were not very large, and therefore fail to support labelling theory. His data are somewhat ambiguous, however, since there is no control group of similar persons who were not hospitalized.

A series of much stronger studies, whose findings support labelling theory, are those of Greenley, Rushing, Linsky, Scheff (1964), Wenger and Fletcher and Wilde. My study has already been discussed. Greenley, as indicated above, studied the relationship between length of hospitalization and several social and psychiatric variables. He found that even when the patient's psychiatric condition is controlled, there is a strong relationship between the family desire for the patient's release and the length of hospitalization.

Rushing and Linsky each did studies on the relationship between psychiatric commitment and social class and other social characteristics. Since they indicated that their data only partly overlap, I will cite both studies (Linsky, 1972; Rushing, 1972). Both used the same technique, which I believe controls for the patient's condition. If they had merely used commitment rates as their dependent variable, we would be left with the perplexing question: are commitment rates higher in the lowest social class because there is more mental illness in that class or for other reasons? (See the New Haven studies by Hollingshead and Redlich [1958].) However, both Rushing and Linsky used an index made up of the ratio of involuntary to voluntary hospital admissions, as a measure of societal reaction. I believe that such a ratio will control for gross variations in rates of mental illness. What the index provides, hopefully, is a measure of the most severe societal reaction, i.e., involuntary confinement, but with the phenomenon of mental illness at least partly controlled, assuming that the voluntary commitments are equally "mentally ill." Perhaps this assumption should also be investigated. Both studies show a strong relationship between powerlessness and commitment rates. In the study by Wenger and Fletcher, the presence of a lawyer representing the patient in admission hearings decreased the likelihood of hospitalization. This relationship held within three degrees of manifest "mental illness."

Finally, Wilde's study (1968) concerns the relationship between the recommendations for commitment made by mental health examiners and various social characteristics of the pre-patients, with controls for the patient's psychiatric condition. In all five of these studies strong relationships are reported between such social characteristics as class, and commitment rates, with psychiatric conditions controlled for. These five studies support labelling theory since they indicate that social characteristics of the patients help determine the severity of the societal reaction, independent of psychiatric condition.

The controlled studies by Robins (1966) and by Gove (1974) provide data which fail to support labelling theory. Robins used psychiatric diagnoses of adults who had been diagnosed as children as part of an evaluation of child guidance clinics. Robins noted that some of the children diagnosed were treated and some were not. She argues that this data can be used to evaluate the effects of "the severity of societal response to the behavior problems of the children." She found that, of the adults who had psychiatric treatment as children, 16% were diagnosed as having sociopathic personalities as adults. Of the persons who did not receive psychiatric treatment as children, 24% were diagnosed as having sociopathic personalities as adults. Since the difference between the two percentages is not statistically significant, the hypothesis that psychiatric treatment was beneficial is not supported, but by the same token, neither is the labelling hypothesis that psychiatric treatment, particularly when involuntary, may
stabilize behavior that would otherwise be transient. This finding is somewhat equivocal, however, because of the sampling problems of the original Cambridge-Somerville study.

With a sample of hospitalized mental patients, Gove (1974) has studied the relationship between the patient’s psychiatric record and his economic and social resources. His data suggest that individual resources facilitate treatment, rather than allow the individual to avoid the societal reaction, and therefore support the medical model rather than labelling theory. Some caution is necessary in interpreting these findings, however, since patient characteristics were based on hospital data. For example, he finds that more of the records of patients with low resources present the patient as “never psychiatrically normal,” than patients with higher resources. Does this mean that low resource patients have been “mentally ill” longer, or that the hospital tends to construct their case histories in this way, retroactively (Goffman, 1961, p. 145)? In any case, Gove’s interpretation of his data contradicts the conclusions of Linsky and of Rushing. Since the studies do not use the same indices, it is not possible to compare them directly.

The final two studies to be discussed provide still stronger support for labelling theory. The first, Temerlin’s (1968), is a test of the influence of suggestion on psychiatric diagnosis. Temerlin finds that psychiatrists and clinical psychologists are extremely suggestible when it comes to diagnosing mental illness. Four different groups diagnosed the patient in the same recorded interview under different conditions. One control group diagnosed with no prior suggestion, one group was given a suggestion that the interviewee was sane, and a third group was told that they were selecting scientists to work in research. In the experimental group, it was suggested that the interviewees were mentally ill. The diagnoses of the control and experimental groups differed greatly. In the control groups the great majority made diagnoses of mental health; whereas in the experimental group, not a single psychiatrist out of twenty-five, and only three out of twenty-five psychologists, diagnosed mental health. One weakness of this study is that it takes place in an artificial setting, with an enacted interview; but it strongly supports the unreliability of psychiatric diagnosis and the presumption of illness.

The study by Rosehan (1973) took place in real settings—twelve mental hospitals. For this study, eight sane persons gained secret admittance to the different hospitals. They all followed the same plan. In his initial admission interview, each pseudo-patient simulated several psychotic symptoms. Immediately upon admission to the ward, the pseudo-patients stopped simulating any symptoms of abnormality. In all twelve cases the pseudo-patients had enormous difficulty establishing that they were sane. The length of hospitalization ranged from seven to fifty-two days with an average of nineteen days. The study’s major finding is as follows:

Despite their public show of sanity, the pseudo-patients were never detected. Admitted except in one case with a diagnosis of schizophrenia, each was discharged with a diagnosis of schizophrenia in remission. The label “in remission” should in no way be dismissed as a formality for at no time during any hospitalization had any question been raised about any pseudo-patient’s simulation . . . the evidence is strong that once labelled schizophrenic the pseudo-patient was stuck with the label (p. 252).

Rosehan also collected a wide variety of subsidiary data dealing with the amount and quality of contact between the pseudo-patients and the hospital staff, showing a strong tendency for the staff to treat the pseudo-patients as non-persons.

This study, like Temerlin’s, strongly supports labelling theory. Both provide good models for future studies of labelling theory, the Rosehan study with its use of actual hospital locations, and the Temerlin study with its experimental design.

We can now provisionally summarize the state of evidence concerning labelling theory. If we restrict ourselves to systematic studies explicitly related to labelling theory, eighteen are available. Of these, thirteen support labelling theory, and five fail to. Although the studies vary in reliability and precision, the balance of evidence seems to support labelling theory.

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