Understanding Suicide: Why We Don’t and How We Might
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Introduction
Introduction

Several years ago, Lester (2000) suggested that suicidology had come to an end. The thesis of that article was that very little of consequence had appeared in the last ten years that furthered our knowledge of suicide – no new theory and no ground-breaking research.

The end of the 20th Century produced an interesting trend in the scholarly world – a sense that everything was coming to an end. Francis Fukuyama (1992) declared that history had come to an end and that little of importance would happen in the future. David Lindley (1994) followed with a declaration that physics had discovered all that there was to be discovered, and John Horgan (1997a) declared that science in general had solved all of the puzzles. It seems clear that Fukuyama was wrong in asserting that nothing of importance would occur in world history in the future – the rise in global terrorism or climate change cannot be described as “of little importance” – but let us look at Horgan’s views in more detail.

Horgan (1997a) argued that pure science had entered its twilight, that is, there are no more profound revelations that will occur. He noted that science as a whole was not ending but that *path-breaking* science was ending. He argued that scientists will continue to keep busy and make tiny advances, but that they will never achieve their most ambitious goals. He noted that scientists themselves, as well as the media, hype each new research finding, disregarding whether it is spurious or genuine, and this gives us the illusion that scientific research is making great progress. More recently, Horgan (1997b) has suggested that even applied science might be ending. After all, he noted, where are the nuclear fusion reactors that were predicted in the 1950s? Has the war on cancer declared by President Richard Nixon in 1971 been won yet?

The same seems to be true in the discipline of psychology, particularly with regard to theories of personality and systems of psychotherapy. The last major theory of personality was George Kelly’s published in 1955, and the last major theories of psychotherapy were introduced by Albert Ellis and Fritz Perls in the 1960s and Arthur Janov in the 1970s. Since then, a degree in marketing would be more appropriate than a degree in psychology, for the so-called “new” theories proposed have been little more than re-packaging of the older theories, combined in a new mix and with new terms for old concepts – a “new improved product” that consumers should regard warily.

What of suicidology? Every ten years for the last thirty years Lester (1972, 1983, 1992, 2000) reviewed the suicidology literature as completely and extensively as he was able. The reviews cover the period 1897–1997. In the latest edition, Lester (2000) concluded that nothing of importance has been published in the 1990s! For each recent decade, Lester selected the major contributors to the field of suicidology: Edwin
Shneidman and Norman Farberow in the 1960s and Aaron Beck and David Phillips in the 1970s, for example. These people introduced new topics and research foci for suicidology and produced a large body of work on those topics. There was no one, in Lester’s opinion, worthy to be chosen for the 1990s.

The topics for research in the 1990s were those which had been identified in earlier decades. Indeed, it was depressing to see the same kinds of research being conducted as has been published many, many times before. A large sample of subjects is administered depression, hopelessness, self-esteem and life stress scales to predict suicidal behavior using a multiple regression model. A sample of suicidal subjects is examined for its psychiatric characteristics. (How many more times must we read about comorbidity as an indicator for suicidality?) A set of social indicators is entered into (again) a multiple regression analysis to predict suicide rates over time or over regions. A few brains are examined for a dozen or so biochemicals in up to 30 or so brain areas, with inconsistent findings. In this last case, it is obvious that the reports on suicide are often side issues – the goal of the study was to explore biochemical causes of depression or schizophrenia but the investigators saw that they could produce an additional paper or two by studying the suicides in the sample.

In addition, some topics in suicidology have been removed from our purview, topics such as sex differences in suicidal behavior or the variation of suicidality over the menstrual cycle. It is not that we solved the problems or adequately tested theories of these phenomena. It seems rather that we simply lost interest. For example, with regard to sex differences in suicidal behavior, despite the many papers and books published on the topic, there have been almost no empirical tests of relevant theories in the last twenty years. Rather we read simple discussions of the topic. This has been particularly true recently of the female suicide rate in China which appears to be higher than the male suicide rate. Dozens of papers have noted this difference (almost all using the same WHO data), and most have speculated on the causes. However, no paper has attempted to test a theory for this difference!

In the 1990s, a number of suicidologists argued that we needed a meaningful and generally agreed-upon terminology for suicidology, and several papers appeared on this topic (e.g., O’Carroll et al., 1996). A meaningful terminology that was generally agreed upon would be useful (although getting general agreement might be difficult) and there continues to be work in this area (e.g., Silverman, Berman, Sanddal, O’Carroll, & Joiner, 2007) but what we really need are good theories and, in particular, good competing and incompatible theories, combined with methodologically sound empirical tests of hypotheses generated from these competing theories.

Let us give two examples. Henry and Short’s (1954) theory of suicide and homicide, regardless of how well it predicts and explains suicidal phenomena, is a complex theory. It has many assumptions, proposes many hypotheses, and cleverly interweaves psychological and sociological concepts. In contrast, psychological models of suicidal behavior imply (by using multiple regression techniques) that suicidal behavior is simply a weighted sum of various psychological states and traits – a little bit of hopelessness plus a little bit of stress, etc. There are rarely any interaction terms, and certainly no tests of theories of the form \( S = \frac{(A + B^2)}{(\sqrt{C} - \log D)} \), where \( S, A, B, C \) and \( D \) are psychological or behavioral variables, theoretical forms that are common in the natural sciences.
Consider Durkheim’s (1897) classic sociological theory of suicide. There are four types of suicide based on two dimensions (social integration and social regulation). Two dimensions provide a zonal analysis with four types such as high social integration plus low social regulation, etc. These four types do not match Durkheim’s four types. Rootman (1973) noted this and reported a preliminary study, but his study has been largely ignored. Instead, sociologists simply examine correlations of variables such as divorce and birth rates with suicide rates. They never empirically demonstrate that variables such as divorce or birth rates are measures of social integration or social regulation. Are divorced people less socially integrated or socially regulated than married people? An empirical justification of this assumption is never presented in a sociological paper on suicide. Even the dominance of Durkheim’s theory has been criticized by Gibbs (1994) who wrote on “Durkheim’s heavy hand in the sociological study of suicide.”

So is this the end of suicidology? Perhaps, but we think not. In order that it may not be the end, in this book we critique respectfully the field of suicidology while making constructive suggestions for what needs to be done in the future. We hope that these suggestions will provide the groundwork for a new beginning for suicidology. Our aim in this book is not review the whole field. There are books that provide a broad and thorough introduction to suicidology such as the Comprehensive Textbook of Suicidology by Ronald Maris, Alan Berman and Morton Silverman (2000) and summaries of research and theory up to 1997 by Lester (2000).

In Chapter 2 we will first review the general methodological issues that are major problems for research into suicidal behavior. Some of these issues are present in all research, but others are specific to research into suicidal behavior.

In the next four chapters (Chapters 3 through 6), we critique the four major disciplines that contribute to our understanding of suicide. To be sure, we acknowledge that there are disciplines (major and minor) that also contribute (criminal justice, economics, political science and public health, to name just a few), but we feel that our criticisms and recommendations for the disciplines we have selected also apply to the others.

In the next section, we critique and make recommendations for a number of research topics in the field of suicidology. In this section, again, we are not attempting to review the whole field, but rather we have chosen representative examples of different areas of research. For example, the critique and recommendations in Chapter 8 on studies of sexual abuse and suicidality are relevant to many topics of study, including substance abuse and suicidality, eating disorders and suicidality, borderline personality disorder and suicidality, and so on. The chapter on sex differences has been chosen to illustrate the issue of “fads” in suicidology, topics that arouse great interest for a period of time and are then abandoned without any of the major questions fully answered. However, some of the chapters in this section do address seminal issues in the field, assessing suicidal risk and the formulation of typologies of suicidal individuals.

Finally, we draw together our thoughts after this endeavor and we present a summary of our recommendations for the field of suicidology. We hope that you will be stimulated and even provoked by our thoughts and recommendations, and that you will be motivated to prove that we have not reached “the end of suicidology.”
References


General Methodological Issues

The goal of the present book, as we have discussed in the Introduction, is to critique the field of suicidology and make constructive suggestions for future research and theory. In Part 1, the contributions of several disciplines to our understanding of suicide (such as psychology and sociology) are reviewed and proposals made for future studies in those fields. In Part 2, several topics in suicidology (such as twin studies and studies of the association between sexual abuse and subsequent suicidality) are reviewed and, again, proposals made for future studies on those topics.

There are however, some general problems and issues in the field of suicidology, and the purpose in the present chapter is to review and discuss these general issues. This chapter is organized around the following areas: the accuracy of suicidal classifications, the method of substitute subjects, subject bias, control groups, sample size, the validity of measures, ecological and time series research, statistical versus clinical significance, and problems with literature searches.

Accuracy of Suicidal Classifications

Completed Suicide

The problems of making reliable and valid psychiatric diagnoses of psychiatric patients are large and have received a great deal of discussion and debate. Indeed, one of the major reasons that the American Psychiatric Association is now preparing the 6th revision of their Diagnostic and Statistical Manual is to improve the reliability and validity of psychiatric diagnoses.

One would think, at first glance, that the field of suicidology would not have this problem. Surely a death from suicide or an attempt to kill oneself is easily certified. Certainly, the classification of suicidal acts is less problematic than the classification of psychiatric disorders. However, a closer examination of the classification issue in suicidology suggests that, even here, problems exist.

First, for completed suicide, many central statistical offices rely on the certification of death made by coroners and medical examiners. These individuals have a variety of backgrounds, including law and medicine. In some regions of the world, juries of lay men and woman, under the guidance of a judge, make this decision.

In rare cases, the decision is not simple. Some men hang themselves for auto-erotic sexual activity. Hanging produces an erection and so can accompany masturbation.
Occasionally, the man fails to remove his head from the noose and dies (Shankel & Carr, 1956) making a determination of suicide likely in the absence of disconfirming evidence. In other cases, it is difficult to distinguish between suicide and murder. There are several famous “suicides” for whom murder rather than suicide is suspected, including the film star Marilyn Monroe, the United Nations diplomat Povl Bang-Jensen, and the President of Chile, Salvador Allende (Lester, 1996a).

Douglas (1967) attacked the validity of official suicide rates based on the proposition that the social characteristics of the society affected the decisions of coroners and medical examiners. For example, if the religion of a society condemned suicide harshly, then more suicides would be “covered up” and classified as from another cause as compared to a society which did not condemn suicide as harshly. Douglas asserted that this phenomenon could account for differences in suicide rates by religion, sex, age, and other variables. His argument became well-known and widely accepted, despite the fact that he, himself, produced no supporting empirical evidence.

When coroners and medical examiners are uncertain about the cause of death, they can label the death as “undetermined” or, in England, as an “open verdict.” Barracough (1970, 1974) found that a change in coroner affected the proportion of open verdicts in English jurisdictions and could account for regional differences in suicide rates (such as England versus Scotland). Several studies have presented cases to coroners from different nations and found differences in how they would classify the deaths (Atkinson, Kessel & Dalgaard, 1975; Brooke, 1974). A number of researchers have investigated this phenomenon in order to identify factors that may account for these differences.

For example, Farberow, MacKinnon and Nelson (1977) studied 202 counties in western states in the United States and found that suicide rates were higher if the coroner was a pathologist and lower if the coroner was a lawyer. Similarly, Ajiki et al. (1991) found that the suicide rate in a Japanese prefecture was dependent on whether a medical examiner or medical practitioner certified the deaths. Jarvis et al. (1991) found that the certification of deaths in a sample of Canadian medical examiners was affected by their religion, the size of the town in which they worked and their experience. Thus, there are a number of professional, social and individual differences that may impact the official classification of deaths related to suicide.

Typically, when researchers themselves study all of the deaths in a community, as opposed to relying on “official” data, they find that the actual suicide rate is grossly underestimated. For example, Clarke-Finnegan and Fahy (1983) found a suicide rate in Galway County of 13.8 per 100,000 per year as compared to the official rate of 5.8.

Suicide rates are also different when calculated from police records rather than the records of medical examiners, with rates based on police records generally lower (William et al., 1987). The suicide rate is also typically higher if more post-mortems are ordered in a region (Asencio et al., 1988). In addition, the characteristics of the decedent also affect the certification of death. For example, in England, Salib (1996) found that an open verdict was more likely than a judgment of suicide if there were problems with alcohol, use of an overdose for the method of suicide, no evidence of intent, and no prior psychiatric history.
On the other hand, Jennings and Barraclough (1980) found no effect on the number of suicide verdicts versus open or accidental verdicts after changes in the law about certification of death in England. While these results may suggest greater consistency in certification despite changes in law and legal procedures, alternatively, they could suggest that changes in the law and legal procedures may not be able to overcome the impact of professional, social, and individual differences as they relate to these three classifications or verdicts.

Recommendation 2.1: It is important for sound epidemiological research and sociological studies of suicide that rates of completed suicide are accurate. Public health agencies should work with coroners and medical examiners to ensure that death records supplied to central government agencies are free from bias introduced by differing professional, legal, religious and economic perspectives. For example, coroners in Pennsylvania have for many years prepared two death certificates, one for the government and a special one for funeral directors to ensure that insurance companies do not withhold payment of the death benefit.

Calculating Suicide Rates

(1) The Population Base
Almost all nations of the world calculate their suicide rates based on “per 100,000 of the population per year.” However, the number of suicides by children and young adolescents has been very small. Indeed, in some jurisdictions, deaths of people under a certain age are rarely classified as suicides, presumably because we cannot be sure that children have a mature (and accurate) conception of what death is. Therefore, including children and young adolescents in the population base makes little sense and results in a downwardly biased estimate of the suicide rate. Interestingly, Israel is the only nation that uses the population aged 15 and over as the base for suicide rates.

Recommendation 2.2: Since suicide in very young people is rarely, if ever, classified as such, rates should be calculated on a population base that does not include young children so as to provide a more accurate statistical picture of suicide. It would be appropriate if an organization like the World Health Organization convened a meeting to suggest an appropriate age range to use for this population base.

(2) Age-Adjusted Suicide Rates
Occasional sociological studies of aggregate suicide rates use age-adjusted suicide rates which control for differences between regions (or years) in the age distribution in the population. Using age-adjusted suicide rates might change the results of research. For example, Lester (1992a) found that the divorce rate predicted the crude suicide rate in the United States over time, but not the age-adjusted suicide rate. Age-adjusted suicide rates make better sense for research since they control for differences in age in the populations. However, suicide rates could be standardized for other variables also, such as the sex distribution, religious affiliations, etc. Broadening the somewhat exclu-
sive focus on age-adjusted suicide rates to include other important variables may lead to a more comprehensive understanding of the relation between suicide and other sociological factors.

**Recommendation 2.3:** More use should be made of age-adjusted suicide rates in research, as well as suicide rates that are adjusted for other demographic variables such as sex and religious affiliation when those characteristics are relevant to the research question.

(3) **Opportunity-Based Suicide Rates**

Suicide rates could be based on the availability of means for suicide. For example, suicide rates using car exhaust could be calculated as suicides per 100,000 cars per year, suicides by firearms as suicides per 100,000 guns per year, etcetera (Lester, 1992b). Analyses based on method opportunity or accessibility might provide an alternative perspective on suicide as a function of method that may have important implications for social policy in general and suicide prevention in particular. This idea is common in criminology where, for example, bank robbery rates can be calculated as robberies per 100,000 of the population per year or as robberies per 1,000 banks, per year.

(4) **Other Measures**

Thorson (1993) suggested that studying the percentages of deaths due to suicide might be of interest. In support of this suggestion, Lester (1994) found different ecological correlates across the states of America using this measure as compared to those using the suicide rate.

**Recommendation 2.4:** Rather than being tied to the typical ‘rate per 100,000 per year’ measure, researchers should explore the effect of using suicide rates calculated in logical, alternative ways on the results of their studies. Inclusion of alternative approaches may lead to a more fine-grained understanding of suicide and impact the refinement and development of theories of suicide.

**The Time of Death**

Rich et al. (1985) compared the date of the suicidal act and the date of death on the death certificate and found them to be the same in only 46% of the suicides. In addition to representing a discrepancy of great importance for studies of the distribution of suicides over the days of the week, these results bring into question the accuracy of documentation related to other information collected through official agencies.

**The Source of the Information**

Fishman and Weimann (1997) found that the classification of motives for suicides in Israel were quite different if official records were used rather than press reports. For example, 16.1% of suicides reported in the press involved romantic or love motives versus only 1.2% from official records. Mental problems were more common in the official re-
cords (58.9%) than in press reports (27.6%). When differences such as these are seen in empirical data, it is difficult to determine objectively which source leads to the more appropriate classification.

**Missing Nations**

The sample of nations reporting suicide rates to the World Health Organization is quite small. For 1980, Lester (1996b) was able to obtain a sample of only 64 nations with consistent data over a period of years. In fact, out of the current 192 member nations in the United Nations, the WHO report for December 2005 lists rates for only 95 nations (www.who.int/mental_health/prevention/suicide_rates/en/). Most African nations and many South American nations do not collect and report suicide rates for the whole nation.

Furthermore, given that culture plays a large role in affecting suicide rates, it is surprising that nations do not report suicide rates by ethnicity. For example, Belgium has both Fleming and Walloon cultural groups, but does not report their individual suicide rates. Similarly, the suicide rate of the Basques in France and Spain is unknown.

In Africa, the situation is worse. Most of the nations created by the colonial powers have several ethnic groups combined. When the suicide rate of an African nation is reported, for example, Zimbabwe (Lester & Wilson, 1988), no effort is made to distinguish between the ethnic groups (in the case of Zimbabwe, the major groups are the Ndebele and the Shona).

**Recommendation 2.5:** Efforts must be made, perhaps by the WHO, to get more nations to report suicide rates and to encourage nations to collect data by ethnicity.

**Attempted Suicide**

Diekstra (1997) commented on the apparent confusion over terminology and definitions in suicidology, especially with regard to nonfatal suicidal behavior. For those engaging in non-lethal suicidal actions, a variety of terms have been used – attempted suicide, parasuicide, deliberate self-harm or self-injury, intentional self-injury and nonfatal or nonlethal self-destructive behavior – and the milder forms of these actions begin to resemble self-mutilation. In fact, Silverman (2006) listed 36 different terms used for suicidal attempts! The problem with the term “attempted suicide” is that it implies that the individuals were trying to kill themselves. Since many who have attempted suicide deny suicidal intent, this term seems to be inappropriate.¹

The choice of term may also affect responses in surveys. Respondents may be asked whether they have attempted suicide, inflicted harm on themselves or particular

¹ Diekstra noted that, in addition to research issues involved in this labeling, there may be effects on the attempted suicides themselves and on the members of their social networks which may be stigmatic and influence the prognosis for the individuals. For example, if the term attempted suicide is use in a vignette of a patient, all other features being the same, lay people tend to perceive more psychopathology in the patient.
methods may be specified (e.g., Have you ever tried to hang yourself, etc.). While each of these terms may be interpreted by the researchers as indicating a “suicide attempt,” respondents may not be assigning the same meaning to the terms. Schwarz (1999) has provided empirical support for this concern using more general data highlighting the impact of survey wording on responses. His analysis demonstrated how alternative ways of asking the same question can drastically impact the results.\(^2\) Similarly, Safer (1997) found that anonymous surveys resulted in a higher incidence of suicidality in the respondents than identifiable surveys. Clearly, prevalence rates may depend critically upon the exact wording used and the perception of anonymity of the respondents.

The critical issues in classifying behaviors as attempted suicide concern motivation and the lethality of the injury. In terms of motivation, if an individual admits to having an intent to die and if the injury is sufficiently severe, then the term “attempted suicide” may make good sense. However, if neither of these conditions is met, then self-injury as a classification seems more appropriate. A complication with regard to motivational determination, of course, is that the reported intent following the behavior may be different than the intent preceding the behavior (Andriessen, 2006). There may be a variety of reasons why an individual would reinterpret his or her motivations following the behavior, including the potential for secondary gain (Shea, 1999). For example, true intent prior to the behavior could be reported as no intent after the act in order to avoid hospitalization, protect loved ones from emotional distress, or as a function of self-denial. Similarly, no intent to die prior to the behavior could be reported as suicidal intent afterwards in order to help gain access to health care resources or enlist support from significant others.

Another consideration in this regard has to be the length of time between the behavior and the assessment of intent. Clearly, an empirical question that needs to be addressed is whether self-report of intent changes as a function of the length of time between the act and the self-report. It seems reasonable to consider that post-event cognitive processing may influence the self-report of intent to die. Thus, there may be a variety of individual and contextual-based reasons that a report to die after the act is reinterpreted by the individual. The potential discrepancy between pre-act and post-act reports of behavioral motivation and possible reinterpretations as a function of time with regard to intent to die could substantially impact the accuracy of classification.

The lethality of the behavior is another critical issue. Engaging in a highly lethal behavior may lead to a clearer classification of a suicide attempt. However, at lower levels of lethality, the distinction between an attempt, a “gesture” and self-mutilation becomes more difficult to make. An added complication in classifying suicide attempts as a function of lethality is that the medical-based judgment of the level of lethality (i.e., the medical likelihood that the behavior will result in death) may be very different from the subjective understanding by the suicidal individual. For example, a person may mistakenly believe that taking a certain combination of medications will

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\(^2\) For example, when asked if they are in favor of the death penalty for murder, the majority of American respondents say yes; but if asked to choose between the death penalty and life imprisonment without parole, the majority choose the life sentence (Lester, 1998).